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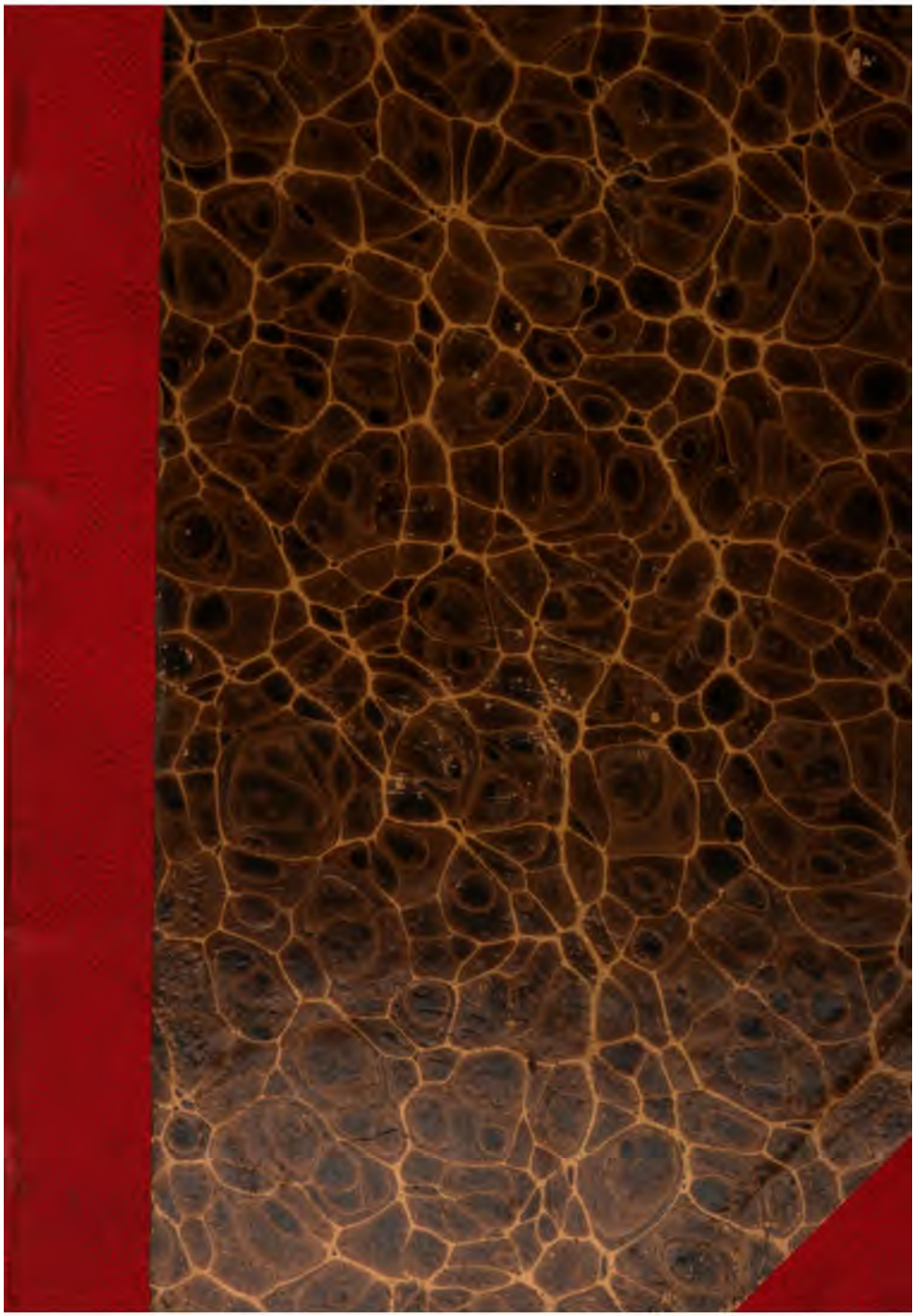

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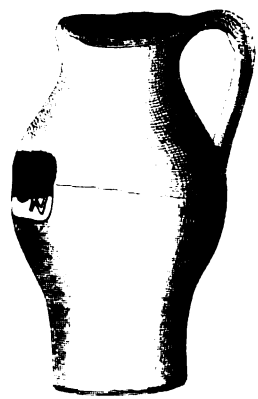

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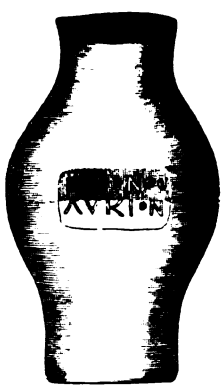

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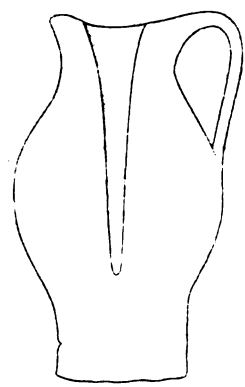

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MEDICAL SCIENCE.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Pathology of the Bronchio-Pulmonary Mucous Membrane.* By C. BLACK, M.D., Chesterfield, Bachelor of Medicine, and formerly Medical Scholar in Physiology and Comparative Anatomy in the University of London; Fellow of the Royal College of Surgeons of England, etc. etc.

THE extent of the bronchio-pulmonary mucous membrane, the relation which it bears to the important function of respiration, and the pathological condition to which, like all living structures, it is subject, indicate the importance which attaches to a correct knowledge of the mode in which morbid action influences its structure and particular function. Until the microscope became one of the principal aids in demonstrating the minute structure of organs, and in tracing the very minutiæ of disease as affecting organic atoms, pathologists had perhaps arrived, as nearly as it was possible, at the then highest obtainable knowledge of the intimate nature of disease affecting, in its various forms, the bronchio-pulmonary mucous membrane—of the modification of function arising therefrom—of the bearing of these conditions on the maintenance of life—and of the curative indications to be observed. But since that instrument has been added to the means of pathological research, the knowledge of the past generation of physicians seems to comprise but a fractional part of pathological science, which now developes an extensive field for investigation, in which many valuable and important discoveries

are yet to be made. But, whilst to the microscope we owe our improved method of investigating the physical alterations of structure effected by disease, we are no less indebted to organic chemistry, for enabling us to detect those metamorphoses of living atoms, and the various organic and inorganic products arising therefrom, which characterise the various deflections from health in the different tissues and organs of the body. Of both these means of investigation, then, I shall avail myself in my endeavour to advance our knowledge of the pathology of the bronchio-pulmonary mucous membrane. But a correct knowledge of diseased structure and of diseased action predicates a perfect acquaintance with healthy structure and with healthy action. Let me, then, linger for a short time on the threshold of this investigation, to take a cursory view of the intimate structure and arrangement of the membrane under consideration, and of the different purposes which it serves in the healthy condition of the organism.

Structure of the Bronchio-Pulmonary Membrane.

The bronchio-pulmonary mucous membrane, like every other mucous structure, consists of:—

1. An epithelium, which covers its free surface.
2. A basement, primary, or germinal structure, on which the epithelium rests.
3. A layer of simple, fibrous tissue, vessels, and nerves, subjacent to the basement membrane.

The Epithelium.—This structure consists of flattened cells, forming scales, which unite by their edges, and thus constitute a continuous layer, which extends throughout the bronchi and the ultimate cells of the lungs. These epithelial cells are sometimes oval, more frequently polygonal, nucleated (the nucleus occupying as nearly as possible the centre of the cell), and have their free surface set with delicate cilia. In contradiction to the statement, that the epithelium is not continued into the ultimate cells of the lungs, it may be observed, that if a very thin slice of pulmonary tissue be taken from the surface of the lung, macerated for a short time in distilled water, to decolourise it, and be afterwards subjected, between two slips of glass, to the microscope, each pulmonary cell is seen to have a perfect lining of epithelium, as shown in the adjoining figure.

Fig. 1.

Fig. 1.—Section from the surface of the lung, showing the epithelial lining of the pulmonary cells.

If the same section be now exposed for a few minutes to the action of a weak solution of potash, the continuous layer of epithelium is, in a great measure, broken up into patches, which may be easily demonstrated either by inclining the instrument, and thus collecting them at the lower edges of the slides, or by separately subjecting a drop or two of the fluid to the microscope. They measure from $\frac{1}{700}$th to $\frac{1}{2500}$th of an inch, and are mingled with granules from $\frac{1}{7000}$th to $\frac{1}{10.000}$th of an inch, around which a faint, light cell-wall is still visible, thus showing them to be the nuclei of epithelial cells.

Fig. 2.

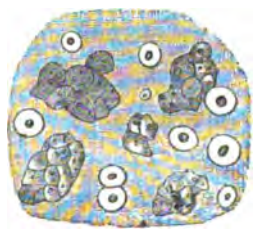

Fig. 2.—Epithelium broken up into patches and isolated cells, by the action of a solution of potash.

Not unfrequently the action of liq. potassæ detaches portions of the basement membrane itself, some of which have epithelium attached to their free surface; whilst others are destitute of epithelium, the solution of potash having effected a separation. Those portions of basement membrane which are free from epithelium have a slight grayish appearance, resemble a thin layer of tremulous jelly, and present no structural appearance beyond darkish-coloured points, set at regular distances from each other, which form the germinal centres of the epithelial cells.

Fig. 3.

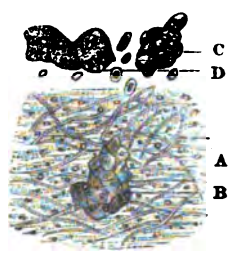

Fig. 3.—C, Epithelial patch detached by liquor potassæ.
D, Fragment of basement membrane, showing two nuclei in process of development.
A, Fig. 1 after the action of liquor potassæ, showing the basement membrane, except at one point,
B, Where the epithelium is still attached. The areolar fibres of the mucous membrane are also seen through the basement structure.

These cells serve two purposes in the economy; one of which has

for its object the protection of the basement membrane, by the continuous layer which they form; and the other, the secretion or elimination of a viscid fluid, termed mucus, which lubricates the free surface of the membrane, and thus protects it from irritation. Such, then, are the uses of the epithelium in the healthy condition and action of the organism. We shall, however, hereafter see, that under certain morbid conditions of the system affecting the bronchio-pulmonary membrane, the epithelial cells act the part of true excretory organs, and thus eliminate from the blood the elements of disease, in the same manner as the renal epithelium is believed to eliminate the scarlatenic poison in acute desquamative nephritis.[1] But there is this difference between the eliminative action of the two epithelia—the morbid products of one (the bronchio-pulmonary epithelium) are physical, tangible, and chemically determinable; whilst those of the other (renal epithelium) are, in the particular instance cited above, assumed, and as yet elude our most delicate means of research.

The Primary or Basement Membrane.—Beneath the bronchio-pulmonary epithelium the basement membrane is spread out, as before stated, in a thin, delicate, structureless layer, which is continued into the ultimate air-cells, constituting a part of their walls. Its thickness, in the larger bronchi, measures from $\frac{1}{18.000}$th to $\frac{1}{20.000}$th of an inch; whilst, in the pulmonary cells, its sectional diameter is about $\frac{1}{23.000}$th of an inch. It is studded, at almost equal distances, with small projecting points, which measure on an average $\frac{1}{8000}$th of an inch, and which are evidently the germinal nuclei of the epithelial cells, as is proved by the fact, that in patches of the basement membrane liberated by a solution of potash, and from which the epithelium has been dissolved, these points or nuclei are occasionally observed to be undergoing a process of development into cells, some of which have already obtained a growth, varying from $\frac{1}{5000}$th to $\frac{1}{6200}$th of an inch.

Fig. 4.

Fig. 4.—Basement membrane, showing several of its nuclei in the process of development into cells.

It would seem that the composition of the basement membrane is similar to that of the blood-plasma, after coagulation of the latter;

[1] *Vide* Dr G. Johnson's admirable work on "The Diseases of the Kidney"—a work which, in conjunction with the excellent papers of Dr William Gairdner of Edinburgh, of Professor Gluge of Brussels, and of Messrs Simon and Toynbee of London, on the same subjects, has given a new era to renal pathology.

that its existence is very temporary, and that it is being continually disintegrated at its epithelial surface in the maintenance of cell-growth; whilst its opposite surface is as continually receiving increments of addition from the nutritive plasma, which is incessantly supplied by the vital operations of the capillaries which ramify in the areolar tissue beneath. Hence the difference between healthy nutrition and that morbid process which is designated "inflammation," is one of degree rather than of kind. In the one, the elements of nutrition are supplied gradually, continuously, and in quantity proportioned to the absolute requirements of the part; in the other, they are poured forth suddenly, copiously, and greatly in excess of all natural demand. To use a homely comparison, the basement membrane may be likened to a field, well tilled and sown, in which the nuclei of the membrane represent the seed; the intervening structure, the soil; and the fluid which is continually permeating this structure from the vessels beneath, the moisture, which latter, together with heat, forms a necessary condition for germination and growth. The nuclei, like the impregnated seed, are impressed with a vitality by virtue of which they are enabled, under the combined influence and co-operation of the natural temperature of the part, and of the fluid portions of the blood which are continually passing from the areolar to the epithelial surface of the basement membrane, to assimilate to themselves the intervening basement-structure, and thus to develope themselves into cells. This intervening substance is generally so proportioned that its most superficial layer is exhausted by the growth of the different nuclei into fully developed cells; but should any of the nuclei prove abortive, or should circumstances conspire to detach the growing nuclei from their nutritive matrix, which is frequently the case in both instances, the intervening superficial layer of basement substance is not exhausted, but cast off, as epithelial or basement patches, in the secretion of the part. Hence the secretion of the bronchio-pulmonary membrane consists essentially of epithelial cells, or mucus-corpuscles, disseminated through a homogeneous fluid menstruum; but owing, as before-stated, to the operation of accidental causes, all the nuclei of the basement membrane do not attain perfect cell-development, and thus patches of its superficial layer appear in almost every specimen of the fluid examined.

The Fibrous Tissue of the Bronchio-Pulmonary Membrane.—Subjacent to the basement membrane, we find a fibrous tissue, which, together with capillary vessels and filaments of nerves, forms a third layer entering into the composition of the bronchio-pulmonary membrane. These fibres are of two kinds, and are known as the white and yellow fibrous tissue. The diameter of the former varies from $\frac{1}{18,000}$th to $\frac{1}{22,000}$th of an inch; whilst the latter measure from $\frac{1}{8000}$th to the $\frac{1}{12,000}$th of an inch. The white fibres appear to pursue a definite, isolated direction, without branching or uniting with accompanying fibres; whilst the yellow send out branches in their course,

which unite with those of neighbouring fibres of the same character. The latter are far more numerous than the former, and to their property of elasticity may be ascribed the expansive and contractile power of the pulmonary cells; whilst to the former is probably owing the property of maintaining the patency of the cells during complete inactivity of the lungs.

Fig. 5.

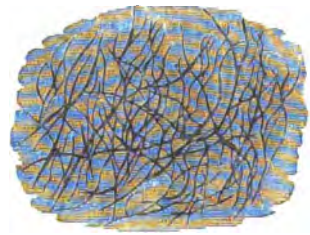

Fig. 5.—Fibrous tissue of the bronchio-pulmonary membrane.

Such then appears to be the particular structure and the individual uses of the different tissues which compose the bronchio-pulmonary membrane. Together they constitute a structure which forms innumerable involutions in the production of the pulmonary cells, the aggregate surface of the walls of which is calculated by Haller to be fifteen times greater than that of the exterior of the body. The diameter of these cells varies from $\frac{1}{200}$th to $\frac{1}{65}$th or $\frac{1}{70}$th of an inch, the former measurement being that of the cells situated towards the middle of the substance of the lungs, whilst the latter measurements more particularly represent that of the cells near the surface of the lungs, where the superjacent pressure of pulmonary tissue is less. The extent of surface obtained by this arrangement, and the free distribution of capillaries on the exterior of the cell-walls, have, for their special object, the exposure of the venous blood of the latter to the air contained in the pulmonary cells, by which an interchange of oxygen and carbonic acid gas takes place in extent proportionate to the healthy requirements of the system. In the healthy development of the body, the means, in this respect, are exactly proportioned to the ends required; nothing is deficient; nothing is in excess. We thus, by simple induction, infer, that these conditions cannot be altered without producing a corresponding modification of function of the affected part or organ, and through it a corresponding effect on the system at large, in proportion to the importance of the organ to the maintenance of life. But were life dependant on a strict maintenance of these exact conditions, disease would, in every instance, prove fatal. To obviate this, we find that life may be maintained, although perfect health may not be enjoyed, within a certain range of excess or deficiency of action of the different organs which contribute to the preservation of life, by virtue of the similarity of action of the different parts of the same organ, and of a kind of compensa-

tory action of organs exercising a totally different function in the system. Hence the complete abolition of the function of a portion of lung rendered useless by disease is endeavoured to be compensated for by an excess of action of the other portions of the same organ which are yet entire, in which effort the skin and liver may be made to unite. But this defective action of one, and excessive action of another, organ can only take place within certain limits, beyond which death must inevitably result. We thus learn the necessity of preserving, as far as may be practicable, the natural balance or equilibrium between the capability of action and the amount of function required, of an organ. Our efforts to do this must be based on a proper knowledge of healthy structure and of healthy action, coupled with a just estimate of the nature and effects of disease, as manifested in the modification or alteration of structure, secretion, and vital endowments generally, and of the power of remedial agents to affect the living body.

Now, preparatory to the pathological consideration of the bronchio-pulmonary membrane, the structure and uses of the tissues entering into its formation have been described. There yet remains to be considered the secretion with which its free surface is, in the healthy condition of the part, slightly besmeared, and by which it is, under ordinary circumstances, protected from causes of irritation, which would otherwise affect it from without.

This secretion, in the healthy state of the bronchio-pulmonary membrane, is more or less transparent, colourless, grayish, or slightly yellow, viscid, not entirely coagulable by heat,[1] precipitated by alcohol, and by the acetic and mineral acids, and not miscible with water, in which it floats, owing to the bubbles of air which it imprisons. Its chemical reaction is alkaline, and its composition involves the following organic and inorganic constituents.

	In 100 parts.
Water,	99·20
Organic matter,	·68
Chlorides of Sodium and Potassium, Alkaline Sulphates and Phosphates,	·12
Lime,	a trace
	100·00

It will thus be seen that it contains 0·68 per cent. of organic matter, of which the chief animal constituent is mucin, to which allusion will again be made.

Microscopically considered, mucus consists of definite, isolated, granular cells, floating in a viscid, homogeneous fluid, which latter is the contents of cells that have already attained their full growth,

[1] If a quantity of mucus be examined under the microscope, then subjected, between two slips of glass, to the flame of a spirit-lamp, and afterwards be re-examined, the cells will, from coagulation of a part of their contents, be found to be more granular than before the application of heat.

and have liquified in the process of natural decay. Hence, as an accidental, although an almost invariable, constituent of the bronchio-pulmonary secretion, we find shreds or debris of the cell-walls, with which a few superficial basement patches, the origin of which has already been explained, are generally associated. The granular cells, or mucus-corpuscles, when they have attained their full development, measure from $\frac{1}{1500}$th to $\frac{1}{2500}$th of an inch, whilst their granules, on an average, equal $\frac{1}{18,000}$th of an inch. In this condition they are isolated, present a dark, well-defined outline, and are more prominent than pus cells, with which they are frequently confounded, but from which they are distinguishable by the last two characters, and by the greater average diameter of their granules.

Fig. 6.

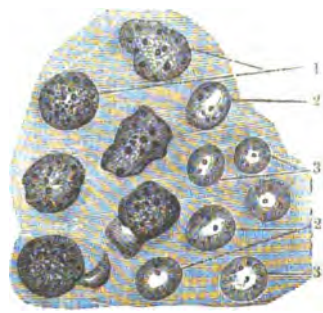

Fig. 6.—1. Corpuscles as seen in tough gray mucus.
2. Corpuscles as seen in ordinary mucus.
3. Fully developed pus-cells.

They are evidently the epithelial cells of full growth, and their contents are not so much the result of any absolute secretory power which they possess, as of an endosmotic property, which allows the gradual imbibition of surrounding fluids to take place into their cavity, in proportion to the growth and vital extension of the cell-walls. This statement is in accordance with the doctrine, that growth and secretion do not take place at the same time—only growth.[1]

Thus the nuclei of the basement membrane germinate, grow, and develop a cell-wall at the expense of the molecules of fibrine which surround them, and which form a part of the basement membrane; whilst the albumen, the other chief constituent of that membrane, rendered soluble by the watery and saline constituents of the blood, which, as before stated, are continually permeating it, passes by endosmosis into the cavity of the cells. The nuclei and cell-walls, therefore, consist of fibrine, whilst their contents are no other than albumen in combination with the various saline constituents mentioned in the table above.

[1] Professor Goodsir.

The proofs that such is the chemical composition of the mucus-corpuscles, may be found in the separate application of acetic acid and liquor potassæ to different portions of the secretion itself, as well as to the epithelium and basement membrane. Before, however, detailing the action of these re-agents on the structures just named, it is necessary to remark, that acetic acid, contrary to the opinion expressed by physiologists, does not *dissolve* albumen, but, like other acids, *coagulates* it.

The following simple experiment, which I have frequently repeated with the same result, will prove the truth of my position. If to the white of egg acetic acid, free from all trace of sulphuric, hydrochloric, and nitric acids, be added, and the mixture be stirred with a glass rod for a few minutes, distinct flocculi of coagulated albumen will immediately form. If more acetic acid be now added, and the mixture, after stirring, be allowed to stand for a short time, the whole will pass into a firmly coagulated mass, in which condition it may be dried and preserved almost any length of time.

To determine the different degrees of coagulation, the following experiment was performed. Two drachms of the white of egg were measured, and one drop of pure acetic acid was added. A small, distinct, and isolated coagulum was immediately formed on stirring the mixture with a glass rod. At intervals of a few minutes the acetic acid was still added until fifty drops had been used. The coagulum increased at each addition of the acid, and when fifty drops had been added, it occupied the whole fluid. After remaining at rest for an hour, other sixty drops were added, in ten-drop quantities, at an interval of a few minutes, when a firm coagulum formed, and was not displaced on inverting the glass containing it.

On this point Dr Carpenter ("Manual of Physiology," page 101) says:—"Nitric acid is particularly efficacious in occasioning coagulation (of albumen); on the other hand, acetic acid and common tribasic phosphoric acid do not precipitate it, these acids having the property of dissolving pure albumen." Again (page 102 of the same work), in speaking of caseine, the same author remarks, that caseine "differs from albumen, however, in this, that it does not coagulate by heat, and that it is precipitated from its solution by acetic acid." In his "Principles of Human Physiology" (3d edition), page 78, he further states, that albumen "is thrown down from its solution in a coagulated state by alcohol, creosote, and by most acids (particularly nitric), with the exception of the acetic."

Kirkes and Paget ("Handbook of Physiology," p. 14), state that "coagulated albumen,—*i.e.*, albumen made solid with heat is soluble in solutions of caustic alkali and in acetic acid, if it be long digested or boiled with it."

With a view of ascertaining how far this statement was correct, I subjected two scruples of albumen, coagulated by heat, to the action of a drachm of pure acetic acid for five days, at the expiration of which

time not the slightest change had occurred in it. When, however, the same quantity of coagulated albumen was boiled with acetic acid, it underwent solution in little less than half an hour. Liquor potassæ had also the effect of causing perfect solution in about twelve hours.[1]

From the foregoing experiments it is clear, that acetic acid coagulates albumen, and that liquor potassæ dissolves it, whether it be semi-solid, as we find it in the egg, or coagulated by heat.

Fibrine, on the other hand, swells up, becomes spongy, semi-transparent, and of a light yellowish-brown colour, from the action of acetic acid; whilst by liquor potassæ it is charred, shrivelled, and somewhat hardened, rendering, however, by a very slow disintegration, the fluid in which it is immersed of a brown colour, like water impregnated with burnt sugar.

Now, proceeding upon these data, we shall find, in the application of these tests to the bronchio-pulmonary membrane and its secretion, that acetic acid, by rendering the cell-walls more transparent, and by precipitating, in small granules, the albumen of the mucin within the cells, gives an increased transparency to the epithelium generally, and an increased distinctness to the cell-nuclei and granules. When applied to the basement membrane, it expands the nuclei and the molecules of fibrine which form a part of that structure, owing to which the basement membrane is rendered lighter and more transparent. This effect is not counteracted by any chemical action on the albumen of the basement membrane, because the experiments before detailed prove that cold acetic acid has no effect on coagulated albumen. Hence, the basement membrane is simply rendered lighter by the action of acetic acid, by virtue of the chemical effect of the latter on the fibrine alone. Acetic acid, added to a quantity of mucus, causes it to run together, and to assume a kind of ropy, tenacious, semi-membranous structure, which is the result of the coagulation of the free albumen of cells which have already liquified and passed to decay.

Now the immediate action of liquor potassæ on the bronchio-pulmonary epithelium is manifested by,—

1. An increased transparency of the cells, owing to complete liquefaction of their albuminous contents.

2. A more or less breaking up of the epithelium, owing to solution of the albumen which, as a part of the basement membrane, intervenes between the edges of the epithelial cells.

Sometimes these cells are separated in patches; at other times they are isolated, or have a small fragment of the intervening basement structure attached to them. When liquor potassæ is applied to the basement membrane itself, that structure undergoes partial solution, owing to the albumen of which it is in great part composed;

[1] Dr Pereira, in his "Materia Medica," vol. i., p. 399, propagates the above mentioned errors, with respect to the action of acetic acid on albumen.

and the nuclei and granules of fibrine are set free; but if the action of liquor potassæ be prolonged, they too are gradually charred, and ultimately dissolved, and in this way the whole of the basement membrane is destroyed.

If liquor potassæ, or any other alkali, be added to the mucous secretion of the bronchio-pulmonary membrane, the granular cells at first become more transparent, owing to the solution of the substance which intervenes between the different granules, then a number of the granules themselves disappears, and subsequently some of the smaller cells appear so globular, from distension of their cavity by the liquor potassæ, that none of the granules can be seen. By and by they again collapse, and now a dark central nucleus is visible, around which, at some little distance, a faintly dark outline of a cell-wall can be distinguished, the space between which and the central nucleus is of a light colour, as shown in figure 2.

If the specimen under examination be set aside for two or three hours, and then re-examined, each nucleus will be found to have spread, which is no doubt owing to the charring and gradually disintegrating action of the liquor potassæ; for a drop or two of distilled water being added, they wash away the loosened molecules of the nucleus, and show it to have undergone a reduction in its original size.

(To be continued.)

ARTICLE II.—*Case of Acute Rheumatism, succeeded by Chorea and Affection of the Heart.* By J. WARBURTON BEGBIE, M.D., Fellow of the Royal College of Physicians, and Physician to the New Town Dispensary.

(Read to the Medico-Chirurgical Society of Edinburgh, November 24th, 1852.)

THE relation subsisting between the two diseases, rheumatism and chorea, has now for some time been recognised by the profession, while from many of its members the subject has received much attention and study. That an amount of obscurity, however, still exists in regard to it is evident from the various explanations which have been advanced to account for the association of the rheumatism with the nervous disorder; and although the subject has engaged, and does still engage, the notice of many fully qualified for its investigation, it will not, I think, be considered that the recording of any accurately observed case is to be looked upon as superfluous or uncalled for. Acting on this belief, I beg to submit to the Society the following case, and the few remarks which succeed it:—

J. O., æt. 10, daughter of a groom, was placed on the roll of the New Town Dispensary on the 7th June last. I saw her that afternoon, in the house of her grandmother, in Jamaica Street. She

was a delicate-looking but comely child, of fair complexion. In infancy she had suffered severely from measles and hooping-cough. Her grandmother, with whom she resided, had for many years been a cripple from rheumatism; and her own father had had two attacks of rheumatic fever. The little patient had been seized two days before my visit with shivering, followed by the occurrence of severe pains in the larger joints of both legs and arms. At the time I saw her, the knees, ankles, and all the smaller joints, were much swollen, red, and very painful. The shoulders, elbows, and joints of hands, were similarly, though to a less extent, affected. There was much fever, quick pulse, and furred tongue. The urine was dense, and the bowels confined. Heart's action violent, but unattended with any morbid sound.

She was ordered a purging dose of calomel with half a grain of opium, and a black draught to be taken the following morning.

June 8th.—The pains and fever much as yesterday. Has slept pretty well. Ordered a pill with one grain of calomel and one-fourth of a grain of opium to be taken four times in the twenty-four hours.

It is not my object to dwell on this portion of the case, and I need not, therefore, weary the Society by detailing the daily condition of the patient; let it be sufficient to say, that she passed through a very severe rheumatic fever, being only convalescent on the seventeenth day. She did not, however, so far as I could make out, suffer during that period from any extension of the rheumatic inflammation to the heart or pericardium. There certainly was not on any occasion during these fifteen days any morbid sound to be detected on auscultation, or any increased precordial dulness to be made out on percussion. The mouth had been slightly affected by the mercury, which had immediately thereafter been suspended. Up to the 23d of June I saw her daily; from that date she was convalescent, and thereafter, up to the end of the first week of July, I saw her frequently: on more than one occasion examining her chest, and always with the same result. On the 20th of July, not having seen her since the 7th, I was requested to visit her, on account of a peculiar shaking and nervousness, to use her mother's words. I found her suffering from confirmed chorea. The mother informed me, that the disease had been creeping on for some days, and that ever since her rheumatic fever she had been nervous, and very easily excited and alarmed, which was different from her previous character. She could not attribute the occurrence of the disease to any particular cause, indeed it had come on gradually; but she was quite sensible that it had been augmented by a fright, sustained the day previous to my visit, when a cat had somewhat incautiously attacked a favourite hawk, which was kept in the room in which my patient slept. She had given a loud scream at the time, and during the following night had awoke from sleep with the same. When I saw her, the choreic affection of the lower limbs was so aggravated as to render

progression nearly impossible, the arms, especially the left, were similarly affected. The left side of the body was, on the whole, more markedly affected than the right. Articulation also was greatly impaired, and she made most singular grimaces. She had never suffered from any nervous disorder before. The tongue had become furred.

On examination of the heart, I now, for the first time, noticed a deviation from the natural sounds. A distinct blowing murmur took the place of the first, as heard towards the apex of the heart, and this was accompanied by a greatly increased action of the organ.

Ordered a purge of jalap, to be repeated each day for three days.

The purges improved the tongue, but did not apparently influence the nervous disorder. Accordingly I ordered her to take five drops of Fowler's solution twice daily immediately after meals.

The arsenic was continued regularly without the intermission of a single dose, from the 24th of July up to the 20th of September, when the physiological action of the mineral being induced, as represented by a very white tongue, swollen eyelids, and a somewhat irritable state of the mucous membrane of the stomach and bowels, I ordered its suspension. During that period the nervous affection had perseveringly continued, at times augmented in severity, at others apparently yielding, at one period so severe as to cause the strict confinement of my little patient to the house, at another, permitting her to walk, assisted by her mother, and even to visit me at home.

On the 23d of September, I made the following note of her condition:—Chorea almost entirely disappeared; walks well and steadily, though she still complains of occasional sinking at the knees; slight twitches of the muscles of the face. The cardiac murmur continues distinct; clearly heard over the whole cardiac region; it is most distinct towards the apex, and is not propagated in the cervical vessels.

November 17th.—Is quite free from all choreic movements; expresses herself as well, except for the beating at the breast. The palpitation is at times inordinate. No decided increase of precordial dulness exists. The murmur continues as before.

Remarks.—This case is similar to many others which have been placed on record, and is, with one point of difference, the same as more than one case alluded to by Dr Kirkes, in his most interesting papers[1] on this subject, the only dissimilarity being, that the evidence of affection of the heart, though looked for during the rheumatism, was not detected, but became established about the time of the recurrence of the chorea. I had indeed come to the conclusion, that happily the heart had escaped implication in the disease. The case

[1] Medical Gazette, 1850.

is interesting, from the speedy manner in which the choreic affection succeeded the declension of the rheumatic. There is one question of great interest to which I should wish to draw the attention of the Society, and that is,—The association of disease of the heart, unattendant on rheumatism, with the nervous disorder. I think that the evidence of some affection of the heart will be found in many cases of chorea. In Dr Kirkes' analysis of thirty-six cases, three were of this nature. In some cases the cardiac affection will no doubt be found to be inorganic—functional derangement merely—attended by a murmur with the heart's first sound, heard most distinctly over the upper sternum, propagated in the cervical vessels, and in all probability associated with the so-called venous murmur, in the neck. Further, the general appearance and symptoms of such patients point to the probable dependence of both nervous and cardiac affection on a disordered state of the blood, in short, on anæmia.

But apart from these, there is another class of cases, in which the evidence of organic disease of the heart, independent altogether of rheumatism, is quite as marked as its functional derangement is in the former. I remember to have seen one such well-marked case under the care of Dr Paterson—now of Tiverton—in the Royal Infirmary, an account of which, with Dr Paterson's concurrence, I afterwards published.[1] The patient, a boy of seven years of age, was admitted into the hospital, suffering from a first, but very severe, attack of chorea. Immediately on his admission, a loud musical murmur was detected accompanying the first sound of the heart, heard most distinctly towards its apex. Neither in this boy's history, nor in that of any of the members of his immediate family, which were carefully inquired into, was there any account of rheumatism. Under treatment the chorea speedily subsided, and after a residence of nearly a month he left the hospital, the cardiac murmur remaining as before. Scarcely six weeks thereafter the little boy died suddenly, sitting at his tea table—he dropped down dead. Unfortunately I was unable to obtain an examination of the body; but the manner of the death certainly corroborated the opinion formed from the observation of the symptoms and physical signs of disease during life.

I do not propose to allude to the various theories known to all the members of the Society, which have been advanced to account for the association of rheumatism with chorea.[2] That which has been suggested by Dr Begbie has received the willing assent of Dr Watson and of Dr Kirkes, appears to accord with the views entertained by Dr Todd, and finds no adverse facts in the numerous observations of M. Sée, whose elaborate memoir on this subject, read to the Academy of Medicine, and published in their "Transactions," has left nothing to be added to its historical bearings.

[1] Medical Times, 1849.

[2] Monthly Journal, 1847.

The case I have now related certainly goes to establish the correctness of Dr Begbie's theory, that the morbid condition of the blood, which gives rise to rheumatism, also gives rise to chorea. The child had inherited from two generations the rheumatic diathesis, and only became a sufferer from chorea on the declension of a rheumatic attack. Assuredly no other theory which has been advanced so simply or correctly explains the now frequently observed facts of one member of a family being affected with chorea, another with rheumatism, and perhaps a third being the subject of both affections. But I acknowledge, with Dr Kirkes, that there still exist several very interesting circumstances, which require more attention and investigation, before the association of these two diseases shall become thoroughly understood.

ARTICLE III.—*Contributions to Toxicology.* By DOUGLAS MACLAGAN, MD., F.R.S.E.—(*Continued from* Vol. XIV., p. 135.)

Poisoning with Arsenic; Death after Seven Days; Detection of Minute Quantities of Arsenic in the Viscera.

SARAH GILLAN or M'Vey was tried at the Glasgow Winter Circuit, 1851, on an indictment charging her with having, "on one or more occasions, between the 20th and 29th days of September last, within the house in New Vennel of Greenock, occupied by her and her deceased husband, Thomas M'Vey, administered, or caused to be taken by him, several or one or more quantities of arsenic, or other poison, by which he died on 28th September." The facts of the case, so far as they bear on its medico-legal history, are shortly these :—

The deceased, early on Monday, 21st September, went by steamer from Greenock to Tarbet, in Argyleshire,—a voyage which, on an average, is of five hours' duration. He was observed to look unwell when he arrived at his lodging at Tarbet, and said he had been slightly sick, which he ascribed to his having taken a glass of spirits in the steamer. He had, before leaving Greenock, taken some loaf bread and tea, which was as usual prepared for him by the prisoner; and, in answer to inquiries as to the probable cause of his subsequent illness, stated that he had swallowed nothing else except the whisky above mentioned. On arriving at Tarbet he had some tea prepared for him, along with which he ate some loaf bread, and an oat "bannock," prepared by the prisoner, which he had brought with him from home.

Soon after taking this meal he began to vomit. The period which elapsed from the taking of this tea and oat-cake to the commencement of the vomiting was not very correctly ascertained, but appeared to be about half an hour. The vomiting was attended with

pain in the stomach, and was succeeded by purging during the night; in short, he had all the symptoms of irritant poisoning, to so severe an amount, that his landlady called for assistance (not medical) under the idea that her lodger was dying. He determined to return home next morning; which he did, and was seen in his house on the evening of this day (Tuesday), by the fireside, very ill. Next day, however, he was better, and was able to move about the house; and on Thursday was so well as to be able to go out for a little into the street. On Friday morning, however, on which occasion it did not appear that anything had been given to him which would have contained a fresh dose of poison, he was again seized with vomiting, of a thin, watery, greenish-coloured fluid. He remained very ill all Friday and Saturday; and died on the evening of Sunday.

The circumstances tending to inculpate the prisoner were, first, the fact of her having, shortly before these events, purchased arsenic on more than one occasion; that the existence of rats—the pretended reason for getting the poison—was very doubtful; and lastly, that there had been some ground for bad feeling between her husband and her, in relation to an Irishman who had lodged with them, who was kept in custody, though not indicted, as a suspected *particeps criminis*, and who certainly gave full warrant for any suspicions which might be entertained regarding him, by his singing sundry very uproarious songs at the deceased's wake on the Sunday evening, and by his actually taking steps, on the second day after his death, to procure "lines" for a marriage between himself and the newly-widowed spouse.

The result of the trial was a verdict of "Not proven." There was no attempt to deny that the death had taken place from arsenic, but the ground of defence was, there was a want of proof as to the administration by the prisoner. It certainly appeared that the real symptoms of poisoning manifested themselves first at Tarbet, whereas the indictment charged the crime as having been committed within the house at Greenock. Such an indictment, according to the judge's charge, required that the poison "be taken in or at the house in the Vennel of Greenock, and no other where; and if the arsenic was found to have been taken at Tarbet, that was a case to which this indictment could not apply."

No doubt the indictment would have been completely sustained, had it been proved that a second dose had been given to M'Vey, after his return home; but no proof of this was given, nor does it appear to me in reality that such had been the case. I believe the truth to be, that the poison was contained in the oat bannock, the probability of this being strengthened by the fact, that the druggist who supplied the arsenic to her, mixed it with oatmeal in his shop; and thus the poison really was taken at Tarbet. It could hardly have been contained in any of the articles taken at home by M'Vey, at his early meal before starting on the morning of the 21st; for the symptoms, which that same day at Tarbet were so acute, could hardly have

delayed showing themselves for upwards of five hours; and had the slight sickness which the deceased had felt before arriving at Tarbet been owing to the commencing action of a large dose of arsenic, he would hardly have been so well, on arriving at his destination, as to take a fresh meal. That the prisoner, and perhaps her paramour also, had a guilty knowledge that the cake contained arsenic is most probable; but there was a defect of evidence as to who prepared, and who gave him, this cake. The prisoner, therefore, having the benefit of these doubts, escaped with the above verdict.

The case had been placed in the hands of Drs Auld and Laurie of Greenock, who made an excellent report on the post-mortem examination, and who, moreover, subjected certain parts of the body to chemical analysis. The result of their experiments was such as to lead them to the conclusion that arsenic was present, but being desirous of having their opinion confirmed by further investigation, the Crown authorities submitted the case to me. My first experiments with Reinsch's process showed that there was arsenic present, but in excessively minute quantity. It appeared to be a case where Crown counsel might probably eventually wish the analysis repeated by another expert, and as the amount of material would probably prove insufficient for allowing of two satisfactory series of experiments, I got permission to associate my then colleague, Professor Anderson, with me, and the result of our researches was embodied in the following conjoined report:[1]—

"Edinburgh, 26th November 1851.

"We, the undersigned, have, in accordance with instructions from the Crown agent, chemically examined various portions of the body of the deceased Thomas M'Vey, of Greenock, and have to report as follows:—

"On 9th October, there was delivered to Dr Maclagan, by John Wyllie, sheriff-officer, Greenock, a box, enclosing eight jars, containing separate portions of the body of Thomas M'Vey, and labelled in correspondence with an inventory, signed by Drs Auld and Laurie of Greenock. Subsequently, on 29th October, there was delivered to Dr Maclagan, a box, enclosing a jar, containing one of the kidneys and a portion of the lung of Thomas M'Vey; and lastly, there was delivered to Dr Maclagan, on 3d November, also by the hands of John Wyllie, a box, in which was a pot, containing the spleen, a portion of the liver, and a portion of the small intestines, of Thomas M'Vey.

"Every one of these articles was duly secured and sealed, the seals were unbroken, and the contents of those examined by us corresponded with the labels and inventories of Drs Auld and Laurie.

"The object of our researches was, to determine whether or not arsenic was present in any part of the body of Thomas M'Vey.

"The first article subjected to examination was the jar No. 3 of the box delivered to Dr Maclagan on the 9th October, and labelled portion of stomach and contents. This portion of stomach presented the appearance of general diffuse redness, especially at the pyloric or right end of the organ. The contents were found to be only about a teaspoonful of greenish gray tenacious

[1] The scientific reader will bear in mind, that such a report as this is worded so as to be intelligible to an ordinary jury, to whom it is always read in court.

mucus. No foreign matter of any kind was observed. The first experiment with the stomach was made by Dr Maclagan alone, who followed in this instance what is well known to chemists as Reinsch's process for the detection of arsenic. About a third part of the portion of stomach sent was used in this analysis, and the results obtained by Dr Maclagan, although not amounting to unequivocal proof, were such as to lead him to the conviction, that arsenic was present, but only to a very small amount.

"Dr Maclagan, believing from this preliminary experiment that the investigation of this case would require a series of somewhat tedious and delicate operations, obtained from Crown counsel authority to associate Dr Anderson with him in the inquiry, and, feeling the propriety of examining some other portions of the body than those already sent to him, applied for and obtained the articles delivered to him by John Wyllie on the 29th October and 3d November.

"All the subsequent investigations were made by us conjointly. In all our experiments we employed the following method of analysis, which is commonly known as Marsh's process. This consists in placing the suspected matter in contact with pure zinc and a pure acid,—that which was used was muriatic or hydrochloric acid; and both our zinc and acid were prepared by ourselves, and were ascertained by repeated trials to be entirely free from all impurity from arsenic.

"By this Marsh's process, if any arsenic is present, arseniuretted hydrogen gas is evolved, from which the arsenic can readily be separated by causing the gas to pass along a glass tube heated to redness, the effect of which is to cause the deposit in the interior of the tube of arsenic, in the form of a dark metallic crust. It is necessary, however, in such investigations, to prove that the metallic matter thus deposited in the tube is really arsenic, and for this purpose we followed a method recommended by one of us, as giving the most ready and unequivocal evidence on this point. This consists in placing the tube containing the metallic crust in a bath of oil, and exposing it to a heat not exceeding 500 degrees of Fahrenheit's thermometer. At this temperature the metallic deposit of arsenic is volatilized, becoming at the same time converted into the substance called arsenious acid by chemists, but better known as white arsenic, and is deposited in the upper part of the tube in the form of crystals, readily recognisable by their appearance. No other substance but arsenic possesses these properties.

"By the above proceedings we examined the under-mentioned portions of the body of Thomas M'Vey, and obtained the following results:—

"1st. Portion of liver with gall-bladder attached, No. 8 of the box delivered to Dr Maclagan on 9th October. The whole material here weighed two and a half ounces. From this we obtained several stains of metallic arsenic in separate tubes, which, when subjected to the oil-bath, gave very distinct and characteristic sublimates of arsenious acid. The quantity of liver here acted on being very small, we repeated the analysis of this organ on one-half, being five ounces, of the portion of liver sent us on 3d November. From this we obtained a very copious metallic crust, which in the oil-bath sublimed into very fine crystals of arsenious acid. We now took this tube, boiled it in distilled water, and applied to the solution thus obtained the three established liquid tests for arsenic—viz., sulphuretted hydrogen, ammonio-nitrate of silver, and ammonio-sulphate of copper—all of which gave the reactions characteristic of arsenic; thus affording additional indisputable proof, if any had been wanted, of the presence of arsenic in the liver of Thomas M'Vey.

"2d. The kidney which was contained in the jar sent to us on 29th October. From this also we obtained a characteristic metallic crust, and crystalline sublimate of arsenic.

"3d. Upper portion of duodenum, being No. 4 of the box of 9th October. From this we obtained a slight metallic crust, which in the oil-bath entirely

sublimed, and which therefore, no doubt, was arsenical; but it was not sufficiently large to afford a characteristic crystalline sublimate.

"4th. Portion of stomach. This was the remainder of portion of stomach formerly subjected to Reinsch's process. We now, by Marsh's process, obtained from it a small but distinct deposit of arsenic.

"We also examined a portion of the spleen and small intestines, from neither of which, however, did we succeed in procuring any arsenic. But, after the very unequivocal results obtained in our previous experiments, especially with the liver and kidney, we did not think it necessary to examine any of the other articles submitted to us.

"The results of our experiments, then, are,—

"That the body of Thomas M'Vey contained arsenic.

"That this poison existed abundantly in the liver, to a nearly equal extent in the kidney, to a slight amount in the stomach, and in the duodenum and pancreas; and that it could not be detected in the spleen, and in a part of the small intestines.

"The detection of arsenic in these various bodily organs proves that it must have been introduced during life, as it could have reached them only by being absorbed into the blood, and deposited in these tissues. The impregnation with arsenic of so many distinct organs of the body, and of some of them, as the liver and kidney, to such an amount as found by us, shows that there had been administered a large and probably fatal dose of that poison.

"ANDREW DOUGLAS MACLAGAN.
"THOMAS ANDERSON."

Upon the above case I beg to offer one or two remarks. First as to the symptoms. It will be observed that there was a marked intermission in the course of M'Vey's fatal illness. He had no vomiting, at least to any severe extent, during the Wednesday and Thursday, but on the Friday morning vomiting recommenced. Of course the natural explanation of this was, as implied in the indictment, that the prisoner had repeated the dose, and there can be no doubt that the woman had access to the deceased all this time. But, in the first place, there can be no doubt that intermission of symptoms generally, and of vomiting in particular, does occur in cases of arsenical poisoning. There was a good exemplification of this in the instance of the girl Davidson, formerly reported by me (*Monthly Journal*, January 1852), whose vomiting diminished on the fourth day, was trifling on the fifth, was absent on the sixth, but returned, accompanied by purging, on the night of the seventh. In this (Davidson's) case, for reasons assigned in my account of it, there could not have been a repetition of the dose, and yet there was, on these days, distinct improvement in the general symptoms, and a temporary cessation of the vomiting. It is not necessary, therefore, because M'Vey relapsed into vomiting on the Friday, and ultimately sank two days after, to suppose that a fresh dose had then been given to him. Secondly, the facts of the case do not appear to warrant such a conclusion. He was not observed to vomit more than once—on the Friday morning—and that was after taking some tea, which, though the prisoner was in the house, was prepared for him by his mother-in-law, and handed to him by his son. The murderess would not have trusted the administration of a finishing dose to other hands,

and no suspicion attached to any of the other parties. In fact, M'Vey's death seems to have taken place rather with symptoms of gradual sinking than of great violent irritation. The information on this point is defective, as he was not seen by any medical man; but the witnesses who testified as to his state during the last days of his life, spoke to his being "very low," and complaining that he was "losing his sight now and then." He had no coma, retaining his consciousness, and conversing freely, within a short time of his death. Lastly, it appears to me that, had a fresh dose been given on the Friday, which proved fatal on the Sunday, there should not have been so small an amount of arsenic detectable in some of the organs of the body, especially the tissues of the stomach, duodenum, and small intestines. There was not enough of vomiting to procure complete ejection of such a dose in that way, and it could not otherwise have been eliminated from the system to such an extent in that space of time.

I am not disposed dogmatically to assert that there was here a true intermission, and afterwards a fatal relapse, in the symptoms, from a single dose. We must admit the possibility of a second quantity having been given. But there is at least a good probability that this had not been the case. I have ventured to call attention to this point, because it is a question frequently put by counsel, in reference to accusations of repeated administration of the poison, whether intermissions do take place in acute arsenical poisoning, and whether the recurrence of vomiting, etc., after a temporary cessation, infers the probability of a fresh administration. No doubt, after a long lapse of time, and a complete, or even considerable, restoration of health, this is the legitimate inference from such an occurrence; but where the symptoms have only been partially, or for one or two days, in abeyance, a medical witness must be prepared to admit the possibility of an intermission and subsequent recurrence of symptoms, independently of a second administration. We may, therefore, give a greater latitude to the following remarks of Dr Christison. "In some cases of the kind now under consideration, a short remission, or even a total intermission, of all the distressing symptoms has been witnessed, particularly when death is retarded to the close of the second or third day. This remission, which is accompanied with dozing stupor, is most generally observed about the beginning of the second day. It is merely temporary, the symptoms speedily returning with equal or increased violence."

The next point on which I would offer a single remark is, as to the organs in which the poison could be detected in largest quantity. There is no doubt in my mind, from the examination of a good many cases, that where the quantity of arsenic left in the system is but small, the liver holds the first place, and the kidney the second, as the organs in which the poison is most likely to be found. My friend Dr George Wilson, in an interesting paper on the chronic poisoning of horses by lead, has pointed out the spleen as the organ

in which, in these animals at least, the lead can be most readily detected (*Monthly Journal*, May 1852). Dr Wilson, in calling the attention of physiologists and medical jurists to this question, restricts his remarks to lead. It does not appear to apply to arsenic. In M'Vey's case, and in another, where I examined both liver and spleen, the latter organ in the one instance yielded none, and in the other very little, of the poison, whilst it was in both readily found in the liver. I should have thought that the fact, that the liver afforded the best chance for detecting arsenic was pretty generally known; but that little attention has been paid to this has been proved to me by the fact, that, in a large proportion of cases submitted to me for analysis, the liver is not sent till specially asked for.

Lastly, I beg to call attention to the procedure followed by Dr Anderson and myself in this case for the detection and identification of the arsenic. I allude to the collection of the arsenic, where the quantity is very minute, in small glass tubes, from Marsh's apparatus, and the proving of its real nature by sublimation within these tubes at a regulated temperature in the oil-bath (*vide Monthly Journal*, Nov. 1848, p. 290.) To me this process has proved so simple and so easy of execution, so delicate in the results obtained by it, so advantageous in excluding the necessity for any chemical re-agent whatever, for distinguishing arsenic from antimony or from anything else, and also in affording, when tubes of equal size are used, so easy a method of determining approximatively the proportion of arsenic in different articles examined, that, in operating on small quantities of material, or where little arsenic is present, I have of late always in practice adopted it in preference to any other. Nothing, assuredly, can surpass Reinsch's process in simplicity, and it is capable of detecting very small quantities of arsenic; but in M'Vey's case, and in many others which have occurred to me, the oil-bath process has yielded unequivocal results, where the same could not have been sworn to from Reinsch's method.

A very ingenious method for collecting the whole of the arsenic in a suspected matter has been described simultaneously and independently by Dr Schneider of Vienna, and Dr Fyfe of Aberdeen. It consists in putting the article to be analysed into a retort, along with lumps of common salt, gradually adding to this sulphuric acid, and distilling into a very cool receiver. The fluid is a solution of chloride of arsenic, from which the whole amount present may be separated and weighed, as sulphuret of arsenic. I have repeated this process more than once, and especially in one case, in which arsenic had previously been detected by Reinsch's method. But, ingenious as it is in theory, it is rather troublesome in execution, and the results were not so satisfactory as these authors led me to expect. In this last case there was a considerable amount of putrefaction, and the distillation, instead of yielding me a clear fluid, containing all the arsenic in the state of chloride, left the larger part of the arsenic in the neck of the retort in the form of yellow sulphuret.

This, of course, was easily recovered, by washing down the neck of the retort with ammonia. But the most serious objection which it presented to me was, that the distillate, which was of intolerably fœtid odour, had a quantity of a black greasy animal matter floating in it, which I could not remove by filtration. Before determining the absolute amount of arsenic present, which was my object in the experiment, this had to be destroyed by nitric acid, or aqua regia; and as this entailed the conversion of the arsenic into arsenic acid, and its subsequent reduction by sulphurous acid before it could be estimated as sulphuret (As S_3), and this had to be united with what was deposited in the retort, it appeared to me altogether so troublesome, at least as applied to putrescent organs, that I cannot form much expectation in its being valuable in medico-legal practice. In fact, the chief result which it is designed to give, the precise estimation of the amount of arsenic present, is rarely of any importance medico-legally, though interesting in a scientific point of view.

28, HERIOT ROW, EDINBURGH, December 1852.

ARTICLE IV.—*Monthly Report on Cases treated in the Regimental Hospital, 79th Highlanders, during November* 1852. By JOHN GRANT, Esq., Surgeon.

Feb. C. C.

SINCE the regiment left Gibraltar in 1848, cases under this head have been so mild and ephemeral, as scarcely to merit the appellation of fever, being examples rather of pyrexia, or general malaise, consequent on a debauch or exposure to cold, and readily yielding to an emetic and purgative.

The first exception to the above observation was treated during the past month, being a case of

Feb.-Typhus Mitior.

The prominent symptoms were cerebral,—viz., headache, partial deafness, injected conjunctiva, contracted pupils, delirium mite, dozing drowsiness, etc.

As he complained of pain in the right mammary region, he was bled to twelve ounces, and cupped over the chest, before the character of the disease was diagnosed.

The crassamentum was buffy, but not being cupped the propriety of venesection appears the more questionable, the firmness or amount of contraction of the coagulum being in my opinion more indicative of acute inflammation than the existence of the buffy coat.

The exhibition of Grave's mixture (tart. ant. and tinct. opii in camphor water) seemed to allay the cerebral irritation, and restrain the action of the bowels, which at first were rather relaxed.

The patient expressed his satisfaction after the surface, which was hot and dry, had been sponged with tepid water and vinegar. The pulse ranged from 102 to 120; tongue furred, but did not become dry till after the exhibition of wine, which was perhaps commenced rather early, although given with caution.

During the first week the urine was paler than natural, contained no albumen, and was voided with much pain. Towards the latter end of the second week he began to improve, the urine became darker (as the process of elimination advanced?), the pulse diminished in frequency, and he complained of acute pain in the calf of the right leg, where a small sloughing abscess has since formed. Several large pustules and minor abscesses have also appeared about the head and neck as convalescence proceeded.

This case is noticed, not from any peculiarity in itself, but as being the first instance of common continued fever occurring in the regiment for several years, an immunity which may be owing to the healthiness of the stations (Quebec and Stirling) where the regiment has been quartered.

Pleuro-Pneumonia.

This case was alluded to in the report for last month, and terminated fatally.

The right pleura was much thickened, the visceral adhering firmly to the costal surface, and with difficulty detached.

The upper and middle lobes of the right lung were quite solidified, and in a state of gray hepatisation, purulent matter exuding abundantly from a section of the parenchyma, which was of a gray granite colour.

The liver was much enlarged, presenting yellowish patches on its anterior surface, its substance being softened, and of a lightish drab colour.

I have no intention of asserting that the soldier of healthy constitution and sober habits may not be the subject of pneumonia, but have reason to believe that, on exposure to cold, or suppressed perspiration, he is more likely to escape with inflammation of the serous or mucous membranes (pleuritis or bronchitis), my past experience of pneumonia having been generally associated with a constitution naturally weak, or debilitated and disordered by habits of intemperance.

In the above case a large softened and discoloured liver, which must have been diseased for some time prior to the fatal pulmonary seizure, lay in close apposition with the right lung, which was the one chiefly and originally affected.

Besides the predisponent influence (*quantum valeat*) of juxtaposition, something more may be due to a similarity of function; both

organs being devoted to the depuration of the blood, it is not unreasonable to suppose, that a disordered liver, transmitting blood to the lungs imperfectly eliminated, and containing elements which should have passed into the primæ viæ, may operate prejudicially on the parenchyma of the lungs, and exert an influence as baneful as that of urea on the brain. Be this as it may, the coincidence or connection of diseased liver with inflammation of the lungs may not be an unimportant subject of inquiry; and finding pneumonia in military practice to be frequently connected with habits which, in the first place induce hepatic derangement, I have, for this reason, been led, after copious and repeated venesection, to prefer the treatment by calomel and opium to that by tartar emetic; and for this preference an additional inducement may be found in the very frequent combination of pleuritis with pneumonia.

In the case above alluded to there was not much cough, and little or no expectoration. The patient was largely and repeatedly bled, without arresting, and it is to be hoped (*pace eorum qui aliter sentiunt*) without accelerating the disorganising progress of the disease; the system was but slightly and with difficulty affected with mercury; and he died on the sixth day after admission, having, by his own account been several days ill before reporting himself sick.

Phthisis Pulm.—Pneumothorax.

In this case the existence of effusion in the left side of the chest being so evident, and the dyspnœa so distressing, paracentesis was performed, and upwards of two pints of purulent matter evacuated, affording decided relief, and protracting the patient's existence.

The other cases treated during the month were slight and unimportant.

EDINBURGH CASTLE, Dec. 1852.

ARTICLE V.—*Notes on some Ancient Greek Medical Vases for containing Lykion; and on the Modern Use of the same Drug in India.* By J. Y. SIMPSON, M.D., Professor of Midwifery in the University of Edinburgh.

THE physicians and surgeons who, in ancient times, pursued their medical profession at Rome, and in different parts of the Roman empire, have left us various palpable relics of their craft. Thus, in the ruins of Pompeii and Herculaneum, numerous surgical instruments, pharmacy, and drug-bottles, etc., have been found; and elaborate drawings and accounts of these have lately been published by Savenko, Vulpes, Renzi, and others. On the sites of the old

Roman cities and colonies throughout Western Europe, various surgical and medical relics of the same kind have been at different times discovered: as lancets, probes, cupping-glasses, scalpels, oculist-stamps, phials, etc. But of medicine, as it was still earlier exercised in Greece and in the Grecian colonies, few such tangible vestiges remain. We have, it is true, had carefully transmitted down to us the imperishable professional writings of Hippocrates and others of the purely Greek school; but time has spared few, or indeed almost no, material remnants of the professional instruments or vessels used by the ancient Greek surgeons and physicians.

Perhaps the great rarity of such archæological remains may serve as some apology for the present notice of some specimens of ancient Greek medical vessels or vases. Besides, the vases which I wish to describe are interesting in other points of view. They are all of them intended to contain one and the same drug, as shown by the inscriptions on their exterior; this drug was derived by the ancient Greeks chiefly from Hindostan,—one of the many points of evidence of the former freedom and frequency of the traffic between the south of Europe and India; and at the present day the same drug is still employed extensively and successfully, by the native practitioners of the East, for the very purposes for which it was, in former times, used by the medical practitioners of Greece.

The drug to which I allude is the *Indian Lycium* or *Lykion*, the ΛΥΚΙΟΝ ΙΝΔΙΚΟΝ of Dioscorides. In modern collections and writings, I know of four ancient vases or drug-bottles intended to contain this valued eye-medicine. If our museums, however, were properly searched, perhaps various other Greek vases, for the same or for similar medicines, would be detected. The four specimens of bottles or vases for *Lycium* to which I have adverted are the following:—

1. In the collection of Greek antiquities contained in the British Museum is a small vase, made of lead, and of the exact form and size represented in Plate I., Fig. 1. The vase is of a sub-ovoid form, and is somewhat above an inch in height, and about three quarters of an inch in breadth. An inscription, preceded by the ornament of a small tripod, encircles the middle of the vase. The inscription is in Greek letters, of which the following is a correct copy:—

This inscription may either be read as ΛΥΚΙΟΝ ΠΑΡΑΜΟΥΣΑΙΟΥ—the *Lycium of Paramusaeus*—as suggested to me by Mr Birch, who first had the kindness to direct my attention to this vase; or, and perhaps more correctly, it may be rendered ΛΥΚΙΟΝ ΠΑΡΑ ΜΟΥΣΑΙΟΥ—the *Lycium* sold *by Musaeus*. Mr Birch informs me, that he

thinks he met with the name of *Paramusaeus* as a medical practitioner in Fabricius' "Bibliotheca Græca." I have not been fortunate enough to detect the name in question, notwithstanding some considerable search through that learned work. On the other hand, the name of *Museus*, or *Musaeus*, is well known in Athenian biography. (See Fabricius' Bibliotheca, vol i. pp. 120-133). I should, perhaps, have already stated, that the vase in question was sent to the British Museum, among a collection of antiquities from Athens.

2. Through the kindness of M. Sichel of Paris, I am enabled to give in Plate I., Fig. 2, an engraving of a second *Lycium* jar, not hitherto published, of nearly the same dimensions as the specimen contained in the British Museum. This second specimen is not made of lead, but of pottery-ware. It bears upon its side the inscription :—

HPAKAEIoY
AYKoN

This inscription—"the *Lycium of Heracleus*"—has the word AYKON spelt without the I; errors of this kind being, as is well known, very common in old Greek and Roman letterings.

3. M. Millin of Paris published, nearly forty years ago, an account of a similar vase, found at Tarentum, a well known Greek colony and settlement. ("Description d'un Vase trouvè a Tarente." Paris, 1814.) This vase is slightly larger than either of the above, but somewhat mutilated. It is made of clay, and has on its front, in Greek letters, the inscription *Lycium of Jason*.

IACoNoC
AYKIoN

The form and size of this jar are represented in Plate I., Fig. 3. M. Millin fancied, that probably this small vase or jar was intended as a child's toy; but two years after he wrote, M. Tochon d'Anneci gave an account of a similar jar, and first suggested that it must have been destined to contain a collyrium or an ointment—*destinè a contenir un collyre ou un onguent.* (See his "Dissertation sur l'Inscription Grecque, et sur les Pierres Antiques," etc. Paris, 1816.)

4. The vase described by M. Tochon is delineated in Plate I., Figs. 4, 5, and 6. It is of the same material, and nearly of the same size, but less mutilated than that previously delineated by M. Millin. It presents also in front the same inscription (see Fig. 5), namely,

IACoNoC
AVKIoN

M. Tochon believes further, that this vase was found, like that of Millin, at Tarentum. At least, it was originally given to M. Tochon by a person who had resided for a long time in that city, and who had himself acquired the specimen there. M. Sichel has reason to think it not improbable that his specimen (Fig. 2) also came from Tarentum. And it is perhaps not uninteresting to remark, that Galen, Celsus, and various other old medical authors, repeatedly mention a Greek physician of the name of Heracleus or Heraclides, who practised at Tarentum, and was the author of various treatises on the Materia Medica, etc. (See an enumeration of his writings, etc., in Kuhn's "Opuscula," vol. ii. p. 156, etc.) Among his large collection of collyria and medicines for diseases of the eye, Galen gives formulæ for making different eye medicines bearing the name of Heracleus, as, for example, two "*agglutinatoria pilorum Heraclidæ Tarentini* (Ἡρακλείδου Ταραντινόν)." See *Kühn's edition of Galen*, vol. xii. p. 741.

The medicine mentioned in the preceding inscriptions, the LYCIUM or AYKION, was a drug which enjoyed much favour among the ancients; and it was supposed to be possessed of great medical value and virtues. It was used principally as an astringent remedy to restrain inflammatory and other discharges. Dioscorides, Galen, Oribasius, and Paulus Ægineta, dilate upon the medicinal properties of the Lycium. Two varieties of Lycium were in use, one obtained from Lycia and Cappadocia, etc., and the other from India. The latter was regarded as by far the most valuable. Thus, when treating of the two varieties of Lycium, Galen mentions the Indian as the most powerful for all purposes—το Ινδικον ἰσχυροτερον ἐστιν εἰς ἅπαν. (*De Simp. Medicam.*, lib. vii. 64.) Dioscorides recommends Lykion as an astringent for the cure of various complaints, as psoriasis, and pruritus of the eyelids, purulent ears and tonsils, ulcers of the gums, chapped lips, fissure of the anus; in cæliac and dysenteric affections, both in draughts and clysters; in hæmoptysis and coughs; in female fluxes, hydrophobia, and so forth. The Indian, he states, cures inflammation of the spleen and jaundice, prevents menstruation, purges water, and is a counter-agent to deadly poisons. (*Dr Adams' Trans. of Paulus Ægineta*, vol. iii. p. 234.) So late writers as Paulus Ægineta, Ætius, etc., allude also to the superior value of the Indian variety. For instance, in Roxarius' edition of Oribasius it is stated, that the Indian Lykion "præstat ceteris et est efficacius." (*Medicin. Collect.*, lib. xi.) Avicenna, the celebrated Arabian physician, who gives a long account of the medical uses, etc., of Lykion, remarks, "Magis vincens, secundum existimationem, est quod Indicum est," etc.; and he compares its properties with that from Mecca. (*Canon Medicinæ*, lib. ii. cap. 398.)

Of all the uses to which the Lycium was applied in medicine, by far the most important was the employment of this drug, and particularly of the Indian variety, as a collyrium or local application to

the eye, in the treatment of different varieties and forms of ophthalmic inflammation. Thus Scribonius Largus, the reputed body physician to the Emperor Claudius, and one of the most original among the ancient medical writers, declares that "he attributes to no collyrium whatever such great efficacy as to the genuine *Indian Lycium* used by itself. For if," says he, "near the commencement of ophthalmia, any one anoints himself with this collyrium, he will immediately—that is, on the same day—be freed from present pain and future swelling. It is unnecessary (he adds) to dilate on its virtues, for a person experienced only in other collyria would scarcely credit the effects of this simple drug." (*De Composit. Medicamentorum*, cap. 3.) Marcellus lauds its powers in nearly the same words. (*De Medicam., Lib.* cap. 8.)

The Lykion, or Lycium, is still used extensively by the native medical practitioners of India, under the Hindoo name of *Rusot* or *Ruswut*. In a learned article on the nature of the λυκιον of Dioscorides, contained in the "Transactions of the Linnæan Society," vol. xvii. p. 82, Professor Royle has shown, that the Indian Lycium or Rusot is an inspissated extract, prepared from the wood or roots of several species of Berberis, as the *Berberis lycium*, *aristata*, etc., growing on the mountains and plains of Upper India, and principally procured from Nuggur-kote, near Lahore.[1] "On inquiring," says Dr Royle, "in the shops of the druggists in the bazaars of India, I everywhere learned that both the wood (*dar-huld*) and the extract Rusot were imported from the hills into the plains, and that large quantities continued to be brought from Nuggur-kote as well as other places." And he adds,—"The *Rusot* is at the present day procurable in every bazaar in India, and used by the native practitioners, who are fond of applying it both in incipient and chronic inflammation of the eye; and in the latter state both simply and in combination with opium and alum. It is sometimes prescribed by European practitioners; and I have heard that it was found very efficacious by Mr M'Dowell in the ophthalmia of soldiers who had returned from the expedition to Egypt. I have myself occasionally prescribed it; and the native mode of application makes it particularly eligible in cases succeeding acute inflammation, where the eye remains much swollen. The extract is, by native practitioners, in such cases, rubbed to a proper consistence with a little water, sometimes with the addition of opium and alum, and applied in a thick layer over the swollen eyelids; the addition of a little oil I have found preferable, as preventing the too rapid desiccation. Patients generally express themselves as experiencing considerable relief from the application."

[1] The other variety of Lycium, described by Dioscorides as procured in Asia Minor (Lycia, Cappadocia, etc.), is now generally supposed to be an extract from the *Rhamnus infectorius*, or other species of *Rhamnus*. See Professor Royle, in *Linnæan Transactions*, vol. xvii. p. 87; Dr Adams, in his admirable edition of *Paulus Ægineta*, vol. iii. p. 234.

My friend, Dr Wise, the author of that learned work "Commentaries on the Hindoo System of Medicine," some time ago brought to Scotland with him a small quantity of the Indian Lykion. I have seen one or two cases of recent conjunctival ophthalmia treated by the application of this Lykion, with speedy relief and cure. Dr Wise has been so good as furnish me with the following interesting note regarding his own extended experience with it.

"The use (says Dr Wise) of the mixture of Lykion or Ruswut is very generally known over Hindostan, where diseases of the eye are common, and probably over Asia and Africa, if we are to believe that this was the black application employed with such success to the diseased eyes of our soldiers in Egypt. It is likewise probable that Dioscorides obtained it nearly two thousand years ago from the East, where the plant is indigenous, and introduced it into Europe. Having found great personal benefit from the application of the mixture of Lykion to my eyes when inflamed, I employed it extensively when superintendent of the Eye Infirmary, Calcutta; and so convinced was I of its efficacy, that I brought a supply with me to Europe with the intention of bringing it to the notice of the profession. I found you investigating the subject; and at your suggestion, Dr Walker was so kind as to try the medicine, and I am sure will inform you of the results he saw derived from its use. The Indian mixture consists of equal weights of Lykion and burnt alum, with half the weight of opium. These ingredients are mixed with lemon juice, and reduced to the consistence of cream, and applied round the eye-lids and over the eye-brow of the inflamed eyes. This mixture is washed off, and again applied twice in twenty-four hours; and it was only when accompanied with fever, that aperients and other parts of the antiphlogistic regimen were required. In less urgent cases the mixture was only applied at night, and produced no inconvenience, unless when it dried, and the lids felt stiff, when it was softened by applying a little moisture. I found the Lykion mixture most useful in all cases of inflammation of the external tunics of the eye. When both eyes were inflamed, it was interesting to mark the advantage this simple remedy had when applied to one eye, while the usual remedies of leeches, blisters, etc., were applied to the other eye. Another most important application of the Lykion is when the ophthalmia is accompanied with severe pain. On such occasions, after applying the mixture, a piece of live charcoal (*gool*) produced the most soothing effect when approached near the eye. With this intention, the charcoal was placed upon an earthen cup, and held on a wooden stand by the patient, and he approached or withdrew it from the eye according to his own feeling. The great relief in this case was in part from the anodyne effect of the opium."

Mr Walker has kindly given me the following note of his experience with the Lykion at the Edinburgh Eye Dispensary:—

"I have used (he writes me) the Indian Lykion in a considerable number of cases of eye disease. The affections in which I found it

most useful were those of the conjunctiva, such as the simple, catarrhal, and pustular forms of inflammation. In them its action was well marked and beneficial, the disease generally subsiding in a day or two; sooner perhaps than it would have done under the ordinary treatment. I have had no opportunity of trying it in purulent ophthalmia; but I believe that in it also it would prove of service. Cases of slight rheumatic and catarrho-rheumatic inflammations have been benefited, but not cured, by it alone. In some affections of the eye-lids, as ophthalmia tarsi and chronic ophthalmia, it did good; but such cases often get well with very little treatment. I applied it to the eye-lids in the form of a paste, with opium and burnt alum, as recommended by Dr Wise. This was repeated two or three times a-day. The patients generally complained of a burning and smarting of the lids after its application, similar to what is produced by a mustard blister."

The four ancient Greek vases, mentioned in the preceding notice, as inscribed with the name of the drug *Lykion* or *Lycium* are each of very small dimensions, the plate representing all of them of their original sizes and forms. They are small, in consequence, in all probability, of the foreign drug which they contained being difficult to procure in large quantities, and being hence an article of high price in the markets of Greece and Italy. The value set upon the contained drug would seem to be indicated by another circumstance —namely, by the shape of the interior of the vases. In the specimens described by Millin and Tochon, the cavity of the jars is narrow and conical from above downwards, the mouth being wide, and the interior becoming more and more tapering and contracted as it descends downwards. The section of the interior of the vase of Tochon, given in Plate I., Fig. 6, represents this peculiar and deceitful form of the cavity. In consequence of this peculiarity in their form, these jars contained, in fact, much less of the Lykion than their mere external appearance indicated. This remark, at least, holds true of the two vases from Tarentum bearing the name of *Jason*. The vase of *Museus* from Athens, belonging to the British Museum, appears more honest at least in its construction. The high price of the pure Lykion probably led also to the fact mentioned specially by Dioscorides (lib. i. cap. 133), Pliny (lib. xxiv. cap. 14), and Serapian (lib. ii. cap. 398), of the frequent adulteration of the drug. And, perhaps, as in similar inscriptions on some modern medicine-nostrums and packets, the names of the preparer or vendor, *Jason*, *Heracleus*, and *Museus*, stamped on the vases, were added in attestation of the purity and unadulterated character of the drug which these vases contained.

ARTICLE VI.—*Notice of Cases of Diabetes Mellitus treated by Rennet.*

By JAMES GRAY, M.D., Glasgow.

CASE I.—John Dryburgh, a married man, forty-four years of age, one of the foremen employed at St Rollox Chemical Works, temperate and regular in his habits, had always enjoyed good health until August 1850, when he observed that his calls to make water were more frequent, and the quantity voided much greater, than usual. The increased discharge was accompanied by great thirst. These symptoms gradually increased, until they became so aggravated that, he says, "had it not been for shame, I could have remained constantly by the well, drinking and making water by turns." His appetite was upon the whole good, every article of diet agreeing with him, except cheese. He was habitually costive, and troubled with uneasy sensations about the epigastric region. He had also experienced numbness in the upper extremities, from the elbow to the tips of the fingers; and in the lower extremities from the knees to the toes; and he likewise had occasionally a sensation of heat rushing along the arm and forearm to the fingers, continuing about fifteen minutes, and then leaving him feeble, and unable to do anything for some time afterwards. He had lost upwards of three stones in weight; and was very weak, low-spirited, and unable for his usual employment. He had tried various remedies, all of which proved ineffectual in giving him the slightest relief.

On the 7th of July the countenance was expressive of great anxiety; he was much emaciated; the pulse was 66, full and regular; the respiration slow; the mouth dry, in consequence of which he was unable to articulate distinctly; the tongue coated with a thick yellowish fur; the appetite good; and the thirst excessive, so that he drank upwards of three gallons of liquids in the course of twenty-four hours. The urine was very abundant, of a bright straw colour, sweet to the taste, loaded with sugar, as indicated both by the sulphate of copper and the caustic potash tests, and in density 1045.

He was ordered a table-spoonful of creasote mixture, to be taken when the thirst was urgent; and ten grains of the alkaline phosphate of soda, three times a-day, in a glass of water. His diet was made to consist of beef, fish, tea without sugar, eggs, bran-bread, butter-milk, and water. Under this course of treatment, and attending to the state of the skin and bowels,—alternating the creasote with tincture of nux vomica and tincture of colocynth,—he continued for three weeks, with little improvement in the general symptoms; during which period the density of the urine varied from 1045 to 1037.

On July 30th he was directed to take a tea-spoonful of rennet in a glassful of water after each meal, and to continue the alkaline phosphate of soda.

On August 3d the thirst was not quite so urgent, and he thought he did not pass so much water. He complained of distressing pain in the fore-arms and legs. He did not disrelish the animal diet. Urine paler in colour, and density 1027.

On August 7th he continued gradually improving, the density of the urine having descended to 1025. As the bowels were confined, he was directed to take ten drops of tincture of colocynth every four hours until the bowels should be acted on.

On August 13th he felt easier about the pit of the stomach; the pains in the arms and legs were not so troublesome; the urine was acid to litmus paper; contained no albumen, but a superabundance of sulphates, chlorides, and uric acid.

On August 21st he felt decidedly better; the thirst has greatly abated; the bowels were regular; the tongue clean; and he slept well. The quantity of food and drink now used in twenty-four hours was 3 lbs. of meat, ¾ lb. brown bread, seven cups of tea without sugar, eight ounces of water, and half as much butter-milk. The density of the urine was 1024; and there were only slight traces of sugar. The alkaline phosphate of soda was discontinued; and a few drops of muriatic acid were given along with the rennet.

He had a smart attack of diarrhœa on the 24th. On the 25th he gave up taking the rennet. The tongue was slightly coated with a yellowish fur; and the stools black coloured. He slept well. The quantity of urine passed in twenty-four hours was sixty-four ounces, and its density 1022·5.

I could not detect the slightest trace of sugar by the caustic potash test. The uric acid was present in lozenge-shaped crystals. Lactic acid was found by Pelouze's test; that is, on boiling the urine with caustic potash, it became turbid, and when allowed to stand a deposit fell to the bottom of the test tube, which, when placed on a slip of glass, partially evaporated, and, examined under the microscope, presented numerous crystals, some of them much resembling a sheaf of corn, and others arborescent. This deposit is dissolved on the addition of a solution of prussiate of potash.

On August 28th the tongue was clean and moist. He had no longer any thirst, and took no more of fluid than he used to do while in perfect health. He was always ready for food, and was using more farinaceous food. He did not feel any uneasiness from being restricted to the diet prescribed. He had gained six pounds in weight. No rennet had been taken for three days. The density of the urine was 1024. The rennet was resumed.

On September 2d he felt daily stronger, had no thirst, and fewer calls to pass urine; slept well; felt no longer pains in the stomach, fore-arms, and legs; was able to work, and did so with pleasure; and had added three additional pounds to his weight. The urine was 1022, and presented slight traces of sugar.

On September 8th he still continued to improve, and gained flesh daily. The density of the urine was 1022; it contained lactic

and uric acids; but, when boiled for some time with caustic potash, did not change colour.

On September 13th he said his health appeared to himself as good as previously to his illness.

On November 8th he felt quite well; had gained about two stones in weight. Since commencing the rennet the urine had a density of 1018, and no sugar could be discovered in it by any test. He was taking the ordinary mixed diet, and continued the rennet.

December 24th.—This patient continues to enjoy good health.

CASE II.—William Clark, aged twenty-nine, ship-carpenter, married, of regular habits, had enjoyed good health until within the last three months, about which time he complained much of thirst, frequent calls to pass urine, general debility, and loss of flesh. These symptoms increased so much towards the close of autumn as to render him unable for his employment.

When I was first consulted by him on the 11th of October last, I found the urine to contain a large quantity of sugar, and its density to be 1044. He complained much of pain in the legs and forearms. The tongue was furred; and his articulation indistinct. He had lost a stone and a half in weight. The pulse was 78. There had been no cough.

He was ordered ten grains of the alkaline phosphate of soda three times a-day, in water, and to live as much as possible on an animal diet.

13th.—Density of the urine 1040. A tea-spoonful of rennet to be taken after each meal.

16th.—He felt a little easier, and the thirst was not quite so urgent. The density of the urine was 1034. He was directed to continue the rennet and phosphate of soda.

21st.—He was more cheerful, and returned to work. Specific gravity of the urine 1035. The phosphate of soda was omitted.

25th.—Within the previous twenty-fours he passed 252 ounces of urine, and used 196 ounces of liquids, half a quartern loaf, about two pounds of meat, three eggs, and a boiled cabbage. He perspires occasionally. Frequently troubled with heartburn. Urine 1030 in density. He has gained two pounds in weight.

30th.—He feels stronger in the arms and chest, and the pains in the legs are entirely gone; but there still remains a slight œdematous swelling of the ankle. Urine 1034 in density.

November 5th.—For the last twenty-four hours he has passed 108 ounces of urine. The thirst now not nearly so troublesome; the swelling at the ankle is gone; he feels stronger for his work; the tongue is clean; the pulse 76; the appetite good; the urine 1030 in density. He was now directed to discontinue the use of wheaten bread altogether, and, as a substitute, to have bread made

with six eggs and four ounces of butter, kneaded into a loaf with common bran, and baked in a slow oven.

10th.—Within the last twelve hours he has only had three calls to pass urine. He has added four pounds to his weight since he was last weighed. He feels much stronger. The density of the urine is 1030; the sugar is greatly diminished, as shown by the tests; but there is a considerable quantity of lactic acid.

December 24th.—Soon after the last report, I directed him to use chlorine inhalation. I was led to make use of this remedy by the following circumstance. Conversing one day with John Dryburgh, he happened to remark, that, when he had occasion to go into the chlorine-room during his illness when his thirst was urgent, a free flow of saliva took place, which immediately afforded him relief. Under the impression that the chlorine might both act on the sugar circulating in the blood, and also in some way on the nervous system, I was induced to try it in Clark's case.

For ten days or a fortnight before using it, this man passed urine varying in density from 1030 to 1040. In a fortnight after first using it, the density fell to 1020, and every trace of sugar disappeared. During the last four weeks the quantity has never exceeded fifty-six ounces in twenty-four hours, and the density has been pretty steady between 1018 and 1020; nor have I ever been able to observe any sugar in it. Every symptom of the disease has now left him; he has regained his former weight; and he feels strong, and as able for his work as ever he was. His diet, however, has not yet been changed; but I have no doubt, judging from John Dryburgh's case, that, by and by, he may with safety return to his usual way of living.

CASE III.—I have now (December 24) a third case under my care, who has been ill for eight months. He had been treated in the usual way by several medical men, but without success. At first he made seventeen pounds of urine of the density 1037. Now the quantity is four pounds, and the density 1028. He has used both rennet and chlorine. Before commencing the treatment, his sight was much impaired; but it is now quite restored, and he has almost regained his original weight.

The issue of this case will be reported afterwards.

Remarks.—I was led to administer rennet, from knowing the effect which it has upon a solution of sugar. I reasoned thus:—If out of the body it converts a solution of sugar into lactic acid, it may have a similar effect upon a solution of sugar within the body; and bearing in mind that lactic acid is found in the juice of flesh, and, according to Liebig, is a supporter of the respiratory process, I considered that, if sugar formed in the body of a diabetic patient could be converted by the rennet into lactic acid, it would be burned in the lungs; and that if a larger quan-

tity was formed than could be consumed in this way, that portion would be excreted by the kidneys. In this I have not been mistaken; for lactic acid was more than once detected in the urine of the first two patients.

It is necessary that the rennet be recently prepared, and a teaspoonful capable of coagulating a pint of milk in five minutes; as it not unfrequently happens that, by being kept for some time, it becomes decomposed. Its quality should therefore on all occasions be tested with milk before being used as a medicine.

[*Note by the Editor.*—At our suggestion, Dr Gray postponed the publication of the first of these cases until further trial was made of the remedy he has recommended. The results of his practice, however, must be no longer postponed. We may add, that we have employed rennet, and learned of its employment in several cases, in which it has not seemed of service. The particulars are scarcely ripe for publication. But the results show that the remedy is probably not applicable in all cases and circumstances.]

Part Second.

REVIEWS.

A Treatise on Auscultation and Percussion. By Dr JOSEPH SKODA. Translated from the Fourth German Edition, by W. O. MARKHAM, M.D. London, 1853. Post-8vo. Pp. 346.

WE most cordially welcome this addition to our medical literature; for although the list of works on the subject of the physical signs of chest diseases, already available to the English student, is a large one, we question if, with the exception of the translations of Laennec, it contains another treatise of so original and suggestive a character. It is not our purpose to point out here the peculiarities of Skoda's doctrines. To the more prominent of these we have frequently had occasion to direct our reader's attention. Our object at present is rather to examine how Dr Markham has executed his task as a translator; and having been long familiar with the original, and knowing those points where the obscurity of the style was likely to perplex the English reader, we do not hesitate to affirm that he has succeeded in presenting to the medical public a faithful and elegant English version of the German treatise.

The nomenclature adopted both by author and translator may perhaps be objected to by some readers, and will perhaps never find favour with those who have a hankering after *pathognomonic* signs. But, in our estimation, the greatest merit of the work is the extremely logical and decided mode in which it disposes of these mischievous fallacies. Professing to write upon physical diagnosis, Skoda draws from physical signs only physical conclusions, not a complete diagnosis. He consequently makes frequent use of the term (*unbestimmt*) indeterminate or indefinite,—a phrase applied to resonance of the voice, to respiratory murmurs, and to râles, from which *per se* no trustworthy conclusion can be drawn as to the state of the organs of respiration. The reader must not suppose that, from these "indeterminate" signs, when taken in conjunction with other symptoms, no indications can be drawn. He will find that they are, for the most part, sounds which may be produced in various ways, regarding whose physical mode of production the ear affords no certain information, but for which the careful physician may often assign the true cause, by consulting the whole history and symptoms of the case under observation. No one can have seen much of hospital practice without meeting with some such incident as the following. Several inexperienced observers successively examine a patient's back. One hears an "indeterminate" sound, which he calls crepitation; another terms it a bronchial râle; a third, subcrepitant râle; a fourth, muco-crepitant râle; a fifth, friction sound. In short, "quot homines tot sententiae;" and in these circumstances a patient is, we believe, in considerable jeopardy, unless his treatment be directed by a deaf man, or by one whose practice is not wholly guided by his ears, For the advocate of crepitation thinks of pneumonia, and thirsts for blood; he of the bronchial râle contemplates ipecacuan, antimony, and opium; the sub-crepitating and muco-crepitating sounds incline their respective adherents to vote for œdema pulmonum phthisis, and a blister; the fifth observer recognises pleurisy, and a necessity for leeches, calomel, and opium. Any one of these gentlemen may be right, and yet they all may be wrong. Nay, wrong they all indubitably are, if they presume to found a diagnosis upon a single sign; and woe to the unhappy patient if an equivocal sign is to guide the treatment. If the five observers were all duly impressed with the idea, that there may be sounds which cannot be referred at once to their physical source, they would seek to interpret the mode of the production of the sound heard by reviewing a variety of other circumstances in the history and condition of their patient, and might then rationally proceed to diagnosis and treatment. In short, they would regard the problematical sound as "indeterminate."

It must be admitted that the term "consonating," which occurs in various chapters of the work before us, is not free from objection, and involves the admission of a theory. Dr Markham is quite sensible of this blemish in a nomenclature otherwise remarkable for its

simplicity and practical character, and anticipates criticism in the following passage of his preface.

"I do not intend to enter here into any examination of the particular doctrines which Skoda holds; they must be studied *in extenso*, before they can be fairly appreciated. I am sure that a partial consideration of them has led more than one of his critics, and consequently those readers who gain their knowledge of them through such critics, into an erroneous interpretation of their real meaning. If obscurity attaches itself to isolated portions of an author's reasonings on any particular subject (and, in some rare instances, this fault may perhaps be objected to Skoda), it is evident that the reader should endeavour to seek his real meaning in a fair consideration of the whole of the argument, and not by partial views of it.

"In making this remark, I would particularly refer to Skoda's explanation of bronchophony, by the theory of consonance,—a subject in connection with which his name is most generally associated in this country. I will here give a short sketch of this theory, and of the arguments by which Skoda supports it; for its full comprehension, I must beg the reader to study the author's own account of it.

". . . . The voice passes into the parenchyma of the lungs through the medium of the air in the trachea and the bronchial tubes, and is not propagated along their walls; it traverses healthy, as readily as it does hepatized, lung, and even somewhat more readily; consequently, bronchophony does not depend upon an increase of the sound-conducting power of consolidated pulmonary tissue; moreover, when the lung is consolidated, the thoracic voice increases and diminishes in force, without any concurrent change taking place in the condition of the lung. This variation in its strength evidently results from the circumstance of the bronchial tubes being at one moment blocked up by mucus, etc., and at another freed therefrom by the cough and expectoration, etc.; if the bronchophony depended upon conduction of sound, it would be a matter of indifference whether the tubes contained air or fluids. It must not be forgotten, that, according to the ordinary laws of reflection of sound, the more solid the parenchyma, the more difficult does the passage of the sound from the air into it become.

"That the air in the mouth and nasal cavities consonates with sounds formed in the larynx, is proved by the fact of the changes which the voice undergoes through opening and closing the mouth and nose, whilst the condition of the larynx remains unaltered; just in the same way does the air in the trachea and bronchial tubes consonate with the laryngeal sounds. Now, air consonates only in a confined space, and the force of the consonance depends upon the form and size of the space, and upon the nature of the walls forming it. The more solid the walls, the more completely will the sound be reflected, and the more forcible the consonance. The cause of the loud voice produced by a speaking trumpet is well known. But the air will consonate with certain sounds only. In the trachea and bronchial tubes, it becomes consonant with the laryngeal voice, in so far as their walls have a like or an analogous character to the walls of the larynx, of the mouth, and of the nose. Within the cartilaginous walls of the trachea and the bronchial trunks, the voice consonates nearly as forcibly as in the larynx; but as the bronchial tubes divide in the lungs, they lose their cartilaginous character, becoming at last merely membranous in structure, and therefore very ill adapted for consonance. When, therefore, the consonance is increased in these latter tubes, we may be sure either that the membrane forming them has become very dense or cartilaginous, or that the tissue around them is condensed and deprived of air, whereby the sound-reflecting power of the tubes is increased. Of course the communication between the air in the tubes and the air in the larynx must be uninterrupted.

"The walls of a confined space frequently vibrate in unison with sounds excited within it, as do those of an organ-pipe, or of a speaking-trumpet. The

larynx vibrates with every sound, and its vibrations are perceptible at a considerable distance from their point of origin; so, also, must the walls of the bronchial tubes, which are distributed through the parenchyma of the lungs, vibrate when the voice consonates within them; and the vibrations thus excited will extend to the surface of the thorax, passing through several inches of thick fleshy parts, or of fluids, and manifest themselves there as the consonating sounds of the bronchial tubes. . . . .

"Such is a general account of Skoda's theory of the cause of the bronchophonic voice. The critical manner in which it has been brought before the profession, has certainly not much aided in its propagation; and I suppose, with the great majority of physicians amongst us, the theory in question is looked upon as little more than a piece of German mysticism. The reason of this, in my opinion, is, that the subject has come before them *in partibus*, and not as a whole. Whatever may be the ultimate fate of this theory, one thing may be very fairly predicted to result from Skoda's enunciation of it, and from a consideration of the experiments and reasonings on which he bases it, and that is, a recognition of the fact, that Laennec's explanation of bronchophony, by the increased sound-conducting power of consolidated pulmonary tissue, is not true, at least in every case."—*Translator's Preface*, pp. iii–vi.

From this specimen our readers may judge of the style of the work before us. We can confidently recommend it to the student already familiar with the work of Laennec, as a safe guide and monitor in his clinical studies; and to the practitioner, unable to grapple with the German original, as a very excellent translation of an important treatise too little read and understood in this country.

Dr Markham has acted modestly and wisely in presenting the whole of Skoda's work in the very words of its author, without attempting condensation, expansion, or annotation. He says,—

"The work throughout is of a suggestive character, and in such a point of view cannot, I believe, fail to be of service to the thoughtful student. It certainly is not to be regarded merely as a hand-book of details. In this respect it might perhaps be considered as somewhat defective; for there are certain subjects in it which have been but lightly touched upon, and are meagre in description. I might instance the chapter on percussion of the abdominal organs, and the account of aneurisms. I once had an idea of attempting to fill up what I believed was wanting, with the hope of rendering the book more complete for the hands of the student; but upon reflection, I have thought it better that the work should retain its original form. Its deficiencies may be readily supplied from numerous other sources; and further additions of the kind referred to might perhaps only detract from the character and peculiar value of it.

"Neither have I thought it advisable to omit any of the controversial portions of the work. These may here and there seem over long, perhaps even wearisome, but to have passed them over would hardly have been doing justice to their author."—*Translator's Preface*, p. vii.

The translator's preface is well written, and contains a distinct summary of the chief merits and peculiarities of Skoda's treatise. The volume is handsomely got up, moderate in price, and well worthy of the attention of every one who professes to wield the stethoscope and pleximeter.

On Diseases of the Liver. By GEORGE BUDD, M.D., F.R.S., etc. Second Edition. London: 1852. Pp. 486.

So long as the functions of the liver remain in obscurity, and the uses of its educts are not accurately determined, so long must our knowledge of its diseases and their treatment be a matter of pure empiricism. Is the bile important as a secretion necessary for digestion, or as an excretion which is eliminated by the lungs? What is its exact chemical composition? Is it formed only in the hepatic cells? Or, if so, how does it enter the excretory ducts? These questions do not admit of a positive answer. A few years ago it was ascertained that one of the biliary principles (taurine) contained a large proportion of sulphur, so that the liver was imagined to be the organ charged with excreting that elementary substance which entered the body mixed with food. Within the last two years it has been shown that the liver secretes sugar as a normal constituent; and that a product, generally supposed peculiar to the vegetable kingdom, and to occur in animals, only as the result of some derangement in digestion, is always present in them, from man down so low in the scale as the mollusca. Besides bile and sugar, the liver secretes fat in large quantities; and it may be asked, How far are these three principles convertible into each other, or how far will one perform the duties of its fellows? But, again, what are these duties? If the liver secretes sugar, are we to suppose that diabetes is neither a renal nor a stomach disease, but one of hepatic origin? New facts are required to solve all these difficulties.

After carefully reading Dr Budd's new edition, we cannot say that any of the important questions we have asked are satisfactorily answered, although it is far from our intention to blame the author on that account. He has not failed, in the introduction, to notice M. Bernard's researches, establishing the hepatic secretion of sugar, but no change in pathology or practice seems as yet to have resulted from this singular discovery. He continues to speak of suppurative, gangrenous, and adhesive inflammations; and under the last head, he discusses cirrhosis. We had hoped that this old-fashioned pathology had been exploded; and fear that we are not likely, by its assistance, to determine those points so necessary for advancing our knowledge of hepatic diseases.

Then, with regard to therapeutics, we are told that mercury, iodine, the salts of soda, muriate of ammonia, and taraxacum, undoubtedly render the liver more active, and in this way increase the secretion of bile. On the other hand, alkalies, especially soda, ether, and turpentine, have been supposed to render the bile thinner, and have, on this account, been at various times recommended as remedies for gall-stones. But it may be asked, Are these statements in any way established? What evidence have we that the first class of remedies really augment the action of the liver in the same way that

diuretics cause an increased flow of urine, or purgatives an extra number of stools? We have long looked upon the wonderful properties ascribed to mercury as in a great measure fabulous. It is considered as a specific for syphilis. It is thought to possess the astonishing power of causing absorption of recent, and some say even chronic, fibrous exudations. Then it is a true cholagogue, and increases the flow of bile. But if we look into the evidence of its powers in these different directions, all is vague and unsatisfactory. Its reputation as an antisyphilitic, though once firmly established, is now nearly upset; and the other two actions which have been attributed to it require renewed and more careful investigation. It certainly has a power of exciting the action of the *salivary* glands; but we know of no more analogy between them and the liver, than between them and the kidney. May we not therefore say of it and the supposed cholagogues, what Dr Budd says of the alkalies, ether, and turpentine,—namely, "They are given empirically, generally with a vague notion of what is amiss; and, according to the chances of individual experience, or the fashion of the day, are rated at one time much above their worth, and at another time, in effect probably of this over-estimate, are altogether discarded."—P. 54.

In fairness to Dr Budd, we must say that the unsatisfactory nature of our knowledge concerning diseases of the liver and their treatment is entirely owing to the difficulties of the subject,—difficulties which can only be overcome by long and laborious researches into the anatomy, physiology, and pathology of the organ. All that is really known, with much judicious reflection on what is conjectural, may be found in this book. It is interspersed with numerous cases, which, if they do not serve at all times to indicate the nature and appropriate treatment of every hepatic disease, will at least inform the practitioner how imperfect is his art, and what enormous labour is required to render this class of disorders as intelligible as those of the heart or of the lungs.

On Rheumatism, Rheumatic Gout, and Sciatica: their Pathology, Symptoms, and Treatment. By HENRY WILLIAM FULLER, M.D., Cantab. London: 1852. 8vo. Pp. 404.

WE have read Dr Fuller's work with much pleasure; it contains a full and clear exposition of our actual knowledge of rheumatic affections; we can safely observe concerning it, what cannot with justice be said of the majority of works which issue from the press, viz., that it is one which is wanted. Both the student and the practitioner are indebted to Dr Fuller; he has collected together and systematised a vast amount of scattered knowledge. Facts also, connected with the history of rheumatism (as, for instance, its hereditary transmission), which have been hitherto overlooked by observers in

general, are here established. Dr Fuller appears, from an early period, to have turned his attention to the study of rheumatic affections, and he has evidently mastered the subject he treats of, his statements have been tested by his experience, and he has every way a just right to come before the public. We wish his work the success which we believe it deserves.

In his first chapter, Dr Fuller strongly warns his readers against the common fallacy of attributing to cold the production of rheumatism. According to his views, the characteristics of this disease spring from a vitiated condition of the circulating fluid; a materies morbi is accumulated in the blood, and awaits but the touch of some passing accident—an exciting cause—to produce those particular manifestations to which the term rheumatism is applied, and which are neither more nor less than nature's efforts to shake off the poison which oppresses her. What is the source of this poison, and how does it corrupt the blood? Rheumatism is a disease transmissible from parent to offspring, and many circumstances point to its constitutional origin; we speak of a rheumatic diathesis. If the disease depended upon a poison introduced from without, it would stimulate other disorders so produced; it would not occur sporadically; its course would not be varying and uncertain; one attack would confer immunity from future attacks, etc. The poison does not come from without; "the materies morbi is generated in the system as a product of malassimilation, a faulty metamorphic origin." What is its nature? Dr Fuller here falls back upon a suggestion first thrown out by Dr Prout, and afterwards taken up by Dr Todd, viz. that malassimilation causes a rapid development of lactic acid, which accumulates in the system by reason of defective cutaneous action. Some noxious and specific element which the skin should excrete is retained in the body.

Now, this theory of the development of a poison in, and of its attempted elimination from, the system by a rheumatic attack, is legitimate enough, and has analogies for its support, but we think Dr Fuller, throughout his work, rests his faith too firmly upon it, and in consequence very frequently bends facts to meet his hypothesis. That "acute rheumatism is a blood disease," as Dr Watson says, few observers in these rampant days of humoralism will be bold enough to deny; but it is surely time enough for us to specify the proximate cause of the disease, and found practical conclusions upon it, when we know what that cause really is; the truth being, that of the essence which creates the disease, and of its manner of working, we know absolutely nothing positive, when we speak of the generation of lactic acid through malassimilation; and of nature's efforts to rid herself thereof, we must not forget that we are using the language of pure hypothesis. We may mention here, that Dr Fuller nowhere speaks of the pathology of the blood in this disease; and we are surprised, that when he points out the many analogies

which exist between gout and rheumatism, he does not refer to the striking fact discovered by Dr Garrod (*Med. Chir. Trans.* vol. xxxi.), viz. that uric acid exists in the blood of the gouty and not of the rheumatic; a fact which Dr Garrod proposes as a sign indicative of a positive distinction between the two diseases.

The hereditary nature of rheumatism can scarcely be denied, and in this sense it is manifestly a constitutional disease; but surely there is nothing in this fact which militates against the probability of the disease manifesting its presence in the body independent of any constitutional peculiarity; phthisis, and scrofula, and insanity, are hereditary diseases, beyond dispute, but are we to suppose that they can never afflict any persons, placed under the influence of whatever favourably exciting causes, except some latent and pre-existing constitutional aptitude for their development lies intertwined with the sources of life in such persons? Half a dozen of persons are exposed to marsh miasmata, three are struck by ague, the rest escape, but on a subsequent exposure, the three who previously escaped are now overtaken by the fever: is it fair to speak of any constitutional tendency to the disease here? Is it not much more rational, in the present state of our knowledge, to be contented with the simple supposition, *faute de mieux*, that the body is at one time more liable to fall into disease than at another.

It is possible that we may not have seized upon the exact sense attached by Dr Fuller to the word constitutional; we certainly have gathered this proposition from his work, viz., that rheumatism is never developed except there be in the body affected some pre-existing original defect; if, by the term, Dr Fuller means no more than that the disease is a blood disease, and affects the whole system, in that sense we agree with him; but the former position we cannot admit, because it is not proven, and because analogies are against it.

The hereditary nature of rheumatism has been hitherto generally disregarded, and still is often called in question. Dr Fuller places the fact beyond dispute; and what will appear to many very surprising, he shows statistically that this disease is more markedly transmissible from parent to offspring than even consumption and insanity. The records of the Consumptive Hospital give 24 as the per centage of cases in which a hereditary tendency was ascertained —in insanity it has been traced in 13 per cent., whilst in acute rheumatism, in 246 cases observed by Dr Fuller at St George's Hospital, it was traced in nearly 29 per cent.

Acute rheumatism appears, for the most part, as a disease of adult life. Of 286 cases, 241 occurred in persons between the age of 15 and 40. It appears in nearly equal proportion between men and women. One attack does not predispose the subject of it to a recurrence of the disease, as generally believed; the paroxysm is an effort of nature to rid the system of a poison; sure it is, that those on whom it once seizes, rarely fail to be revisited by its violence, but this proves no more than that in such persons there exists a

tendency to the formation of the poison, and a proneness to suffer from it. The chronic or acute form of the disease is determined by the *quantity*, not the *quality* of the poison—*this* is ever the same. The predisposing causes are well known. Dr Fuller shows that the disease prevails most during seasons, and in climates, remarkable for damp and variable weather, not during the most inclement weather, nor in the coldest climates. It is rare within the tropics, and almost unknown at the poles. How is this? In the one case, the skin's action is great, and the poison, if generated, never accumulates; in the other, the mode of living among the inhabitants is simple, and the system vigorous, and the form of malassimilation whereby the poison is generated, little likely to occur, and so the essential cause of the disease is absent. Surely Dr Fuller here strains facts to make them fit this theory of the lactic acid generation and elimination. The inhabitants of the polar regions are vigorous of body, and simple in diet, and *therefore* free from rheumatic disorders; but is an athletic negro or a warlike Caffre less vigorous than a tiny Laplander or a smoke-dried Esquimaux? or is the corn diet of the tropics less simple, or more likely to be productive of malassimilation, than the periodical gorgings and fastings of the polar people? Again, is it a fact that the tropical dwellers sweat out the rheumatic element, that this continually oozes from their skins, and cannot accumulate so as to produce disease? We very much doubt this perspiring nature of their skin; travellers continually tell us that tropical natives, as in the interior of Africa, anoint their bodies to prevent the skin cracking through dryness; and as for the sea-coast dwellers within the tropics, who are more exposed than they to the heavy damps and cold dews, and rapid variations of temperature, which Dr Fuller tells us are the most productive exciting causes of rheumatism? Surely the argument here is illogical. We might add, that experienced practitioners have told us that chronic rheumatism is a very common complaint among the natives of India.

No texture and no organ of the body appears exempt from a liability to rheumatic invasion, but the white fibrous tissue is the seat by predilection of the disease.

Dr Fuller divides the affection into four classes:—1st, Acute rheumatism; 2d, Rheumatic gout; 3d, Chronic rheumatism; and 4th, Rheumatic fever.

In his fourth chapter, Dr Fuller gives a very complete description of the symptoms of acute rheumatism. He lays great stress, more than is generally done, on the importance of the free perspirations which occur in its progress. "Arrest them," he says, "and you aggravate the disease; you accumulate the poisoning element in the blood." He admits, however, that they are injurious in cachectic subjects, and in the latter stages of protracted cases; here they are the effects of debility—there, essential to the patient's safety.

The duration of the disease, according to our author's observation of 246 cases admitted into St George's Hospital, is about five weeks,

and this seems to be about the term generally assigned to it; but he believes, that by judicious treatment, an uncomplicated case may be reduced to 10 or 14 days' duration. Dr Fuller's treatment appears to be that rational plan which we believe is most generally adopted now-a-days, since specifics have been exploded, viz., rare use of the lancet, mild calomel purgatives, opiates, alkaline medicines, promotion, if necessary, of cutaneous perspiration, local alkaline and opiate applications. No exclusive line of treatment can be laid down—each case presents particular indications; the value of different remedies is fully and well discussed; the latest of all in the field, like many others that have gone before it—lemon juice—appears, when weighed in the balance of experience, unworthy of the panegyrics which have been bestowed upon it.

The importance attaching to rheumatic affections of the heart, is pretty plainly indicated by the fact, that their history and treatment occupy rather more than one-third of Dr Fuller's work; indeed, from the very general association of cardiac disease with rheumatism, the disorders of this organ seem naturally to fall under the head of this disorder.

Our space forbids us, on this occasion, from following Dr Fuller into this all-important and interesting field of research, but we hope to have an opportunity of returning to it at some future period. We strongly recommend a careful perusal of this portion of the work to the student—it contains a great amount of well-arranged information. On one point we may observe, that we do not think our experience agrees with that of Dr Fuller's, viz., that the more severe the general rheumatic affection, the more liable is the heart to be invaded. Our opinion rather coincides with that of Dr Watson; certainly some of the most "fondroyant" cases of cardiac affections which have come under our notice, have been those where the antecedent general symptoms were slight.

Dr Fuller's treatment of acute rheumatism, as detailed in Chapter X., we consider very judicious. In his hands the method he recommends appears to have been eminently successful in preventing the supervention of cardiac disorders. Of thirty-nine cases treated by him, the heart was affected in seven, but of these seven the disease existed in five before they came under Dr F.'s charge; and in one case, heart disease appeared on the second day, and therefore might have existed previously. Putting these cases aside, then, it would appear that in thirty-three cases treated by Dr F., cardiac disease occurred but in one. We can only say of this, that we hope Dr F.'s future treatment may produce as happy results. His success is assuredly far beyond what our less fortunate experience would lead us to expect.

Before leaving this subject, we would call the attention of our readers to a fact which is not generally noticed by writers, viz., that the heart appears not unfrequently to be the primary seat of the rheumatic affection in cases where this affection can claim a distinctly

hereditary character. In such, the disorder of the heart manifests itself in childhood and in early youth. We have had several opportunities of observing illustrations of this fact. We have under our observation at the present moment two families. In one, two of the children and the father have organic disease of the heart. The father had several severe rheumatic attacks before the birth of these children. His heart is not seriously hindered in its functions. A constant systolic bruit is heard over the aortic valves, but their closure seems perfect. In the children, about 14 and 15 years of age respectively, the heart is so impaired as to produce the severest consequences of heart disease, which threaten early death. One of these children appears to have suffered some pains in the ankles as the first manifestations of illness, but the other never had any external symptoms of rheumatism whatever. Her first complaints of illness were short breath and swellings, the products of her heart disease. In the other family, the father has for years been a martyr to rheumatism. His child, about 2½ years old, has organic disease of the heart. The mother fancies it has had slight pains in its limbs.

We believe that if the attention of observers was more directed to the facts we refer to, the number of so-called idiopathic diseases of the heart would be still further diminished.

The disorders created in the cerebral functions by rheumatism, Dr Fuller attributes to the poisonous element of which he speaks so much.[1] These disorders are rare; they occur for the most part in the weak and the irritable, in those whose blood is vitiated or impoverished. The wild delirium is not the product of inflammation; venesection is here inadmissible, opium and diffusible stimuli of the highest advantage.

Pulmonary complications were observed in 41 of 246 cases of acute rheumatism observed at St George's Hospital, *i.e.*, in 1 of every 6 cases. "Pleurisy or pneumonia or pleuro-pneumonia were present in almost all the fatal cases of carditis." In the treatment of these complications, their special nature must never be lost sight of. Dr F. does well by insisting on this point; for we fear that it is very often forgotten or disregarded.

The remaining chapters of Dr F.'s book, viz., on rheumatic gout, chronic rheumatism, and neuralgia, our space warns us that we must pass by. We cannot, however, conclude this notice without stating our opinion, that the profession has received from the hands of Dr Fuller a very valuable addition to medical literature. Productions of this character do credit to the rising generation of the followers of Hippocrates; and while writers of such a class are thus

[1] The interesting communication of Dr Kirke's (Med. Chir. Trans., vol. 35), respecting the detachment of fibrinous deposits from the interior of the heart, is worthy of being alluded to in reference to this matter.

honourably busied in the fostering of our noble art, it surely has little to fear that its final triumph will be disturbed by attacks from without, or by the treachery and defection of those who are incapable of appreciating and adorning it.

Du Diabète Sucré, ou Glucosurie, et son Traitement Hygiénique. Par M. BOUCHARDAT, Membre de l'Académie Nationale de Médecine, Pharmacien en Chef de l'Hôtel-Dieu, etc. Extrait du Tome 16ième, des Mémoires de l'Académie Nationale de Médecine. Paris, 1852. Pp. 144 quarto.

THE object of this dissertation is to establish as principles,—that *Diabetes mellitus* is a depraved state of the digestive functions,—that it is a curable disease,—and that the treatment depends almost entirely on correcting the depraved state of digestion. There is nothing new in any of these propositions. The first, distinctly announced in 1772 by Dr Dobson, was settled on a firm foundation by Dr Macgregor in 1836. Most late writers, in opposition to the views of Dr Prout, have admitted the second; and the chief source of surprise, indeed, has been that diabetes should not be cured more frequently than it is. As for the third and last, the chemico-physiological details of the treatment have been greatly improved by recent researches into the constitution of the food of man; but since the days of Dr Rollo, although a few sanguine discoverers have from time to time proclaimed each his specific remedy, and some credulous lovers of specifics have for a time believed them, the medical profession at large have put no faith in anything except a well-regulated diet. Hence the inflated paragraph with which M. Bouchardat introduces his treatise may be very apt to engender in his reader a distrust of what is to follow.

"When," says he, "after several years of research, I read before the Academy of Sciences, in 1838, my first memoir on diabetes mellitus, the state of the subject might be summed up in the words of Dr Prout,—the man who, in England, had seen most, and written best, of this cruel malady: diabetes mellitus is a disease incurable and always fatal.

* * * * * *

"A cure, that *ultima ratio*, that end of all therapeutics, had not been attained."—P. 1.

Notwithstanding this flourish of trumpets,—which, indeed, is nothing more than the manifestation of an amiable failing of the nation to which the writer belongs,—our readers may rest assured that there is a mass of interesting information in the various memoirs which M. Bouchardat has produced on the same subject, both then, and since in his *Annuaire*, or Yearly Retrospect

of Therapeutics, and which he has amalgamated and condensed into the dissertation now before us.

The fundamental facts propounded by M. Bouchardat are, that the quantity of sugar in diabetes is always directly proportional to the quantity of amylaceous and saccharine aliment taken in twenty-four hours,—and that by almost entirely withdrawing these alimentary principles, the urine gradually returns to its natural composition and quantity [pp. 5 and 6]. He admits, indeed, that since he first advanced these propositions, the discovery by Scherer of the principle Inosite in flesh, of which it constitutes a minute proportion, and the more important discovery by M. Bernard of the development of sugar in the portal blood by the agency of the liver, independently of the supply of saccharo-amylaceous nutriment, may render physiologically possible the presence of a small proportion of sugar in the urine, apart from the introduction of sugar or starch into the alimentary canal. But still he adheres to the great fundamental principle, that diabetes depends substantially on the saccharification of saccharo-amylaceous food.

How ardently must all desire to see that doctrine satisfactorily established! For, with the knowledge now possessed of the constitution of the human food, a cure ought to be attainable in almost every case. But facts will not bend to theories. Admitting that a great proportion of the sugar which appears in the urine of diabetes is derived from the starch and sugar of the food, we deny that it is all to be accounted for in this way. The very statement of M. Bouchardat, that on withdrawing these alimentary principles from the food, the urine returns to its natural standard only "peu à peu," or very gradually, ought to have been to him a proof that in the interval sugar is formed from some other source. And we can assure him, that whatever may be the alleged universality of the fact in France,—that sugar does at length disappear from the urine under such exclusion of starch and sugar from the food,—the rule is, we grieve to say, very far from being so invariable in this country: for, in repeated instances, we have found the urine steadily from 1035 to 1040 in density, and abounding in sugar, although for several weeks not an atom of sugar or starch had been swallowed.

Still it is a most important principle that a large proportion of the sugar of diabetes, in cases not subjected to proper treatment, is derived from saccharo-amylaceous aliments; that the one diminishes as the other is withdrawn; and that, in many instances, the disease is thus brought to a stand-still, and some diabetics are even radically cured. But this modified fact was well enough known before M. Bouchardat attempted to extend it.

Dr Macgregor of Glasgow, in 1836, was the first who clearly ascertained that sugar is formed in the digestion of the food of diabetics, by proving its existence in the contents of the stomach and intestines. But he did not ascertain in what way the production of this principle was effected. The blank has been supplied by M.

Bouchardat; who says he has constantly found in the secretions of the stomach a principle identical, or nearly so, in all its physical and chemical properties, with the Diastase of barley and other grains, by means of which their starch is converted into sugar in the process of malting. By administering ipecacuan to a diabetic patient, while fasting, he obtains a transparent, colourless, viscous liquid, which, when filtered and agitated with twenty-five times its volume of rectified spirit, gives a white flaky deposit, soluble in water, and again separable from this state of solution by means of alcohol, and in a purer condition. When thus purified, and dried in thin layers on glass at a temperature of about 110° Fahr., a white amorphous substance is obtained, which is soluble in water or weak spirit, insoluble in alchohol, neither acid nor alkaline, nor precipitable from an aqueous solution by diacetate of lead, without action on albumen, gluten, cane-sugar, inulin, or strongly organised cellulose, but capable of converting 2000 times its weight of starch into grape-sugar diffused in seven times its weight of water, and kept at a temperature of 158° Fahr. In every other property, so far as this substance was examined, it corresponded with ordinary diastase. In the empty stomachs of healthy persons, M. Bouchardat has never succeeded in detecting it.

Sugar thus fairly formed in the organs of digestion, it may be presumed to enter the blood. Here, accordingly, it has been often found by prior experimentalists. The author not only confirms their observations, but likewise accounts for some at least of the unsuccessful attempts of others to find it. For, in the first place, he could not discover it when the patient had been fasting for some time; and, secondly, it could not be detected if the blood was allowed to stand for four-and-twenty hours, as it appeared to be converted into lactic acid. It deserves mention also, for the sake of those who may be induced to repeat these inquiries, that it cannot be detected by Trommherz's test, or by the polarization test of Biot, but only by the process of fermentation with yeast. In respect of the other qualities of the blood, the author has not observed any positive deviation from the healthy state. He denies the statement of those who allege that the serum is less alkaline than it ought to be. He has found the whole blood impoverished in blood-globules and fibrin when the disease had subsisted long; but in other circumstances, the proportion of these principles was much the same as in the healthy.

M. Bouchardat has entered into considerable details on the question as to the pathological essence of Diabetes; and in this part of the inquiry he attempts in particular to attach some definite bearing to the late interesting researches of Bernard on the formation of sugar in the blood through the agency of the liver, and on the secretion of sugar by the kidneys when the fourth ventricle of the brain is irritated. But, as he has not arrived at any well-adjusted views on these points, we must exclude this part of his investigations from the

present sketch, and pass on to his principal object, the methodical treatment of the disease.

The first and most essential part of the treatment is the regulation of the food. Of course all articles containing much starch must be excluded. These are too well known to require specification here. Sugar is equally to be forbidden, even cane-sugar, which is converted into grape-sugar in the diabetic stomach. Milk, too, he considers inadmissible, because sugar of milk is, in his opinion, convertible into grape-sugar in the stomach of diabetic patients. This is not clear. Diastase does not effect this conversion. Neither does that modification of it which the author found in the diabetic stomach when empty of food. We have found, too, that the addition of twenty-four or thirty-two ounces of milk daily to other articles of food did not appear to increase the daily discharge of sugar.

All kinds of flesh are admissible in the treatment. That of wild animals has seemed to him the best, also that of insectivorous birds; and he has no objection to the flesh of carnivorous animals, if the patient will eat it; so that sometimes he has allowed a diet of cats and foxes to those who were too poor to afford to dine upon venison, wild-boar, and woodcock. Fish of all sorts, including shell-fish, are excellent; and so are eggs. So too is cream, and cheese also; but milk is forbidden, because an English pint yields about three-quarters of an ounce of sugar in the urine.

Among vegetables, those which are allowable are spinage, chicory, sorrel, lettuce, asparagus, artichokes, Brussels sprouts, cabbage, green kidney-beans. Instead of bread, loaves made chiefly with gluten were proposed by the author in 1841, and are of great service. They are made by the following formula, which was determined after many trials by a baker of repute in Paris. As many practitioners have made inquiries how this bread is prepared, we present the recipe.

"One hundred parts of flour are carefully mixed with forty of water, and left at rest for half an hour in summer and two hours in winter. This dough is placed over a bucket in a sieve made with wire-cloth, No. 120, strengthened with a drainer, No. 15, and surrounded with a rim about six inches in height. A lump of ten pounds may be worked at one time by projecting upon it a stream of water through a tube perforated at the end with numerous holes, and kneading the mass at first gently, but afterwards more and more strongly as the gluten becomes thready, until the water ceases to run away white. The gluten thus obtained amounts to about a fourth of French flour, and a third of Sicilian or Barbary flour.

"The gluten should be converted into bread as quickly as possible; for in a few hours it undergoes alteration, and panifies ill. It is first to be carefully mixed with a fifth of its weight of fine flour, a little salt, and some yeast. It is then allowed to ferment, and placed in the oven at a moderate heat, until sufficiently dried. Bread thus prepared is palatable. According to the taste of the patient, the dough may be mingled with eggs, or cheese, butter, or cream.

* * * * * * *

"Bread may also be made from gluten by incorporating dry wheat flour with gluten in the moist state, drying and pulverising the mass, then mixing this

powder with more moist gluten, and drying and pulverising again, and so on until the powder contains at least sixty per cent. of gluten. A portion is then granulated and roasted gently, and mixed with the rest; for in this way the dough is prevented from becoming too tough, so that panification takes place much more easily. In order to make this gluten-flour into bread, four parts are mixed with one of yeast, kneaded very diligently with the necessary water, and allowed to ferment for twelve hours at a temperature of 60° to 68°. This leaven is then mingled with twelve additional parts of gluten-flour, carefully kneaded, as before with water, and salted to the patient's taste; and after eight or twelve hours of fermentation, it is baked in the oven in the usual way. Bread thus made looks and tastes well, if skilfully prepared.—Eggs, cream, or butter may be mingled with the dough; and instead of salt, the citrate of soda may be used, as it is decomposed in the blood.

"The gluten-flour keeps remarkably well. It may be used for making various cakes, and also for porridge, or for thickening soups."—Pp. 26-28.

Another substitute for bread, more used in this country, is made with bran, and consists of equal parts of well-washed bran, eggs, and butter, well mixed and duly baked in cakes about a third of an inch in thickness. We find that diabetics generally prefer this even to the best gluten-bread, especially if the bran be ground fine in a steel mill. A quality superior to both might be obtained by adding gluten to the bran-bread mixture.

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The drink of diabetics may be at once greatly reduced, as soon as the food is regulated as above; because the thirst is much abated. Wine constitutes an important part of the drink in the author's system of cure, because he imagines he has established that the alcohol of wine, by undergoing combustion in the lungs, supplies the place of starch in the respiratory process. He therefore enjoins a bottle of Bordeaux daily in all instances, sometimes two, and, in the instance of strong men who work hard, so much even as three or four bottles,—care being always taken to increase the quantity gradually, so as to avoid intoxication. Another useful drink is soda-water with a tenth of wine. Fat infusions of meat are also serviceable. Coffee without sugar also answers well. Selzer or Vichy water, tempered with brandy or rum, soda-water similarly improved, may also be added to the list, and increase sufficiently its variety.

Diabetics should always masticate their food very diligently, and their meals must not be large, but rather frequent than large, if the appetite continue too strong. It is remarkable, however, as we have almost invariably observed, that the appetite ceases to be ravenous, when the food is properly adjusted.

The patient should be always warmly clothed in winter. He ought to take as much exercise as he can; for under exercise less sugar is formed from the articles of food capable of yielding it, such as the gluten bread. If tartrates and citrates too are given, as the author recommends, they are converted largely into carbonates, if there be much exercise, but pass chiefly unchanged under rest,—showing that exercise operates powerfully in modifying the changes occurring during digestion or in the blood. The warm-bath and vapour-bath have appeared to M. Bouchardat less frequently service-

able than has been generally thought; river-bathing, if combined with the exercise of swimming, he thinks more advantageous, but hydropathy a very dubious measure.

As for medicinal treatment, he is attached to the carbonate of ammonia as a stimulant diaphoretic, and gives from fifteen to forty-five grains thrice a-day in solution or bolus. The other alkalies he rather condemns. The water of Vichy has seemed to him of use, but not the bicarbonate of soda; which, on the contrary, M. Mialhe has declared so important a remedy that the cure may be trusted to it alone. Opiates, especially in the form of Dover's powder, promote diaphoresis, allay thirst, and subdue nervous excitability. Chalybeates are available when there is anæmia, and are best given in the form of metallic iron, carbonate of iron, or iodide of iron. Cinchona-bark, quina, and simple bitters, restore the powers of the stomach, if enfeebled, besides acting as general tonics. Common salt, instead of increasing, diminishes the patient's thirst. Sometimes iodide of potassium or iron has seemed useful. Antiscorbutics are occasionally indicated as necessary by the state of the gums; in which case cresses may be freely taken with advantage. Constipation must be carefully remedied by adequate aperients. Substances which retard the decomposing influence of diastase may be expected to prove useful, such as the caustic alkalies, the caustic earths, the mineral acids, oxalic acid, alum, and the vegetable astringents. It is worthy of remark accordingly, that all these substances have been commended by one practitioner or another in previous times, when they were used empirically, and before their action on the saccharification of starch was known. M. Bouchardat, however, doubts the utility of all of them, and has not been able to remark any such influence exerted when diabetic patients were using amylaceous food.

These are the leading articles of treatment. In order to follow the progress of cure, he insists properly with great earnestness on following the condition of the urine by frequent experiment. The daily amount of sugar should be ascertained frequently. For that purpose he prefers the polarising apparatus. This however is costly for ordinary practice: and we really think, notwithstanding some insignificant objections to the simple test of ebullition with solution of potash, that, with a very little experience in applying it, and the constant use of a tube of the same diameter, the colour produced is a sufficiently satisfactory test of the progress of things for all ordinary practical purposes.

The most remarkable circumstance in the work of M. Bouchardat, is the familiarity with which he speaks of cures being effected. If his word is to be taken, Diabetes is a curable disease in a majority of cases, even when of some continuance; and other French writers are of the same mind. The opinion of physicians in this country is different, even of those who have long followed a plan of treatment essentially the same, or who have lately adopted all M. Bouchardat's principles and details of treatment. If French practitioners be so

successful as they allege, then assuredly Diabetes is a more inveterate disease with us. In common with others, we have often seen it arrested for many years, and sometimes have witnessed a thorough cure. But in general the cases go wrong in the end, and life is shortened either by some secondary inflammatory disorder, or by the gradual supervention of tubercular phthisis, or simply by the progressive wearing out of the powers of life by non-assimilation of the food.

We apprehend, too, that the disease is more frequent in France. The physicians of Paris seem to encounter it very often. The proper treatment has even become familiar to the general population. We are assured that in the Parisian hotels, whenever gluten-bread is asked for, it is provided with an air of intelligence, and along with it all the other appropriate articles of food for diabetic visitors. The inference is inevitable, that Diabetes must either be uncommonly frequent, or that the term is very liberally interpreted in French medical practice.

Hand-Book of Natural Philosophy and Astronomy. By DIONYSIUS LARDNER, D.C.L. *Second Course: Heat—Common Electricity —Magnetism—Voltaic Electricity.* London: 1852. Pp. 456.

IN a notice of the first volume of this work, we expressed our approval of the plan which the author was pursuing, in condensing and adapting for general use the chief facts and doctrines of physical science. The second volume, which is now before us, embraces the important subjects of heat and electricity. In popularising these branches of natural philosophy, an author is not so constantly embarrassed by the necessity of evading abstruse mathematical details, as when he has to deal with statics, dynamics, or optics; yet he has need for all his powers of illustration, and for that happy talent of interesting his reader, which is essential in order to make his work popular. It appears to us that Dr Lardner has been very successful, especially in the chapters on electrical science, in presenting to the general reader a connected view of facts and theory in an attractive form, without redundancy of detail, and divested of all unnecessary mathematical technicalities. The subjects of astronomy and meteorology have been reserved for a third and concluding volume of the Hand-Book.

Part Third.

CLINICAL REPORTS, LECTURES, ETC.

VALEDICTORY ADDRESS,

DELIVERED AT THE MEETING OF THE EDINBURGH MEDICO-CHIRURGICAL SOCIETY, ON DECEMBER 8TH, 1852.

BY JAMES BEGBIE, ESQ., M.D., PRESIDENT OF THE SOCIETY.

GENTLEMEN,—The time has now arrived when I am called upon to resign into your hands the honourable office which you conferred upon me two years ago; and my last duty in this place is the pleasing one of returning my heartfelt thanks for all the kindness, and all the support, which I have experienced at your hands, during the two sessions in which I have occupied this chair.

The possession of the office I have ever considered a distinguished honour; and I feel confident that the associate whom you will immediately invest with the privileges which I now lay down will regard it in no other light. To be the successors of Thomson, and Abercrombie, and Kellie, and of others also whom to name is but to venerate, is surely no mean distinction; and we may well claim your indulgence if, in attempting to follow their footsteps, and to be guided by their example, we have too often given occasion for the regret, that the genius and talent which gave dignity to the office in former days has not descended to all the more recent occupants of the presidential chair.

Gentlemen, I have alluded to the privileges as well as to the honour of presiding here; and while I thank you for the one, I am also deeply grateful that, in being called to it, you secured to me the enjoyment of the other. The duties of the office have brought with them a rich reward. I have sat here, night after night, an interested listener to the many valuable communications with which we have been favoured, and to the spirited and instructive discussions by which these have been followed. I have been a learner, again and again, and have never had occasion to regret that my presence here has led me to sacrifice advantages, or forego opportunities, which promised to be more precious or valuable than those which I thus enjoyed.

In now closing my term of office, I must crave your indulgence while I attempt to recall to your minds some of the subjects on which we have been engaged throughout its course. It would ill become me, however, to occupy your time with any lengthened retrospective review of the public business of the Society during the period referred to; but I cannot deny myself the pleasure of briefly adverting to some of its more prominent and interesting features, remembering the satisfaction derived, on an occasion like the present, when my esteemed predecessor, Dr Gairdner (whose success I cannot hope to equal), detailed to a willing audience the chief points of interest in a session, which, under his direction, was then brought to a successful close.

In carrying out the great object of our institution—the promotion of professional improvement, and the encouragement of harmony and goodwill

among ourselves—we have been favoured with many communications in the different branches of pathology, of practical medicine, and of surgery; while we have admitted others also, of a more miscellaneous character; all of which deserve your attention, but which your time will not permit me to refer to otherwise than in the form of a passing notice.

In the department of pathology, Dr William Gairdner, early in the first session, read an interesting paper on the favourable terminations of pericarditis, and especially on adhesion of the pericardium, with cases illustrating its secondary effects on the heart. One chief point of interest in this paper consisted in showing, from numerous carefully observed cases, that hypertrophy of the heart, and particularly of the left ventricle, was only intimately connected with adherent pericardium in those cases in which there also resulted disease of the valves of the heart, chiefly the aortic. The statistical data furnished in illustration are very valuable; and the Society, and subsequently the profession, received the communication with marked commendation. On the same evening I had the honour of laying before you some observations on fatty degeneration of the heart, in which were detailed the circumstances connected with the fatal illnesses of Dr Chalmers and Dr Abercrombie, with an account of the morbid appearances found on dissection, showing that, in both, death had resulted from fatty degeneration of the muscular fibre of the heart. Drs Alison, Christison, Maclagan, and Bennett, furnished additional illustrations of this formidable lesion.

Dr Halliday Douglas, early in the same session, favoured the Society with an interesting account of the effects produced by an encephaloid tumour in the upper and front part of the chest; and exhibited the preparation. Dr William Gairdner produced a specimen of emphysema of the lung, with the view of illustrating the doctrine advanced in his memoir on bronchitis, that emphysema is not a primary affection of the lung, but always the result of other lesions. The same author also laid before you a case of aneurism of the thoracic aorta, with observations on the application of tracheotomy in such cases, in which he expressed an opinion favourable to the performance of the operation for the purpose of relieving the dyspnœa, even where the existence of aneurism was clearly diagnosed. Drs Bennett, Spittal, Gillespie, Mackenzie, and Omond, have all contributed, on more than one occasion, to the instruction of the Society in the production of specimens of great pathological interest; and to Mr Spence, also, our thanks are due, for several most interesting preparations in surgical pathology. To the Pathologist of the Royal Infirmary we are also indebted for a valuable notice of the post-mortem examinations conducted by him in that noble institution.

In the department of practical medicine, the subject of puerperal fever, and particularly its manner of propagation, received a full discussion, when the valuable paper of Dr Arneth of Vienna was read by him to the Society. The substance of that communication has already been published; and the experience of Dr Simpson, Dr Moir, and Dr Peddie, as given on the occasion referred to, added greatly to the interest of the meeting. The treatment of typhus fever, by the exhibition of large doses of quinine, as practised by Dr Dundas of Liverpool, engaged the attention of the Society on the reading of a paper by Dr Bennett, whose experience, as well as that of other speakers, was not favourable to the employment of the remedy in such cases. The subject of erysipelas has, more than once, been brought under your notice, particularly during the first session, when Mr Hamilton Bell, and Dr Charles Bell, first introduced to the profession the mode of treatment by the tincture of the muriate of iron,—a remedy which has subsequently been very generally and successfully employed, and in regard to which I had the satisfaction of stating my own experience in a paper read at the close of the last session.

Dr Alexander Wood read a very interesting paper on some of the more important complications of scarlatina, and more especially on the relation sub-

sisting between scarlatina and erysipelas. On this occasion the connection of the two diseases, as evidenced by the albuminuria and pellicular sore throat, common to both, was particularly brought out. The relation of erythema nodosum to rheumatism was illustrated by numerous examples, furnished by Dr Keiller; and the subject of temporary albuminuria, discussed at some length by Dr Warburton Begbie; and that of hemorrhœa petechialis on the reading of a paper by Dr Imlach of Liverpool.

To Dr Seller we are indebted for an ingenious paper on the production of vesicular emphysema in the lungs, as explicable by the principles of pneumatics, the object of which was to show, that the act of *expiration* must be essentially the cause of vesicular emphysema, in so far as it is a mechanical effect, in opposition to the view upheld by many recent authorities, under which it is regarded as a result of forcible *inspiration*. This paper gave rise to a long and animated conversation, in which Dr Seller's views were much controverted. The same author also furnished an able paper on the indications of apoplectic death; and Dr W. Begbie an interesting paper on abscess of the brain.

The subject of hepatic abscess was introduced in an excellent paper, by Mr Tait of the H.E.I.C., and some important information supplied by Dr Burn of the same service; while that of thoracic aneurism has been ably illustrated by Dr John Scott and Dr William Gairdner. To Dr Andrew and Dr Alexander Wood we are indebted for interesting accounts of the operation of the poisons of atropine and oxalic acid; and to Dr Coldstream for a valuable communication on the management of the fatuous.

Time would fail me were I to attempt to catalogue the whole of your proceedings in the department of practical medicine; and I can only further refer to the admirable report of Mr Benjamin Bell on the scarlet fever of George Watson's Hospital; and to his and Dr Hamilton's conjunct report of the cases treated in the Eye Infirmary,—communications which will repay an attentive perusal.

In regard to the communications on subjects of surgical interest, I need hardly remind you that the most important were those in which the treatment of stricture of the urethra was considered; and that the papers furnished on this subject—particularly in respect to the value of the mode of operation by external incision, as recommended and practised by Mr Syme—gave rise to a series of animated debates, in which the temper of the chair was somewhat tried, and the standing law of the Society, that "its object shall be to converse on medical topics, but without the form of debate or disputation," was not a little infringed. The result of these successive deliberations, in which Drs Mackenzie and Dunsmure, and Messrs Miller and Syme, took the chief part, was the conviction and acknowledgment that, in Mr Syme's hands, the operation proposed by him had been entirely successful; and that the profession were laid under great obligations to him for its introduction and successful accomplishment.

The subject of aneurism was also largely discussed, on the reading of a valuable paper by Dr Richard Mackenzie; to whom we are also indebted for an ingenious communication on the restoration of the lips and nose.

The operation of tracheotomy has on several occasions been discussed, and particularly on the occasion of Mr Miller bringing under notice its performance on account of a foreign body in the air-passages, in a case under the care of Dr David Johnstone of Montrose.

Besides these subjects of practical interest, you have discussed two others, of which deserve notice,—the one being that of electro-biology, introduced by the reading of the learned paper of Dr Alexander Wood, and listened to by the largest audience of the session; the other being the important matter of the registration of the causes of death, brought under your notice by Dr William Gairdner, and which has led to the formation of a new society, under the name of the Medico-Statistical Association,—an institution which is fa-

vourably known to many of you, and one which I feel satisfied deserves the cordial support of all who take any interest in the promotion of medical science.

Such is a rapid sketch—a mere title-page, as it were—of the proceedings of the Society during the two sessions now closed; and I cannot help congratulating you on the extent and the value of these communications; they will bear comparison with those of former years, and their reception will, I trust, stimulate their several authors to continued exertion, and bring into the field new labourers, by whose united assistance we may hope to reap abundance of future harvests.

I congratulate you on the addition which has been steadily made to the list of members, on the regular and full attendance, and on the interest taken at the different meetings; and I trust that many, who have as yet taken only a silent part, will ere long take a more conspicuous place in the business of the Society. All have it in their power to advance its interests, and support and improve its character. All of us are daily learning new facts in medicine; and there is no fact so trifling as to be unworthy of record, so that by their accumulation important truths may be established.

Let us avoid the danger and the disgrace of relaxing our exertions in the cause of science, as is too often done, when its pecuniary rewards have been largely bestowed upon us. Our life must be a life-time of study and observation; our daily ambition the promotion of science and the welfare of the human family. Let us remember our responsibilities: success in our calling implies the possession of opportunities and advantages which we are bound to employ for the good of others; a large share of the rewards and honours bestowed on professional merit should lead to fresh exertion in extending the boundaries of the science, by the exercise of which we have already enriched our homes; and let us cherish the thought, that the study and cultivation of medical science, independent of its rewards and honours, never fail to bring rich consolation. He who has most assiduously kept pace with its ever-advancing steps, and daily learned at the bedside of the sick new lessons of experience and observation to be treasured up and recorded, will enjoy, in the trying and anxious hours of professional life, a composure and a confidence which the careless student and unreflecting practitioner can never hope to experience.

While I thus congratulate you on the continued prosperity of our institution, and point to some motives which may influence its future prospects, we have to deplore the loss, by death, of some of its more distinguished members, —of some who have given it a warm and steady support from the period of its infancy, and who have contributed largely to its present efficiency. Among these I regret to mention the name of Sir William Newbigging—a veteran in years, but youthful in labours. He was one of the original founders of the Society; a counsellor during the first years of its existence; a vice-president for some years immediately before his death; and throughout his long life the zealous supporter of its best interests. Sir William did not contribute largely by his pen to the proceedings of the Society; but, during a long series of years, he materially assisted its deliberations, and frequently joined in the discussion of the most interesting practical questions brought under its notice, a task for which his ample and matured experience peculiarly fitted him. We lament his loss as the friend of our Society, and the ornament of our profession. We mourn the loss of another of the original members of our body, one who manifested a steady attachment to its interests, and who occasionally contributed to its literature. I mean Mr Macfarlan. He was cut down in the midst of his years and of his usefulness, endeared to many by his amiable qualities, and regretted by all for his premature demise. We mourn the loss of Dr William Thomson, an early member, and at one time secretary of the Society, a man of large, liberal, and enlightened views, profoundly read in the literature of his profession, and zealously desirous of promoting its improvement. Dr

Thomson had been removed to the seat of a sister University for many years before his death, and we were in a great measure deprived of the benefit of his learning, of his counsel, and of his business talents, which were acknowledged by all to be of the highest order; but his love of his profession and of her institutions was not confined to the city of his adoption, and our Society, as well as every other of a scientific kind, has reason to deplore his early death. We mourn the loss of another early friend and zealous and active member in the death of Dr Spittal, a man of great professional attainments, of pure philanthropy, and social disposition: a regular attendant at all our meetings, a frequent contributor to the list of communications, and a ready, willing, and effective speaker, we shall miss, as we have already done, his happy power of illustrating, even with touches of genius, the various subjects which came under discussion: we deplore his premature removal; and we bring before you also with sorrowful notice the sudden and unexpected deaths of Mr George Reid and Dr Alexander Campbell. The old, and the middle-aged, and the young—the ripe and the green, have been mown down together. It is not for me to draw the lesson from these sad events: it comes home to every heart: may it produce a commanding influence on every life.

Gentlemen, I would willingly have been spared the occasion of referring, even for a moment, to other losses than those we have sustained by the hand of death. Our own act—justified by a stern necessity, and imposed upon us by a due regard to the interests of science, and the honour of our profession—has severed from us some who were our late associates in the business of the Society, all of whom we would have gladly retained, and none of whom we can afford to lose: but they were not of us. They had abjured the faith in which they were educated, and in virtue of which they had obtained status, and honour, and emolument; they vilified the doctrines which they once taught, the teachers whom they delighted to honour, and the brethren with whom they took counsel together; they absented themselves from the colleges, and societies, and convivial meetings in which they once took delight; they waged war against their quondam friends, and against their former selves; and all this without a vindication, or, at best, with but a feeble and abortive attempt to palliate their conduct. The patience and forbearance which we long manifested were exhausted and overcome, and, by an almost unanimous voice, we declared them no longer members of the Medico-Chirurgical Society. We commiserate their position: we lament the injury done through them to a noble profession; but we rejoice in the hope that the age which has seen the rise of this fatal apostasy, may, long ere its close, witness its decline and downfall, when, after years of penitence, our alienated brethren may be received again into the fellowship and the privileges which they have so recklessly sacrificed.

Gentlemen, I congratulate you on the arrangements which have been made for conducting the business of the session on which we have now entered; on the improved—the handsome—apartments in which we are now met; and on the choice you are about to make of a Chairman to preside over your deliberations: of his merits I will not say anything in this place; he is known to you all; he is known to the world; and, with his untiring zeal, his devotion to science, and his unlimited opportunities of observation in all the departments of medicine, I confidently anticipate a prosperous and successful session.

I thank you for the patience with which you have listened to these hurried remarks, and take my leave of the chair.

Part Fourth.

PERISCOPE.

PHYSIOLOGY.

Experimental Researches applied to Physiology and Pathology.

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(*Continued from Vol. XV., p.* 580.)

7. My friend, Dr Cl. Bernard, has recently discovered the curious fact, that after the section of the sympathetic nerve in the neck, the face on the same side, and more particularly the ear, become warmer and more sensible than the other side. The blood-vessels are much enlarged, and a great many are visible which were not so before the operation.

I have found that the remarkable phenomena which follow the section of the cervical part of the sympathetic, are mere consequences of the paralysis, and therefore of the dilatation of the blood-vessels. The blood, finding a larger way than usual, arrives there in greater quantity; therefore the nutrition is more active. Now, the sensibility is increased, because the vital properties of the nerves are augmented when their nutrition is augmented. As to the elevation of the temperature, I have seen, as Dr Bernard has, that the ear exhibits sometimes one or two degrees Fahr. more than the rectum; but it must be remarked that the temperature of the rectum is a little lower than that of the blood; and as the ear is full of blood it is very easy to understand why it has the temperature of the blood. A great many facts prove, that the degree of temperature and of sensibility of a part is in close relation with the quantity of blood circulating in that part.

I base my opinion, in part, on the following experiments:—If galvanism is applied to the superior portion of the sympathetic, after it has been cut in the neck, the vessels of the face and of the ear after a certain time begin to contract; their contraction increases slowly, but at last it is evident that they resume their normal condition, if they are not even smaller. Then the temperature and the sensibility diminish in the face and the ear, and they become in the palsied side the same as in the sound side.

When the galvanic current ceases to act, the vessels begin to dilate again, and all the phenomena discovered by Dr Bernard re-appear.

I conclude, that the only direct effect of the section of the cervical part of the sympathetic is the paralysis, and consequently the dilatation, of the blood-vessels. Another evident conclusion is, that the cervical sympathetic send motor nerve-fibres to many of the blood-vessels of the head.[1]

[1] My experiments prove, also, that the blood-vessels are contractile, and that the nerves are able to put them in action. I have also to remark, that it is a fact well established by Budge and Waller, that the cervical sympathetic is one of the motor nerves of the iris; and that the spinal cord is the origin of the nerve-fibres going from the sympathetic to the iris. Some experiments, which I intend to perform again, appear to prove that the same parts of the spinal cord which give origin to some of the motor nerve-fibres of the iris, originate also the motor nerve-fibres going from the cervical sympathetic to the

8. Nearly all physiologists believe, that the secretion of the gastric juice is stopped after the section of the two pneumo-gastric nerves. It is difficult to solve the question by experiments on warm-blooded animals, because they die too quickly after the section of the vagi. But it is not so with frogs. I have found that they are able to live perfectly well, either after the extirpation of the medula oblongata, or after the extirpation of the ganglia of the par vagum. In both these cases I have found that digestion continues to be performed. Consequently, if the gastric juice is necessary to digestion, it is certain that this liquid is secreted.[1]

9. J. Reid has found, that if the four nerves uniting the spinal cord to the posterior limbs are cut across on both sides in frogs, and if a galvanic current is applied every day to the palsied limbs on one side, these galvanised limbs retain their natural dimensions, while the palsied limbs not galvanised become atrophied.

I have found—1. That if, instead of cutting only the four cerebro-spinal nerves of the posterior limbs, I divide also the branches of the sympathetic nerve which unite with them, the same results are obtained as in Reid's experiment. 2. That if a like experiment is performed on dogs, guinea-pigs, rabbits, and pigeons, the same results are found. 3. That if, after atrophy has taken place in the limb of a mammal or a pigeon, a galvanic current is applied every day during several weeks, the atrophy diminishes little by little, and the limb at length becomes as large as a sound limb. This happens, although there is no return of vital property in the divided nerves. 4. That if the application of galvanism is made on the palsied limbs of very young animals, and continued every day until they have arrived at adult age, these limbs are then found to have grown as much in every respect as the sound limbs.

In addition to these facts, I have to state, that in cases of lead palsy, in which the extensor muscles, as far as I have been able to judge, were completely destroyed and replaced by fibrous tissue, I have seen muscles created by galvanism, and becoming as strong as they are in healthy men.

In a case, which I published two years ago (*Gaz. Méd. de Paris*, 1850, t. v. p. 169), I have found that an increase of five centimetres in circumference took place in the superior part of the leg of a young gentleman, under the influence of galvanism, applied three quarters of an hour each day for six days.

In all the facts before related, galvanism acts by two ways: the one is, that it exercises the muscles, and increases in consequence their nutrition; the other is, that it produces directly some of the chemical changes which constitute nutrition.

The atrophy which happens in paralysed muscles takes place mostly because they remain without exercise, and partly because, when nervous action is deficient, the respiration of the muscles is not carried on as well as when the nervous system acts upon them. Galvanism applied to a palsied limb acts partly in producing the transformation of arterial into venous blood—*i. e.*, what Gustav. Liebig calls the respiration of the muscles. I have seen frequently the venous blood in palsied limbs becoming as black as normal venous blood after the application of galvanism. This change of coloration is not produced by a direct chemical influence exerted by galvanism on the blood; for if galvanism is applied to blood in a vase, nothing of that kind is seen. It is in consequence of an interchange between blood and the living tissues that the change of colour happens. The muscular contraction which takes place under the influence of the nervous system, or that of galvanism, produces in both

vessels of the head. Another conclusion is to be drawn from the results obtained by Budge, Waller, Bernard, and myself; it is, that the cervical sympathetic, instead of receiving its fibres from upwards to give them downwards, receives them downwards and distributes them upwards.

[1] Comptes Rendus de l'Acad. des Sciences. Paris, 1847. T. xxiv. p. 363, 64.

caused an increase in the darkness of the venous blood. This fact proves, that the consumption of oxygen by muscles is increased during their contraction.

I conclude, from the preceding facts,—

1st, Nervous action is not necessary for nutrition.

2d, Atrophy in palsied limbs is more a consequence of absence of exercise than of any other cause.

3d, Muscular atrophy at any stage may be cured by galvanism.

b.—*Influence of the Nervous Centres on Nutrition and Secretion.*

1. Every one knows the singular alterations which take place in the eye after a contusion of the frontal nerve, or a section of the trigeminal or the cervical sympathetic nerve. Every one knows also that the existence of worms in the intestinal canal, and also certain affections of the spinal cord, are able to produce morbid phenomena in vision, and even diseases of the eye, and especially amaurosis. I have found that, after the section of a lateral half of the spinal cord, it sometimes happens that the eye, on the same side where the cord has been wounded, will present strange and various alterations. The part of the cord having that influence on the eye, lies between the ninth and the twelfth costal vertebræ. The alteration exists generally in the cornea. In one case a ridge appeared on the anterior surface of that membrane four days after the operation. On the fifth day the ridge was deeper, and its edges had become opaque; on the sixth day all the cornea was opaque. It remained so for five days, after which the opacity disappeared, and no trace remained of it, or of the ridge. This experiment has been made on guinea-pigs.

2. I have found a considerable hypertrophy of the two supra-renal capsules, on eight or ten guinea-pigs, upon which a lateral half of the spinal cord had been cut in the dorsal region, for eight, ten, or fifteen months. These organs had acquired, in some of these cases, three times their natural dimensions, and in others only the double. There was no appearance of change in their structure.

By an examination of the supra-renal capsules in guinea-pigs, on which I had made a section of the lateral half of the spinal cord, a few hours or a few days previously, I have found these organs congested, and sometimes containing even a slight effusion of blood. It is very probable that such a congestion has been the cause of the hypertrophy found in animals operated on at a much longer time previously. The congestion is certainly the result of a peculiar disturbance in the nervous action. A part only of the spinal cord appears to possess that singular influence on the supra-renal capsules. That part is extended, in guinea-pigs, from the tenth costal vertebra to the third lumbar.

A simple puncture of the cord is frequently sufficient to produce the congestion of both supra-renal capsules.

3. The researches, made before mine, as to the influence of the spinal cord on the urinary secretion, could not give a decided result, because no physiologist had been able to keep any warm-blooded animal living a sufficient time, after the destruction of a large part of the spinal cord.

The results obtained by Ségalas on some animals who have lived from fifteen minutes to an hour after the destruction of the lumbar part of the cord, had led him to conclude that the spinal cord has no influence on the urinary secretion. Longet (Traité de Physiologie, Paris, 1850, t. ii. B. p. 199) says: —"Many observations have demonstrated to me that the visceral organs, which receive their nerves from the sympathetic, *are far from being immediately paralysed by the section of these nerves, and that their action is even maintained much longer than the duration of the experiments in which Ségalas had destroyed the spinal marrow.*[1] Therefore I think I am allowed to maintain, that after such an injury, the nerves going to these organs, and more particu-

[1] The italics are Longet's.

larly to the kidneys, do nothing but spend little by little the *nervous force*, originally and principally derived from the spinal marrow, which is the chief, if not the exclusive, centre of its production; thence the persistence of the renal secretion, as well as that of the movements of the heart, the intestinal canal, the uterine cornua," etc.

I could relate a great many experiments proving the incorrectness of Longet's theory, but a single one is sufficient. I have kept living, nearly *three months*, a young cat, on which the spinal cord had been *completely destroyed* from the eleventh or twelfth costal vertebra to its termination. This cat has lived all that time in apparently good health, and its *urine* has always been perfectly normal. It was acid, as is the case constantly in cats fed on meat, milk, and bread. The bladder was palsied, but the sphincter vesicæ was generally contracted, so that every day I had to compress the abdomen and the bladder to empty this pouch. When I remained two days without doing that operation, the bladder contracted, in consequence of the excitation produced on its muscular fibres by their distension.

This fact clearly proves that the urinary secretion is not under the dependence of the spinal cord.

According to Krimer, the medulla oblongata is the nervous centre upon which the urinary secretion depends. My experiments prove that this opinion is incorrect:—1st. After the destruction of the medulla oblongata in frogs, I have found that the secretion of urine remains as long as the animals have lived—*i. e.*, three or four months. 2d. On hybernating mammals, in winter time I have extirpated the medulla oblongata, after having emptied the bladder. These animals have lived a little more than a day, when I took the precaution of insufflating air in their lungs many times each hour. After their death the bladder was found full of urine, apparently normal.

The medulla oblongata is not therefore a centre on which the urinary secretion depends.

4. The well-known opinions of Ségalas, W. Philip, Krimer, Chossat, Longet, and others, about the influence of the spinal cord on the functions of organic life, are quite erroneous. I have found that birds are able to live for months after the destruction of the spinal cord, from the fifth costal vertebra to its termination. This fact proves not only that the functions of organic life may continue to exist in such a case, but that they appear to be executed then as in healthy birds; for, if the operation has been made on a young bird, it will afterwards grow very well.

I have succeeded in keeping alive, from the 8th of April until the 4th of July, a young cat, about which I have already published a note in this Journal.[1] The palsied parts in this animal have grown proportionately as much as the sound parts. The growth has been such in the palsied limbs that they have acquired more than double the length they had at the time of the operation. The functions of organic life appeared to exist without any apparent disturbance. The nutritive reparation was so powerful, that the pieces of the vertebral column which had been cut off have been reproduced. This fact is important, because it shows that the reproduction of bone is possible in a palsied part.

The temperature of that cat was at the ordinary degree (105° Fahr., in the rectum). The secretion of the hair and nails took place as in healthy cats. I had previously seen on birds that their temperature remained normal after the destruction of a great part of the spinal cord. Besides, I have found in these birds that the secretion of quills and nails continued to take place.

As to the influence of the medulla oblongata on the functions of organic life, my experiments on cold-blooded vertebrata have proved to me, that these functions (except, of course, pulmonary respiration), may continue to exist without any appearance of disturbance.

[1] See Med. Exam., No. v., May 1852, p. 321.

5. After the complete transverse section of the spinal cord in mammals or birds, I have found that the ulcerations which take place around the genital organs do not result directly from the absence of nervous action. One of the causes of these ulcerations is continued pressure, and another cause is the continual presence of altered urine and fæces.

My opinion is well proved by the following experiments :—

1st. I have put, three or four times a-day and for many days, a certain quantity of urine on the posterior part of the neck, in the neighbourhood of the scapulæ, upon guinea-pigs. Before a week elapsed, the skin, at the place acted on by the urine, had lost its hair and epidermis. After a week more there was an ulceration in the skin, and ten or twelve days later the skin was destroyed, and there was an ulcer with a very bad aspect. This fact proves how powerful is the action of urine on the skin.

2d. On guinea-pigs, upon which the spinal cord was cut in the dorsal region, and on pigeons, upon which the spinal cord was destroyed from the fifth costal vertebra to its termination, I have found that no ulceration appeared when I took care to prevent any part of their bodies from being in a continued state of compression, and of washing them many times a-day to remove the urine and fæces.

3d. In cases where an ulceration had been produced, I have succeeded in curing it by washing and preventing compression.

4th. I have found that in animals having the spinal cord cut across, every kind of wounds or burns were cured as quickly as in healthy animals.

Therefore the ulcerations which appear, in cases of paraplegia, do not exist directly in consequence of the palsy; they can be avoided, and in many cases they can be cured.

These conclusions are perfectly true in animals having had an injury to the spinal cord for a shorter time than a year; but on guinea-pigs, upon which a lateral half of the spinal cord had been cut for fourteen, fifteen, or eighteen months, near the tenth or eleventh costal vertebra, I have found an alteration of nutrition in the palsied parts. It was the right half of the spinal cord which had been cut, and in such a case, as I have discovered, the left side of the body behind the wounded part evidently loses a portion of its sensibility, and its temperature is also diminished. I have found, at the time designated, an ulceration coming in the part between the sacrum and the hip-joint. That ulceration has taken a tolerably great extension in surface but not in depth. It became as large as a half dollar. The part ulcerated has never been subjected to any kind of compression, neither to the action of urine and fæces. Another kind of disturbance of nutrition occurred in these animals: they lost the hair of the leg, and of the other parts in which the sensibility was diminished.

6. It is known that erection is a frequent phenomenon in men after a fracture or a luxation of the vertebral column. It is known also, that in men hanged, erection and even ejaculation are not uncommon. Ségalas says he has seen these phenomena produced by the excitation of the spinal cord. Longet (loco cit., p. 201), declares that he has not seen the excitation of the cord producing such effects. It is very easy to ascertain, on male guinea-pigs, that Ségalas is right.

1st. A transverse section of the spinal cord always produces an erection and an ejaculation.

2d. When one of these animals is asphyxiated, erection and ejaculation take place.

3d. If the spinal cord is galvanised, erection and ejaculation are produced.

These facts prove the influence of the spinal marrow on the seminal vesicles. They empty themselves slowly when the cord is galvanised.

In asphyxia, there are universal convulsions even in the muscles of organic life, as uterus, intestine, etc. These last organs are then put in contraction, and it is not astonishing, consequently, that the seminal vesicles became also

contracted. The cause of these general contractions is the excitation of the spinal cord by venous blood, and very probably by a large amount of carbonic acid, as I will elsewhere try to demonstrate.

IV.—ON THE REPARATIVE POWER OF THE NERVOUS SYSTEM.

I have recently published in this Journal[1] the results of my researches on the reparative power of the spinal cord. From these researches I have drawn the following conclusions:—

1st. That the spinal marrow, even in adult mammalia, may be exposed to the action of the air without danger to the life of the animal.

2d. That wounds of the spinal marrow may be repaired.

3d. That after a complete transverse section of the spinal cord, the functions of that organ may be entirely restored.

As to the nerves, the experiments of Fontana, Haighton, Tiedemann, Flourens, Steinreuck, and many others, have demonstrated the possibility of reunion of the two extremities of a cut nerve. But, in most, if not in all these experiments, the return of sensibility and of voluntary movements have not been complete. The following fact is, consequently, very important, because it proves the possibility of a complete re-appearance of the lost faculties after the entire division of a nerve.[2]

A guinea-pig, on which the sciatic nerve had been cut across, exhibited indications of a return of sensibility a month after the operation. Two months afterwards the sensibility was increased, but was still much inferior to that of the sound limb. The muscles then were beginning to contract under the influence of the will. Six months after the section, the animal could evidently move its legs and toes voluntarily; the sensibility then was almost entirely recovered. At the end of about eleven months, the sensibility and all the voluntary movements were apparently alike in the two posterior limbs. The animal having been killed, it was found by my friend Dr Lebert and myself that, except a slight union of muscular fibres with the nerve at the place where it had been divided, the restoration of the original condition was so complete that no indication of the division could be discovered, either with the naked eye or with the microscope. I had seen the usual swelling of the nerve at the point of reunion about the sixth month after the operation, but at the time of the last examination it had disappeared.

V.—ON TURNING AND ROLLING AS PHENOMENA PRODUCED BY INJURIES OF THE NERVOUS SYSTEM.

Pourfour du Petit and Méhée de la Touche were the first experimenters who witnessed turning produced by an injury of the nervous centres. But the first valuable researches on this phenomenon were made by Magendie and Flourens.

The parts of the cerebro-spinal centre which can be injured without producing turning, are: the cerebral hemispheres, the cerebellum, the corpora striata, the corpus callosum, the spinal marrow, and the olfactive and optic nerves.[3] All the other parts of the cerebro-spinal centres are able to produce turning or rolling.

These circulatory or rotatory movements take place sometimes on the same side, and sometimes on the side of the body opposite to that of the encephalon which has been injured.

A puncture of one of the following parts produces turning or rolling on the injured side:—

[1] Med. Exam., No. vi., June 1852, p. 379.

[2] See Gaz. Méd. de Paris, 1849, t. iv., p. 880.

[3] I consider as a part of the nervous centres, the three nerves of the superior senses: the olfactive, the optic, and the auditive.

1st. The anterior extremity of the thalami optici, according to Schiff.
2d. The crura cerebri, according to Magendie.
3d. The bi, or quadri-gemina tubercles, according to Flourens.
4th. The pons varolii.
5th. The posterior part of the processus cerebelli ad pontem.
6th. The auditive nerve, according to my own experiments.
7th. The medulla oblongata at the point of insertion of the facial nerve, according to my experiments in common with Dr Martin-Magron.
8th. The medulla oblongata outside of the anterior pyramids, according to Magendie.
9th. A great part of the posterior face of the medulla oblongata, according to my experiments.

The parts of the encephalon which produce turning or rolling on the opposite side, are:—

1st. The posterior extremity of the thalami optici, according to Schiff.
2d. The crura cerebri, according to Lafargue.
3d. The anterior part of the processus cerebelli ad pontem.
4th. A small part of the medulla oblongata before the nib of the calamus scriptorius and behind the corpora olivaria, according to my experiments in common with Dr Martin-Magron.

Some of these two series of parts ordinarily produce turning and the others rolling. But these two kinds of movements can be produced by the puncture of a single part of the encephalon. Rolling is nothing but the exaggeration of turning; thus, after a puncture of the medulla oblongata, the animal at first rolls, and after some instants, instead of rolling, it turns. If, when it is turning, a slight puncture is made anew, close to the first, then the animal rolls.

Before trying to explain turning, I will give an outline of some of its species.

1st. *Turning and Rolling caused by tearing the Facial Nerve.*

My friend Dr Martin-Magron and myself have discovered that if the facial nerve of a rabbit or a guinea-pig be exposed at its exit from the stylo-mastoid foramen, and be then drawn away from the cranium, so as to tear it asunder near its origin, the animal begins in about five minutes to turn itself round and round, the movement being from left to right when the nerve has been thus torn on the left side, and from right to left when it has been torn on the right side. This rotation is generally preceded by convulsive movements of the eyes, of the jaws, and of the head upon the trunk: and the body is then bent (as in pleurosthotonos) towards the injured side, by the contraction of all the longitudinal muscles of that side, the power of which is such as to resist considerable force applied to extend them. The movement at first takes place in a small circle; but the circle generally enlarges more and more, until at last, after twenty or thirty minutes, the animal walks in a straight line. There is no paralysis of any muscles, save the facial. The effect is not produced, unless the nerve be torn close to its origin.

When the nerve on the other side also is torn, even after a long interval, instead of the tendency to turn to one side, there is, at first, a rolling of the body on its longitudinal axis, which takes place towards the side last operated on. After this has continued, however, for twenty minutes or more, the animal recovers its feet, and begins to *turn*, as after the first operation, but towards the other side. This movement soon ceases.

Dr Martin-Magron and myself think that the cause of these phenomena does not exist in the facial nerve itself, but in the part of the medulla oblongata from which this nerve has originated.[1]

[1] See Gaz. Med. de Paris, 1849, t. 4, p. 879.

2d. *Turning and Rolling produced by an injury to the Medulla Oblongata.*

M. Magendie (Précis Elém. de Physiol. Paris, 1836, t. i., p. 414) says :— "Having raised up the cerebellum, I make a section perpendicularly to the surface of the fourth ventricle, and at three or four millimetres from the median line. If I cut on the right, the animal will turn on the right side ; if I cut on the left, it will turn on the left side."

If we suppose a plane cutting the medulla oblongata transversely at the distance of nearly two lines before the nib of the calamus scriptorius, the posterior face of the medulla oblongata will be divided into two parts : one before that plane, which I will call superior, and the other behind, or inferior.

Now, every puncture on that superior part produces turning or rolling on the side which has been punctured. The slightest puncture on the processus cerebelli ad medullam oblongatam is able to produce a violent and very rapid rolling. As long as the animal lives after the operation, it rolls or it turns at each time it tries to walk.

When (as Dr Martin-Magron and myself have discovered) a deep section is made on the inferior part of the posterior face of the medulla oblongata, before the nib of the calamus scriptorius, turning is produced on the side of the body opposite to the punctured side of the medulla. A rabbit, which has lived thirteen days after the operation, had still the circulatory movement a few hours before dying. Nevertheless, sometimes the animal could walk nearly straight during a few seconds.

3d. *Turning produced by a Puncture or a Section of the Acoustic Nerve.*

Flourens has discovered that, after the section of the semi-circular canals, turning sometimes takes place.

I have found all the facts he relates about this subject perfectly right. It was interesting to know if a puncture or the section of the auditive nerve would produce turning. As it was impossible to operate on that nerve in mammals, I have experimented on frogs. In these amphibia it is easy to find the nerve and to act upon it. I have found that after a puncture or a section on the trunk of the nerve, the animal begins instantly to turn. As long as the frogs live, after a puncture of the acoustic nerve, they turn ; but the circle of turning is much smaller a short time after the operation than afterwards. I have kept such frogs for months.

4th. *On a New Mode of Turning.*

I have found a mode of turning which has altogether some of the characters of turning and of rolling.

In the circulatory movement called turning (*mouvement de manége*) the body of the animal is bent on one of the lateral sides. It has the shape of an arch, and this arch has generally a part of the circumference described by the animal when turning. The smaller the radius of that arch, the smaller is the circle of turning.

In the new mode of turning I have found, the body of the animal is not bent, and when it walks it moves laterally, instead of going forwards. In turning it describes a circle, but the longitudinal axis of its body, instead of being then a part of the circumference, is a part of a radius, so that its head is at the circumference, and its tail towards the centre of the described circle.

That mode of turning has been executed by animals on which the quadrigeminal tubercles and the pons varolii, on one side, had been punctured by a pin. One of the eyes was convulsed ; the other was in its normal condition. The convulsed eye was the right one, and the tubercles punctured were those of the left side.

5th. On the Causes of Turning and Rolling.

I have not room enough to show that the theories of Magendie, Flourens, Henle, Lafargue, and Schiff, are contradicted by a great many facts. I will only present the following remarks:—

1st. As the slightest puncture of certain parts of the encephalon is sufficient to produce turning or rolling, it is evident that those rotating movements do not exist in consequence of an hemiplegia, as Lafargue, Longet, and Schiff believe they do. Another reason is that every degree of hemiplegia exist in man without being accompanied by turning or rolling. Besides, these phenomena have been observed in persons who had no paralysis at all.

2d. The theories of Magendie and Flourens are also opposed, by the fact that a slight puncture is sufficient to produce turning or rolling.

3d. As to the theory of Henle, which is based upon the existence of convulsions in the eye, producing a kind of vertigo, it has against it the facts that, on one side, convulsions may exist in the eyes without any other disorder in the movements; and, on the other side, sometimes turning or rolling exist without any convulsion in the eyes.[1]

Nevertheless, I think that, in many cases, the vertigo consequent on convulsions of the eyes is one element of the cause of turning. I think also that, in certain cases, paralysis of some parts of the body may facilitate the rotatory movements. But their great cause, I think, is the existence of a convulsive contraction in some of the muscles on one side of the body. These convulsive contractions are to be found in every case of circulatory or rotatory movement. As to the cause of these contractions, it exists in the irritation produced in certain parts of the encephalon.—*Philadelphia Medical Examiner*, August 1852.

(*To be continued.*)

MIDWIFERY.

PHLEGMASIA DOLENS OF THE UPPER EXTREMITY, OCCURRING AFTER PARTURITION.

BY J. M. WINN, M.D.

On the 15th of April last, I was called in to see a Mrs G., about forty years of age, residing near the Mile-end Road, and who had been delivered of her ninth child thirteen days before I saw her. I found her in a sinking state. The countenance was anxious; the abdomen tympanitic, and distended to an enormous extent; the pulse small and rapid, and the mind confused. The peculiar and very remarkable symptom, however, was a pale, hard, and extremely painful tumefaction of the whole of the right arm, extending from the shoulder, in an even and diffused manner, to the extremity of the fingers.

The lower extremities of the body were not affected.

Mr Hall of the Mile-end Road, who was in attendance on the case, informed me that his patient had been delivered by a midwife; that she had suffered from occasional rigors, and also from pain, without swelling, of the left shoulder, as if the disease were about to affect both upper extremities.

We ordered milk and brandy, but it was evident that nothing could save her; and she died early on the following morning.

There can be no doubt that the above case was a most unusual form of phlegmasia dolens. I cannot, indeed, find on record a single instance of phlegmasia dolens of an upper extremity occurring after parturition. Dr Rams-

[1] See a very remarkable case observed by my friend Dr Lebret, in *Comptes Rendus* et Memoires de la *Soc. de Biologie*, année 1850. Paris, 1851. t. ii., p. 7.

botham, in the last edition of his valuable work on midwifery, alludes to phlegmasia dolens of the arm as an occasional sequel of carcinoma mammæ; and Drs Stokes and Graves mention similar cases as complications of typhus, but in no instance as occurring after parturition.

The cases which approximate most closely to the one I have instanced are those (No. 1 and No. 2) published by Mr Coulson, in his interesting observations on "Affections of the Joints in the Puerperal State;" but in these cases the disease and pain were confined almost entirely to the neighbourhood of the joints.

I am sorry to add, that every effort to obtain a post-mortem examination was made in vain.

13, Finsbury Square.

RECESSION OF POLYPUS WITHIN THE UTERUS. BY DR RAMSBOTHAM.

Miss F——, of B——, in Essex, about thirty-two years old, consulted me in the autumn of 1839, for irregular hemorrhagic discharges, attended with frequent attacks of vomiting, which had made considerable inroads upon her strength. Nothing could be detected by examination per vaginam, beyond a slightly increased size and weight of the uterus. As she lived in the house of her brother, a highly respectable and intelligent practitioner of medicine, she was necessarily carefully superintended. Nevertheless, she occasionally made a journey to London for my advice; and it was not till the middle of the year 1841 that I detected the apex of a polypus protruding through the os uteri. At the end of July it was sufficiently external to warrant an attempt at removal; and she took lodgings in my neighbourhood for that purpose. She was then so much depressed by hemorrhage, leucorrhœa, and sickness, that she was confined almost entirely to the sofa and bed. On the day when it was agreed that she should remain in London, I could surround the tumour distinctly, and ascertained that it was attached towards the anterior part of the body of the womb. Waiting for her to recover somewhat from the fatigue of her journey, I made an appointment to tie it on August 2d; but found that it had entirely retreated within the uterus, and the mouth of that organ had closed upon it. On the 5th it was again to be felt; and I went on the next day, hoping it would have descended low enough for the operation to be performed; but it had again quite disappeared. However, on the 10th, it protruded lower than it had ever done before. It was not larger than a hazel nut. I could now not only feel the stem, but could discover the point of its attachment. I had some difficulty in passing the ligature around it, as well in consequence of its small size and mobility, as from the constriction of the vagina, and narrowness of the os externum. It was, however, satisfactorily noosed, and came away on the fourth day. On all these occasions, Mr Hamilton, surgeon to the London Hospital, accompanied me. The hemorrhage, which had been going almost incessantly for some length of time, ceased entirely as soon as the ligature was tightened; there was very little return of sickness after the tumour separated. On the 19th, a coloured discharge came on, which lasted five days, and which I have no doubt was a catamenial period, and she went home before the end of the month, free from any kind of discharge, and stronger than she had been for years before.

In this case, had a fresh opinion been sought to confirm or disprove the former one, after the polypus had been first dectected (which would appear to be in many instances but a natural proceeding), the second practitioner applied to, not being able to feel any polypoid or other growth, would be disposed to form a very poor opinion of the powers of diagnosis possessed by his predecessor, even if he did not put a more uncharitable construction upon his conduct than might arise from the presumption of a want of skill. They would teach us, therefore, on all such occasions, not too rely too implicitly on our own judgment, and to abstain from giving utterance to any observations that might damage the professional character of a previous attendant.

Were such a case again to come under my notice, I would endeavour to dilate the os uteri by means of a sponge tent; and I think it most probable, that by so doing, an earlier opportunity would be afforded of using the ligature than might otherwise occur.

COMPARATIVE FECUNDITY OF THE WHITE AND BLACK RACES.

An American practitioner, Mr Pendleton, collected the following statistics from the district of Georgia, the women being from 30 to 40 years of age:—

Colour.	No. Married.	No. of Children Born.	Proportion to Each.
White, . .	587	1207	2·05
Black or Mulatto, .	986	2392	2·42

Thus it follows, contrary to general opinion, that Georgia black women surpass the white in fecundity. Abortions, however, appear more common among the black race, in the proportion of 51 to 40.

Mr Pendleton also observes, that it is among the white race he finds the greater proportion of those diseases which produce sterility. The following table exhibits the relative proportions.

Morbid State	Among the White.	Among the Black.
Dysmenorrhœa, . . .	30	14
Ménorrhagia, ...	16	10
Prolapsus of Uterus, . .	26	9
Amenorrhœa, ...	23	9
Leucorrhœa, ...	11	6

—*L'Union Medicale.*

CHEMISTRY.

ON SOME OF THE MORE IMPORTANT CHEMICAL DISINFECTANTS. BY GEORGE WILSON, M.D., F.R.S.E.

The term *disinfectant*, in strictness of language, can only be applied to those agents or substances which destroy or decompose infectious or contagious matter. But it is usually employed in a wider sense, so as to include not only *disinfectants proper*, but likewise *antiseptics* and *deodorisers*. Any attempt to draw a sharp line of demarcation between these three classes of agents, is rendered impossible by our almost total ignorance of the nature of contagious matter. Some substances, such as chlorine and sulphurous acid, possess at the same time disinfectant, antiseptic, and deodorising powers. Some, like common salt, are probably simply antiseptic; of others, such as the salts of the heavy metals, which are in high repute as deodorisers, it may be questioned whether they are of any value as disinfectants, although with some persons they rank at the head of the list. Without insisting at present on this, it may suffice to define the bodies we are about to consider: Thus, a disinfectant is an agent which effects the chemical decomposition of organic poisonous matter, the term poisonous being used in a wide sense to include all the known or supposed causes of the development of disease, which are referred to under the names of miasma, malaria, infectious virus, contagious matter, etc.

An antiseptic is an agent which prevents or arrests the development of or-

ganic poisonous (or non-poisonous matter) without effecting its chemical decomposition.

A deodoriser is a substance which destroys odour, by decomposing or combining with, or absorbing odorous matter. Chlorine, for example, decomposes sulphuretted hydrogen, whilst a salt of lead decomposes it, and charcoal simply absorbs it.

Before considering the relative merits of particular substances belonging to these classes, it is necessary, however, briefly to discuss the important question, Does the poisonous organic matter which occasions certain diseases, occur in the solid, liquid, or gaseous form? The certainty that prolonged exposure to a vitiated atmosphere, such, for example, as that of a fever ward, produces disease, has led to a conclusion in which probably all concur, that the air is one of the chief media through which disease is propagated, and this connection has in turn led to the much more doubtful inference, that infectious matter is truly gaseous or vaporous. This view has probably been strengthened by the recent extensive study of the properties of anæsthetics, and by the many observations which have been made on the rapid and powerful action on the body of substances which enter it through the lungs. It has certainly also been deepened by the opinion widely prevalent, that the gases which are evolved from cesspools, sewers, and stagnant waters in general, particularly sulphuretted hydrogen, hydrosulphuret of ammonia, and marsh gas (light carburetted hydrogen), are the *direct* and *specific* causes of ague and fever.

If this opinion were well founded, the limits and best modes of applying disinfectants could be determined without much difficulty, and our control over infectious diseases would certainly be much greater than it is.

I think, however, that we may with confidence affirm, that the great majority of diseases are not propagated by gaseous poisons. The recent tendency to advocate an opposite opinion, has been mainly occasioned, I believe, by an opinion expressed by the late Professor Daniell, to the effect, that the fatal fever of the African coast is occasioned by sulphuretted hydrogen. This view was founded on an analysis of water brought from that coast, and determined the ventilating arrangements fitted up in the vessels which formed the disastrous Niger expedition. It appears to have been extensively adopted by medical men.

During the frequent prosecutions for nuisance, under the new Police Act, which took place in this city and elsewhere during the last visitation of cholera, it occurred to me, and to other chemists, to be constantly met by endeavours, on the part of the prosecutor, to compel an acknowledgment that sulphuretted hydrogen, hydrosulphuret of ammonia, and marsh gas, or light carburetted hydrogen, which are confessedly given off by sewage waters, are the *direct* causes of fevers and other diseases. So long as this idea prevails, and men rest satisfied with it, as the true explanation of the mode in which fevers and similar maladies originate and are disseminated, they will cease to prosecute inquiry into the matter. It is most important, therefore, to discountenance the notion that we are acquainted with the true *materies morbi*.

That neither sulphuretted hydrogen, nor hydrosulphuret of ammonia, produces any special disease, may be sufficiently demonstrated by the impunity with which persons are known to expose themselves to much larger quantities of these gases than can possibly act on those who suffer from exposure to marsh miasmata. In these the nicest tests have failed to give the slightest indications of sulphuretted hydrogen, and yet a few hours' exposure to such miasms has been enough to develop fever. On the other hand, in every analytical laboratory, sulphuretted hydrogen and hydrosulphuret of ammonia are daily respired for weeks or months together by those engaged in analysis, yet analytical chemists certainly are not specially subject to fevers. At the Bonnington Chemical Works, where the ammoniacal liquor from the Edinburgh Gas Works is largely converted into sulphate and muriate of ammonia, the workmen are exposed to the hydrosulphuret of ammonia, which forms so considerable a part of the liquor; and when it is neutralised with sulphuric and muriatic acid,

sulphuretted hydrogen is given off in such abundance, as to blacken the silver coins and watches on the persons of the bystanders, and even (along with the carbonic acid simultaneously evolved) to render them temporarily insensible, if they incautiously respire the gases. Yet no special malady is known to result from this exposure; and the Bonnington Works enjoy the reputation, in the neighbourhood, of protecting it from the inroads of endemic and epidemic diseases. Similar observations, as to the non-deleterious effects of exposure to comparatively large volumes of sulphuretted hydrogen, have been made at the metal works, where a superficial tarnish of metallic sulphuret is removed by washing with acids, and the workmen are freely exposed to the sulphuretted hydrogen evolved. I need not say that I do not wish to affirm that this gas, or its combination with ammonia, is not a powerful poison, if respired alone, or to deny that the continued entrance of either into the body must debilitate it, and prepare it for yielding to the attacks of disease. But that it is the cause of the fevers, which a very short exposure to the so-called malaria of certain districts infallibly occasions, I altogether disbelieve.

The alleged noxiousness of diluted marsh gas (light carburetted hydrogen), admits of more easy disproof; for were it the deadly agent it has been declared to be, our colliers, who are exposed in coal pits to much larger volumes of it than any other class of persons, should be, to a corresponding extent, sufferers from the diseases which it is supposed to occasion; but, unless when its mixture with air explodes, it is destitute of any injurious action on the pitmen, who are a healthy class of the community.

Another disease, namely influenza, has been imputed by high chemical authorities to the diffusion through the atmosphere of a peculiar gas. Dr Prout regarding seleniuretted hydrogen as its cause, Schœnbein attributing its production to ozone. There is no evidence that either of these views is true, but much may be said in favour of the latter. The last severe epidemic of influenza spread over Europe with a rapidity which almost seems to point to a gas as the medium of its propagation. No one, however, has detected seleniuretted hydrogen in the atmosphere; and air largely impregnated with ozone, may be breathed with an impunity which throws grave difficulties in the way of Schœnbein's hypothesis.

Whilst thus, with the exception of influenza (if it is to be excepted), no gas is known to possess the power of developing an infectious or contagious endemic or epidemic; on the other hand, as Professor Graham has justly remarked, such infectious matters as are accessible to us; for example, "the matter of cow-pox may be dried in the air, and is not in the least degree volatile. Indeed, the volatility of a body implies a certain simplicity of constitution and limit to the number of atoms in its integrant particle, which true organic bodies appear not to possess. Again, the source of such bodies being at all times inconsiderable, they would, if vapours be liable to a speedy attenuation by diffusion, be so great as to render their action wholly inconceivable. It is more probable that matters of contagion are highly-organised particles of fixed matter, which may find its way into the atmosphere, notwithstanding, like the pollen of flowers, and remain for a time suspended in it."[1]

To these statements it may be added, that all chemists now acknowledge that volatility is not essential to the transference of solid bodies to the atmosphere, at least so far as those soluble in water are concerned; for observations on the largest scale have shown, that the vapours of volatile liquids carry with them sensible quantities of all the solids which they dissolve; common salt, nitrate of potass, boracic acid, phosphoric acid, afford marked examples of this; but the list of salts soluble in water which accompany its vapour at temperatures at which, when dry, they are fixed, is endless.[2] The significant word

[1] Elements of Chemistry, p. 336.

[2] In virtue of this, we may anticipate the administration of other medicines than anæsthetics by the lungs, although they may not be volatile. In cases of poisoning, it would

"*malaria*," therefore, which embodies in a single term the evil reputation which the air or atmosphere has acquired, as the vehicle of contagion, may still, if we choose, be retained, although we acknowledge that all accessible contagious matters are non-volatile liquids or solids. It may further be added, that with the questionable exception of influenza, no endemic or epidemic spreads with the rapidity and equability, so far as area of occurrence is concerned, which would characterise it, if it were occasioned by a gas subject to a force so powerful as that of gaseous diffusion. Professor Graham's argument is still more cogent; for, according to his views, if infectious matters were truly gaseous, we should never have endemics or epidemics, unless those matters were developed in immensely larger quantities than by universal acknowledgment they are. In truth, they elude every test, even when applied to large volumes of the most infected atmospheres.

From all that has been stated, it must be inferred, according to our present knowledge, that at least the great majority of the substances which are intended to be reached by disinfectants, are not volatile, and therefore are much less easily decomposed than if they were gases. We may also with reasonable confidence affirm, that they are organic products, and as such consist of carbon, hydrogen, oxygen, and nitrogen, or at least of two (if not always of three) of these elements; and that, like all such compounds, they are readily decomposed by chemical re-agents, especially oxidizing ones. There is no reason to imagine that infectious matters are difficult to decompose, *provided we can reach them*. The difficulty lies in reaching them. Assuming, then, that contagious matters are not volatile, and that they contain (to take the most complex case) carbon, hydrogen, oxygen, and nitrogen, the principles which are to guide us in the application of chemical disinfectants, will not be far to seek. Oxidizing agents will plainly be of great value, as they can readily convert hydrogen into water, and carbon into carbonic acid, and thus disintegrate and destroy the morbific matter. Substances having a great affinity for hydrogen, such as chlorine and its class, will plainly also be of great service. Substances having an affinity for oxygen will also be applicable to the destruction of organic poisons; and, finally, all re-agents which, by contact with organic matter, can determine a new arrangement of its ultimate elements. All the powerful chemical disinfectants act in one or other or all of those ways. I shall refer to five of the disinfectants:—1. Quicklime, including caustic potash and soda; 2. Nitric acid; 3. Chlorine; 4. Aqua regia; 5. Ozone. The value of *quicklime* and of the *caustic alkalies* as disinfectants, has certainly not been overrated, although it may be questioned whether our sanitary authorities have been wise in trusting to lime alone as a purifier. From the careful study of the process of natural and artificial nitrification, and from the results of the application of soda-lime in organic analysis, we have learned that the caustic alkalies and alkaline earths decompose organic matter with the evolution of ammonia, which, by oxidation, may become converted into nitric acid. Wood-work or stone floors, to which a coating of lime-wash cannot be applied, requires only to be washed with caustic soda or soft soap, to obtain an effect identical with that which lime occasions.

2. *Nitric Acid* seems latterly to have fallen into disrepute, but certainly undeservedly. It acts more rapidly on many organic compounds than chlorine does, attacking their carbon as well as their hydrogen; and as it is not required in large quantity, its application is not costly.

3. *Chlorine.*—Of chlorine, which is at present the favourite disinfectant, it is needless to speak. Its peculiar power of decomposing combinations of hydrogen gives it, in one respect, a superiority over nitric acid, which does not de-

be of the greatest importance if we could directly transfer to the blood an emetic or purgative, which we may hope to do along with the vapour of its solvent, aqueous, or non-aqueous. Such a process, however, would be applicable only to medicines which act powerfully in small doses.

compose many of the gaseous hydro-carbons; but it should not be forgotten, that it is only in the presence of light that this action of chlorine is fully displayed, so that its disinfectant influence is comparatively small in the case of dark or ill-lighted apartments, such as underground cellars, the lower cabins or the hold of a ship, which are the very places where disinfectants are often most wanted.

4. *Aqua Regia*, as uniting the properties of nitric acid and of chlorine, each of which has peculiar virtues, the former in particular being a powerful oxidizing agent; the latter possessed of a great decomposing action over hydro-carbons, appears entitled to a high place among disinfectants. It can be cheaply procured, by pouring oil of vitriol on a mixture of nitre and common salt, or by heating a mixture of nitric and muriatic acids.

One of the most rapid and effectual methods of disinfecting a large empty apartment, such as an hospital ward, would be to place in one corner a vessel containing the materials for chlorine, such as oxide of manganese and hydrochloric acid, or oxide of manganese, common salt, and oil of vitriol, and in another corner a vessel containing nitric acid and a few fragments of copper, so as to evolve nitric oxide, which would spread through the apartment, and form nitrous acid there, oxidizing everything oxidable which it contained, whilst the chlorine specially attacked the hydrogenous compounds. The walls might then, if necessary, be lime-washed, with a view alike to destroy any adhering organic matter which had resisted the action of the gases, and to neutralise any traces of free acid.

5. The last of the disinfectants proper to which I refer, is the singular substance ozone, which has a special interest, as being in all probability the great natural disinfectant. Its nature is still matter of speculation. Schœnbein, its discoverer, regards it as a peculiar oxide of hydrogen; Berzelius and Faraday represent it as simply oxygen in a peculiar (or allotropic) state of modification; it has been suggested that it is an oxide of nitrogen; and quite recently M. Fremy has affirmed it to be what he calls "electrised oxygen," *i.e.*, oxygen modified in properties by the action of electricity upon it, a view not materially differing from that of Berzelius and Faraday. There are difficulties in the way of all these views, into which it is not necessary to enter. All that concerns our present subject is, that, by different processes, a substance can be developed in the atmosphere which possesses remarkable disinfectant and oxidizing properties. The oldest known method of producing the so-called ozone, is the exposure of air to a stream of friction or high tension electricity. Its odour may always be recognised in the neighbourhood of an electrical machine whilst at work. Another method is the galvanic decomposition of water, when the ozone accompanies the evolved oxygen. A third, and the most convenient method on the small scale, is the exposure of phosphorus in moist air. By these processes, and by certain others, air is made to acquire a striking power of oxidizing, bleaching, deodorizing, and disinfecting. We cannot doubt that every thunder storm develops some ozone, and other processes also probably produce it. At all events, the atmosphere frequently exhibits an oxidizing and bleaching power, at other times absent, which Schœnbein, Faraday, and others attribute to the development of ozone within it.

No one who has experimented on ozone will doubt its potency. I refer to it here because there are so many reasons for believing that it is the agent which prevents the accumulation in the atmosphere of volatile organic bodies, by converting them into water, carbonic acid, nitric acid, and ammonia, that we cannot avoid looking hopefully to it as destined to prove our disinfectant *par excellence*. Certain as we are that for thousands of years miasmata, malaria, poisonous effluvia, and every gas, vapour, and volatile body developed at the surface of the earth, must have found their way into the atmosphere, and that, nevertheless, its purity is not sensibly affected, we must regard the constituent or condition of the air, which has secured its purity during centuries, as one demanding special study. Further, this constant process of disinfection has not interfered with the respira-

tion of animals, so that we may fairly regard ozone as a substance applicable as a disinfectant in places occupied by human beings or by the lower animals. It is true that the power of producing influenza or catarrh has been attributed to ozone in excess, on grounds, however, almost entirely speculative. This view may or may not be true; but of this I am quite certain, that the well known impunity with which electricians expose themselves for hours together to the action on the atmosphere of large friction machines, which the dullest nostril can discover to be producing abundance of ozone, is enough to show that a large impregnation of the air with this substance neither affects respiration nor produces catarrhal affections. We ought, therefore, I think, to give special attention to ozone. It is not likely that we shall be long without discovering new processes for its production. It will be specially valuable for what are the most important, and, at the same time, the most difficult occasions for disinfection, namely, where human beings cannot be removed from infected apartments. Examples of such cases are found in a large ship at all times. and still more when its crew and passengers are attacked by disease; in the wards of an hospital, from which the sick cannot be taken; and perhaps most strikingly in a large factory, where hundreds of persons assemble daily together, many of most uncleanly habits, and at epidemic seasons fresh from infected rooms, whilst the apartments contain valuable metallic machinery, and fragile silk, cotton, linen, or woollen goods, which interpose an additional obctacle to the free employment of gaseous disinfectants. The condition of our ships, as regards ventilation and wholesomeness, is proverbial; and on inquiry of residents in Manchester and Glasgow, I find that, where disinfection has been attempted in factories, which it rarely has, it has consisted in sending a man once a day through every room with a quantity of blazing pitch, which was supposed to fumigate into purity the atmosphere, whilst it set all the workpeople coughing.

How difficult it is to prevent the spread of erysipelas, gangrene, fever, and the like in hospitals, every medical man knows too well. Ozone at least deserves a trial as a disinfectant in such cases.

Antiseptics.—The only antiseptics to which I shall refer are two. The first is sulphurous acid. It is a powerful antiseptic, for it resists thoroughly the decomposition or decay of organic matter. In reality, however, it as much resists the development as the decay of organic bodies, and thus it doubly prevents the evolution of organic poisons. Dr Christison long ago pointed out how small a quantity of this acid is sufficient to destroy plants. In the wine countries it has been used from time immemorial to prevent the souring or acetification of the lighter wines, when kept in casks partially filled. Professor Graham, who strongly recommends it as a disinfectant, draws attention to the fact, that at Manchester the offensive effluvia of the cochineal dyĕ-vats, which resist the action of chlorine and nitric acid, are at once destroyed by sulphurous acid. My own attention was directed to it from the employment of it on a large scale by paper-makers and others, to secure the preparation of pure gelatine—a substance peculiarly liable to enter into putrefaction. Sulphurous acid can be easily prepared by burning sulphur, or by heating oil of vitriol along with charcoal or vegetable matter. Its corrosive action is very slight; its disinfecting action very powerful. The sulphite of soda is now prepared in quantity at different chemical works. The addition of a stronger acid sets free the sulphurous from its salts. As to its mode of action, if we concur with Liebig in believing that morbific matters resemble ferments in being active only whilst undergoing a decomposition, which is mainly determined by the oxygen of the air, we may suppose sulphurous acid to render the poisonous matter inert, by preventing its oxidation. This acid, moreover, is a powerful deoxidizing agent, and it may be by removing oxygen from organic poisons that it renders them inert, by decomposing them.

Further, sulphurous acid can combine with certain elements of organic bodies, as we see in its temporary bleaching action on vegetable colours; and it may be thus that it neutralises morbific matters. In one or other, or all, of those modes,

this agent may act as a disinfectant; but at all events its action is very powerful, and it deserves much more attention than it has received.

The only other substance to which I shall at present refer is pitch oil, one of the products of the distillation of tar. It is an antiseptic of the most powerful class, and very cheap, and, if not used in excess, it is applicable as a deodoriser, but its own strong tarry smell interferes with its extensive use.—*Trans. of Edin. Pharm. Society*, in *Pharm. Jour. and Trans.*, December 1, 1852.

Part Fifth.

MEDICAL NEWS.

EDINBURGH MEDICO-CHIRURGICAL SOCIETY.

SESSION XXXII.

MEETING I.—*Wednesday, November* 24, 1852.—Dr BEGBIE, P., in the Chair.

SYPHILITIC DISEASE OF TRACHEA.

1. *Dr James D. Gillespie* exhibited a preparation of tracheal disease uncomplicated with any affection of the larynx. There was a circular patch of ulceration, with necrosis of the cartilages, about two inches below the cricoid cartilage; other smaller ulcerations existed at the bifurcation and in both bronchi. The right bronchus, at its origin, was very much contracted. Dr James W. Begbie and Dr G. had been hurriedly summoned to see the patient in the middle of the night, and had found him in imminent danger from impending asphyxia. They could discover no disease in the chest, so Dr G. at once performed tracheotomy, which afforded great relief to the patient. He lived for two days afterwards, but died somewhat suddenly on the morning of the third day of pleuro-pneumonia. The autopsy showed recent pleurisy on the left side, with hepatisation of the greater part of the lower, and a small portion of the upper, lobe of the left lung. The right lung was in a very congested state.

In answer to a question from the *President*, *Dr G.* stated that the disease was of syphilitic origin, and that the man had been of very intemperate habits.

APOPLEXY, RECENT ENDOCARDITIS, AND DISEASE OF SPLEEN.

2. *Dr W. T. Gairdner* showed a specimen of recent deposition of lymph in the endocardium, in conjunction with older disease of the aortic and mitral valves, and hypertrophy of the heart. The patient, a man below middle age, of whom the detailed history was not known, had died of apoplexy. A large clot was found in the brain, but there was no disease of the vessels of that organ, nor of the great arterial trunks, such as is usually found in apoplexy, especially when connected with heart disease. Another peculiarity in the case was hypertrophy of the spleen, and a friable yellow deposit in its substance, such as is often found in heart disease, in typhoid fever, and under other circumstances. The enlargement of the spleen directed particular attention to the blood, in which a slight excess of white corpuscles was found.

3. *Dr W. T. Gairdner* also showed specimens of a very peculiar disease of the skin and subcutaneous cellular tissue, which consisted in the development of a great number of small soft elastic tumours, composed of a very delicate fibrous tissue, having the softness and compressibility of a down pad or cushion. These tumours were not confined to the skin, being also developed in the neighbourhood of the abdominal muscles, and in one instance in the neurilemma of a small nervous twig going to the transversalis muscle.

LARYNGEAL DISEASE AND PHTHISIS PULMONALIS—TREATMENT OF PHTHISIS.

Dr Bennett read a paper entitled "Illustrations of Laryngeal and Pharyngeal Disease, frequently mistaken for, or associated with, Phthisis." The object of the paper is sufficiently indicated by the title. (See "Monthly Journal" for December 1852.)

A discussion followed the reading of Dr Bennett's paper, in which

Dr Begbie acknowledged the value and interest of Dr Bennett's communication, and remarked that the cases related seemed to confirm the views of Dr Green, both as regarded the pathology and treatment of these affections of the air-passages. The frequency, however, of ulceration of the larynx and trachea, as noted by Dr Green and Dr Bennett, was opposed to the observation and statistics of Louis, who had found that of 180 persons who had died of some chronic disease, not tubercular, only *one* had ulceration of the larynx; whereas of those who died of consumption, nearly one in three had ulceration of the windpipe. Ulceration of the larynx and trachea, then, unconnected with syphilis, was scarcely observed, except as a result of phthisis. In regard to the case of Dr B., just related by Dr Bennett, and considered by him as one of arrested tubercle of the lung, Dr Begbie remarked, that the patient in November last had been admitted to him for examination in reference to a policy of life assurance, and that he had found no difficulty in recommending the acceptance of the proposal, seeing that the individual was a healthy-looking man of thirty-five, who declared that he had never been affected with pulmonary ailments; that he belonged to a healthy and long-lived family; that his parents were living, at the respective ages of seventy-one and sixty-five; that of twelve brothers and sisters, ten were also alive, between the ages of twenty and forty-one, two only having died in infancy; that no hereditary taint of tubercular disease could be traced in the family; and that the proposer himself gave no indication whatever of disease in the lung, though his fauces exhibited the remains of the follicular disease of Dr Green, which originated during a residence in Surinam. Under these circumstances, but in ignorance of the opinion entertained by Dr Bennett, he had recommended the acceptance of the proposal, and still held that he was right in so doing, though, had he been previously possessed of Dr Bennett's opinion, he might have advised the extra premium, to which allusion had been made.

Dr Bennett considered the case as quite justifying the recommendation of Dr Begbie. He considered it in fact as an absolute cure of incipient phthisis; and alluded to the observations which he had formerly brought before the Society, as illustrative of the frequency with which that disease underwent cure.

Dr Andrew Wood said, that whilst it was well known that laryngeal disease was a very frequent complication of pulmonary phthisis, and also that laryngeal phthisis was not unfrequently met with independently of pulmonary phthisis, hitherto it has not been generally observed that the larynx was the starting point in phthisis, or that laryngeal affection exerted a marked influence on its early progress. Were it established, as Dr Bennett had endeavoured in his paper to do, that, in the early stages of phthisis, the symptoms were sometimes of a more aggravated character than could be accounted for by any disease that could be detected in the lungs; and that, on examination, these symptoms could be referred to disease of the pharynx and larynx; in that

ease we might expect efficient assistance in such treatment by the local application of caustic solutions, according to the method some years ago introduced by Dr Horace Green. Dr W. was quite ready to admit that Dr Green's plan was an important improvement in medicine; at the same time he could not help thinking that it was too much of a fashion to look for disease in the pharynx and larynx, and that the swabbing system has been too indiscriminately applied. Even in cases where there could be no doubt as to the presence of affection of the mucous membrane of the pharynx and larynx, the treatment by the sponge was by no means invariably successful. But after all, in the treatment of phthisis, whilst we should not neglect the use of every adjuvant for the mitigation of symptoms, our great dependence was to be placed in general treatment; and perhaps there were few greater improvements in therapeutics than the general introduction, of late years, of nourishing and tonic treatment in phthisis. We had come to consider it as blood disease, and theory had now established, on a firm basis, a method of cure which had many years ago been, in some degree empirically, introduced by Dr Stewart, of Erskine. We had now, however, one great advantage over him, in the possession of an article of nutrition,—viz., cod-liver oil, the use of which in phthisis was unknown in his days. Dr W. considered nutritious diet, including cod-liver oil, as the most efficient mode of promoting the abortion of tubercles by cretaceous formation. Dr W. said he had lately met with a case which seemed to him of importance in relation to the matter. A young girl had a strumous glandular swelling of the neck, which was enlarging, and threatening to go through the ordinary course of such swellings, and become a source of disfigurement. Discarding altogether local applications, the treatment by cod-liver oil and nutritious diet was adopted. Under this the swelling gradually diminished, and seemed likely to disappear altogether. At length, when about the size of a pea, it inflamed, a slight suppuration took place, and there was discharged a small cretaceous mass, after which it healed rapidly, leaving a very slight depressed cicatrix, such as is not unfrequently met with in the apex of the lungs. Dr W. said possibly such cases were not unfrequent, but as it was the only one of the kind that he had met with himself, he thought it might not prove uninteresting, as bearing on the subject of the treatment of phthisis.

Dr James D. Gillespie stated a case which he had seen some years previously. The patient (a merchant in London) had been treated there for some years as having phthisis pulmonalis. On his coming to Edinburgh, no pulmonary disease could be detected, but an affection of the larynx and trachea existed, which was supposed to be of a tubercular nature, as the patient exhibited all the constitutional symptoms of phthisis. The autopsy showed the lungs to be quite healthy, with the exception of the apex of the left lung, which presented an adhesion to the thoracic wall, apparently of long-standing. In a small portion of the apex were a few calcareous deposits, the only traces of tubercular disease in the lungs. The epiglottis and rima-glottidis were much thickened, with cicatrices of ulcers on their surfaces. Immediately below the glottis were several large tubercular deposits, while lower down, along the trachea to its bifurcation, were smaller collections of the same character, mingled with flaky lymph, and surrounding, in various places, ulcerated patches. The whole mucous membrane, when not covered by the appearances above described, was in a state of acute inflammation. Below the bifurcation little appearance of disease was presented. Dr G. remarked that he did not think this case would have been much benefited by the application of caustic.[1]

Dr Bennett considered Dr Gillespie's case as interesting, but said that he thought the caustic application might have very probably produced a good effect on the disease.

[1] This case was seen, in consultation, by Dr Alison; and the preparation was such an interesting one, that I think he has it now in his museum. Phthisis laryngea was diagnosed during life.—J. D. G.

Dr Christison said he should be sorry were it to go forth as acknowledged in the discussions of the Medico-Chirurgical Society, that professional men, in this seat of medical learning, were ignorant until a recent date, as one statement by Dr Bennett, and another by Dr Andrew Wood, appeared to him to assume. First, That cicatrices are found in the upper part of the lungs, and supply evidence that tubercular deposition in the lungs admits of cure; and, secondly, That the proper treatment of pulmonary consumption requires, not a spare system of diet, but a generous diet, including animal food, malt liquors, wine, etc. He could bear witness, at all events, that so long ago as between 1827 and 1832, the fact of cicatrisation occurring in the apex of the lungs, was taught by the late Dr Duncan, jun.; that it was also repeatedly seen by himself during the same period in the pathological dissections conducted in the Royal Infirmary, while he was ordinary physician; and that it has been constantly taught by himself since he became Professor of Materia Medica in 1832, both in his lectures on that subject and in his lectures as Professor of Clinical Medicine. He begged also to observe, that, although the treatment of phthisis by a generous diet and exercise, introduced by Dr Stewart, minister, first of Luss and afterwards of Erskine, was in the beginning much opposed by the medical profession, it was eventually adopted long ago in this city, and was never lost sight of afterwards; and, in particular, that it has been regularly taught by himself, in his Materia Medica course, under the branch of dietetics, since 1832, as the accredited treatment in Edinburgh.

Dr Bennett could not allow the observations of Dr Christison to pass without comment. With regard to the cicatrices of tubercular ulcers in the lungs, the question was not whether they ever occurred or no. Every one who had witnessed many *post-mortem* examinations must have seen them; and it is well known they have been considered as the occasional but sure proof of the healing of tubercular caverns. But what he had endeavoured to establish, together with MM. Rogée and Boudet of Paris, was that this occurred much more frequently than was generally supposed, and that that fact constituted an argument for attempting the curative rather than the palliative treatment of the disease. That a generous diet, and a strengthening system of treatment, in certain cases of phthisis, may have been tried by numerous practitioners, was very possible; but he had yet to learn that this practice, previous to 1841, was considered by the profession as the general rule, or adopted on pathological principles. He considered that it was only since cod-liver oil had been introduced into the treatment of phthisis, and the physiological action of that remedy pointed out as a promoter of nutrition, that correct principles of treatment had been ascertained. He was satisfied that when he returned from the Continent, in 1841, phthisical cases were not treated in the Royal Infirmary of Edinburgh generally by increasing nutrition, and he did not believe that the plan of treatment pursued by Dr Stewart had made any impression on the practice of the profession at large. He had been informed that a claim was about to be made in favour of Dr Stewart, as the real founder of correct principles in the treatment of phthisis; this claim, when fairly brought forth, he (Dr B.) would be prepared to meet. At present (said Dr Bennett) after these principles have struggled into existence, and the good effects of cod-liver oil are everywhere acknowledged, those who most ridiculed the remedy on its first introduction, attribute the good results obtained to the labours of Dr Stewart.

Dr Andrew Wood said, as Dr Christison had thrown down the gauntlet, he was ready to take it up, and to maintain that it was only within a few years that the nourishing and tonic treatment of phthisis had been generally followed. He thought it but justice to state, that to Professor Bennett, more than any other physician, were we indebted for this improvement, as it is to him that British practitioners owe the introduction of cod-liver oil. Before the publication of his work on cod liver oil, in 1841, this most invaluable remedy in strumous disorders was unknown; and it was right that it should be generally known that to Dr Bennett is due the merit of its introduction into British

practice. The researches which had of late years been made in regard to nutrition, normal and abnormal, had thrown light on the matter; and by teaching us the doctrine of the tendency of the blood in strumous habits to deposit albumen rather than fibrine, had led us to know the reason why nutritious diet, with the use of cod-liver oil and tonic regimen, were calculated to act beneficially in phthisical cases.

The discussion was at this point checked by the President as irrelevant, when

Dr Christison, after asking leave to reply, said, that he was not previously aware that any such claim had been made on the part of his colleague, Dr Bennett, as it now appeared was advanced for him; for assuredly both the doctrines he had referred to had been long familiar to himself; and he had long considered them as the prevalent doctrines of the medical profession here. He (Dr Christison) was prepared to prove all this by existing notes of his lectures written down in 1832, and still in the same state as when first written.

The Society having expressed a wish to hear Dr Hamilton on the subject of Dr Stewart's treatment of phthisis,

Dr Hamilton remarked, that it was quite true that the morbid anatomy and the pathological doctrines upon the subject of pulmonary consumption were very different at the time when Dr Stewart propounded his doctrines from what they are at the present day; but this circumstance, as it appeared to him, only enhanced the value of Dr S.'s labours; for there was no want at the period of ingenious theories of the ætiology, and not less of the therapeutics of the subject, as propounded, *ex cathedra*, from the press, and as canvassed in the Medical Society by Dr Stewart, and by many of his illustrious associates. It was to the strong current of the prevalent opinions that Dr S. opposed himself, maintaining that those doctrines were substantially and fundamentally wrong, and that they must be encountered by others, not only different, but wholly contrary. In those now distant days the disease was regarded, if not sthenic, at all events inflammatory; and it was taught that antiphlogistic and depleting remedies, the extreme of care and caution, with counter-irritants, etc., were the appropriate remedies. Dr Stewart, on the contrary, maintained that the disease belonged to the very opposite category; that it was a disease of weakness; in a word, that it was strumous, and was to be treated accordingly, not by debilitating remedies, but by nourishing ones, not by digitalis, blistering, issues, etc., not by withdrawing blood, but contrariwise, by supplying more of the vital stream, fresh and wholesome, so strengthening the constitution that it might throw off the enfeebling diathesis. Hence the prominence he gave to air and exercise, to the regulation of the functions of the skin, by ablution and thorough friction morning and evening. Hence the nourishing and strengthening diet, including, in moderation, rich fish, such as herring, meat, with its full share of fat, whether beef or bacon. Hence the hopes he cherished when others despaired; hence his success in not a few cases which were by others regarded as hopeless; hence progress in the right direction, the stable basis of his merit. No mere quackery, no random lucky hit, but the result of careful observation, reflection, and reasoning, useful in introducing, under many disadvantages, a more successful treatment in this still fearful scourge of suffering humanity.

Dr James W. Begbie read a "Case of Acute Rheumatism, succeeded by Chorea and Affection of the Heart;" and followed it by deductions favourable to the view advocated by Dr Begbie, sen., as to the connection of chorea and rheumatism.—(See original communication in this Journal, p. 11.)

A discussion followed, in which

Dr Simpson mentioned some cases in which he had seen the connection of rheumatism with chorea. In one case that he attended some time ago along with Dr W. H. Douglas, the patient, a young lady of seven or eight years of age, after suffering from severe rheumatism in the lower extremities, presented symptoms of chorea, and ultimately, in the course of the disease, was attacked

with sub-acute pericarditis, which proved fatal. The effusion of coagulable lymph on the pericardium, as ascertained by dissection, was extremely great. He saw, some years ago, with Dr Robertson, another case in an adult, in whom, first, there occurred rheumatic fever, followed by pericarditis; and, secondly, when this pericarditis was at its height, symptoms of chorea supervened. The patient recovered, but with a heart much diseased by the attack. He had seen in one family a set of cases similar to those described a few years ago by Dr Begbie. Two children, a sister and brother, were attacked with rheumatism. In the boy, the rheumatism was followed by an affection of the heart, which has proved permanent, and was probably inflammatory in its origin. In the sister, the rheumatic attack, on the other hand, was followed by severe and prolonged chorea, complicated with a kind of hemiplegia. She recovered at last, under the use of arsenic. The investigations which have been lately published by some French pathologists seem to show that the connection between chorea and rheumatism is even more frequent than had been previously supposed, particularly among young children. In the Parisian Hôpital des Enfans, attention having been particularly directed to this subject, more cases had been found of chorea and rheumatism combined among the young patients than of simple, uncomplicated rheumatism; at least, such is the averment of M. Séé. This pathologist further stated, that in the *post-mortem* examination of individuals dying of chorea, between 35 and 40 per cent. of the bodies were found to present inflammatory effusions upon the surfaces of internal serous membranes; and pericarditis was one of the most common of these complications. Dr S. believed that the old explanations attempted to be given of the connection of chorea and rheumatism, as to its being a nervous or sympathetic connection, etc., could not now be sustained; and that the pathological idea of the relation maintained by Dr Begbie was the correct one, viz., that these two apparently different diseases depended upon some identical or analogous blood poison. Dr S. entertained little doubt, that when pathological chemists discovered what the blood poison in rheumatism really was, that the same blood poison, or some modification of it, would be found to be also the cause of chorea, however much the two diseases differ symptomologically. The morbific state of the blood existing in rheumatism and chorea, pre-disposed, as late dissections have shown, to produce inflammation of internal serous membranes. The morbific state of the blood in albuminuria had, in a noted degree, the same tendency, as specially shown by Dr Taylor and others. These two morbid poisons were analogous in another respect. While the poisoned state of the blood accompanying or produced by albuminuria, had a tendency in some individuals to produce local serous inflammations, it tended in others, and particularly in pregnant females, to affect the nervous system in the form of convulsions. So the blood poison in rheumatism, whatever its nature might be, had a tendency, in the same way, to produce internal serous inflammations in some individuals; and in others, particularly in the young, and in the female sex, to affect the nervous system in the form of chorea. Both thus had a tendency to produce, on the one hand, internal serous inflammatory lesions, and on the other, peculiar affections of the nervous system. Perhaps a closer investigation would prove them also to be analogous in other respects.

Dr Andrew Wood said, that from his having had a large number of children under his observation in hospital, he had frequently met with cases of acute rheumatism. In only one case had it happened to him to meet with chorea as a complication of that disease. The case was that of a girl, who was seized with a severe attack of rheumatic fever, for which she was treated with calomel and opium. After the mouth had become affected, pericarditic symptoms appeared, which were combated actively by local blood-letting and counter-irritation. When comparatively convalescent, well-marked symptoms of chorea appeared. After tedious illness, the girl recovered so as to be able to go about; but symptoms remained indicative of organic lesion of the heart; and

in a year and a half thereafter she died. On dissection, the pericardium was found to adhere intimately and generally to the heart. He had adduced this case principally with the view of calling attention to the treatment of acute rheumatism. This case proved that mercurial action did not prevent the supervention of pericarditis, and other unpleasant symptoms. If, therefore, less severe treatment were found efficacious in treating the disease, it would be a great improvement, as saving the constitution from the damage which mercurialization and other strong drugs, especially in weak habits, are so apt to occasion. Dr W. thought that what had been observed in this, as well as many other diseases, was gradually leading to a humoral pathology. There could be no doubt that the blood in acute rheumatism was greatly altered in its properties, containing, as it did, fibrine in excess, and a large amount of acid; and to these circumstances probably was owing the tendency of the heart to become affected, as well as other phenomena, such as the sudden metastasis of the disease from one joint to another, which were difficult otherwise to be understood. The rational treatment, then, was to give remedies calculated to correct the blood disease. For this purpose, in his own practice, Dr W. had found no remedy more efficacious and more safe than one which had been introduced within a year or two, viz., lemon-juice in large and repeated doses. It may not be at present possible to explain exactly how this remedy acts—whether by the potass it contains or otherwise—still the fact of its efficacy appears to be established by numerous published cases. If further experience should confirm this result, an important step would have been gained by the substitution for severe and often dangerous drugs of one mild in its action and pleasant to take. The only untoward effects which Dr W. had observed to follow its use was diarrhœa; but this was easily obviated by the use of small doses of Dover's Powder.

Some further conversation took place as regards several other remedies for acute rheumatism, Dr Bennett mentioning that he had lately employed in hospital practice the nitrate of potash, as recommended by Dr Basham and others, and found its success to exceed his expectations.

EDINBURGH PHYSIOLOGICAL SOCIETY.

SESSION III. (*Winter* 1852-53.)—MEETING I.—*27th November* 1852.

Professor BENNETT, President, in the Chair.

The President observed that the objects of the Society remained the same as he had recapitulated on former occasions; and that there could be little doubt, from what he knew of the exertions of individual Members during the autumn, that the present Session would equal, if not surpass, former ones in interest. He alluded to similar institutions existing in France and Germany, and believed that, before long, societies established to promote limited branches of natural science, would find it necessary to combine, in order even that their own special subjects should be advanced. In the meantime, he again invited all those who were earnestly engaged in prosecuting a knowledge of the structure, development, and functions of living beings in their healthy and diseased conditions, to join the Society, and further the important objects it was established to promote.

1. *Dr Bennett*, after some observations on the effects of adding pus to the blood, alluded to some experiments of Dr Henry Lee of London, who had injected pus into the veins of an ass, and found it to produce coagulation of their contents. He (Dr B.) had repeated this experiment in a cat, with the assistance of Dr James Drummond, but no such coagulation had taken place. Two drachms of healthy pus were, in this experiment, injected into the crural vein, and the animal, after two days of repose, was as lively as before. He announced

that, through the kindness of Mr Barlow, an ass had been procured, in order that Mr Lee's experiment might be again carefully performed; and he proposed that a committee of the Society should be appointed to investigate this subject.

This was agreed to; and the following gentlemen were appointed to report on some future occasion:—Drs Bennett, Gairdner, Cobbold, Sanders, and Mr Barlow.

MINUTE CYST-FORMATION IN THE KIDNEY, WITH GRANULAR DEGENERATION OF THE CYSTIC EPITHELIUM, AND SOME GRANULAR MATTER IN THE TUBULI URINIFERI.

2. *Dr W. T. Gairdner* showed the right kidney of a man affected with tubercular disease of the lungs and intestines. Both kidneys presented considerable atrophy of the cortical substance, connected with (or possibly dependent upon) the microscopic cyst-formation first described by Mr Simon, of St Thomas' Hospital; they were likewise, however, affected with incipient opaque granulations of Bright, not easily visible by the naked eye, but revealed distinctly by the microscope. The following is the description in the Pathological Register of the Royal Infirmary (XV-26):—

"Kidneys unusually small (dimensions of right kidney 3½—1¾—¾ inches; left kidney nearly the same size); capsule easily stripped; surface slightly uneven, not distinctly tuberculated; venous vascularity of surface considerable, but irregular; on section, cortical substance much diminished in breadth (average ⅛th inch diameter from bases of pyramids); limit-line of pyramids tolerably distinct, a faint appearance of opaque granulations appreciable by the naked eye. On examination with a simple lens, very many minute cysts discovered in the cortical substance, mostly requiring a power of ½ inch focal distance to bring them into view. A similar power, or even the naked eye, distinguishes easily a number of opaque light gamboge-yellow points in cortical substance, the largest about $\frac{1}{50}$th of an inch diameter, accurately limited, and yielding, on being punctured, a fluid of similar colour (A). In the cortical substance there are also some minute hemorrhagic petechiæ, having the usual appearance of extraversation in the tubuli.

"With high powers, the tubuli uriniferi seem to be in part of absolutely normal characters, with the exception of a very few granules in the epithelium; in some of the tubes, on the contrary, there are very many granules (fatty). Epithelium of tubes generally normal where the granules are not in excess. In many places, the loculi of the kidney are filled with distinct cystic structures, consisting of a delicate membrane, quite distended and spherical. The contents of the cysts are, in some cases, not to be seen; in other instances, there are indications of an epithelium very different from that of the renal tubuli, being much more transparent and delicate, generally about a third smaller, and the nuclei in proportion smaller. Some of the cysts, which are about the size of the Malpighian bodies, or rather less, show a distinct nucleus about twice the size of a renal glandular cell.

"In fluid (A) were found, under high powers, an immense number of fatty granules, partly loose, partly agglomerated into amorphous collections, partly composing distinct rounded granular cells, up to $\frac{1}{90}$th of a line in diameter, and partly infiltrated into cells of a very fine delicate transparent character, presenting much of the appearance of a tesselated epithelium, and strongly resembling those above described as lining the interior of the cysts.

"In addition to the above morbid appearances, two or three minute miliary tubercles, of the ordinary appearance in all respects, were found in either kidney."

A section of the kidney, by the double-knife, was submitted to the Society, under a power of 240 diameters, as also a portion of the fluid (A), exhibiting the structures above described.

In reference to these preparations, Dr Gairdner made the following remarks:—

I am glad to have an opportunity of submitting to the Physiological Society these demonstrations of minute cyst-formation in the kidney, a morbid appearance by no means uncommon, and one which I have observed in numerous instances since it was brought into notice by Mr John Simon. The correctness of Mr Simon's observations has been called in question; and it has even been asserted, in a work of high authority,[1] that he has fallen into the error of mistaking the normal distribution of the tubuli, as seen in the meshes of the so-called fibrous matrix of the kidney, for a cystic structure. I trust I have been able here to demonstrate to the Society what Mr Simon actually saw, and what he has described and represented in his memoir in the Medico-Chirurgical Transactions, vol. 31. It will be seen that the cysts are perfectly spherical, having altogether the refraction and the characters of a closed membrane distended with fluid, and that their epithelium, owing to its delicacy, and the globular character of the cysts, is with difficulty seen; while on the other hand, the renal tubuli vary in form, have not the distended appearance, and have a very distinctly visible epithelial lining. I do not believe that any one coming to the study of renal pathology with an accurate knowledge of the histological characters of minute cysts, was in the least danger of making the mistake attributed to Mr Simon, whose descriptions show that he perfectly appreciated the difference between the normal structures of the kidney and the cysts described by him. The resemblance between the pathological structures now before the Society and the normal cysts of the thyroid gland, is very striking; and Mr Simon's well-known investigations on the structure of the glands without ducts, placed him in a favourable position at once to appreciate this resemblance, and to distinguish between the appearance of a closed vesicle and an open tube.

By the use of simple lenses, and of low powers of the compound microscope, these cysts can be easily traced down from a size visible by the unaided eye to very small dimensions. Their character remains quite distinct, as remarked by Mr Simon, even when they are not larger than an epithelium-cell, from which they differ by their almost absolutely spherical form, and comparatively high refractive power. They are generally to be found in considerable numbers on the surface of the kidney, and in this situation are easily detected, even when very small, by the naked eye. In a section of the cortical substance, it is often difficult to distinguish these small cysts from the Malpighian bodies, except by examining their sections with the aid of transmitted light and moderately high microscopic powers. The smaller cysts are, therefore, much more easily identified than those of intermediate size, which are apt to be destroyed in making the sections; and this circumstance prevented me, for some time, from being able to assure myself of the transition of the microscopic cysts into those long familiar to pathologists. In writing on the pathological anatomy of the kidney in 1848, I noticed and figured the occurrence of small cysts, which I had observed even before the publication of Mr Simon's memoir, but which I had not been able, for the reason to which I have referred, positively to identify with those described by him. Subsequent observations have filled up the link which was then wanting, and I have now been long familiar with all the appearances presented by these bodies.

The substantial accuracy of Mr Simon's descriptions has been admitted, and his views on cyst-formation in the kidney have been adopted without reservation, by Rokitansky and others. I think it important, therefore, to take this opportunity of stating some points in which my observations differ from those of Mr Simon, and prevent me from concurring in his views of the nature and origin of these cysts, and of their connection with other forms of renal disease.

1. According to Mr Simon, the formation of epithelium in the interior of

[1] Cyclopedia of Anatomy and Physiology. (Art. Ren.)

the cysts is an accidental phenomenon, not found in the larger varieties, and only occasionally present at certain stages of their growth. I have, on the contrary, never found an epithelial lining absent in the larger cysts; and at the different stages of their existence, its characters undergo so little modification, that I cannot but believe it to be an essential element in the development of these structures. In many cases, especially in the smaller cysts, it is difficult to bring it into view, from the spherical refraction of the surrounding cyst-wall, and the delicacy of the epithelium cells; but the absence of epithelium cannot be assumed in these cases, as careful management of the light and of the focus will often detect it where it would otherwise escape attention.

2. The connection of the cyst-formation with primary disorganisation of the tubes and desquamation of the epithelium, or, in other words, with Bright's disease of the kidney, is by no means so constant as is indicated by Mr Simon; nor, indeed, is it, according to my experience, at all more frequent than might be expected from the accidental concurrence of two very common affections. I believe, indeed, that there is a peculiar form of renal atrophy which results from the formation and secondary collapse of these cysts, as there is a well-marked form of enlargement of the kidney which is due to their progressive growth; but both the one and the other is manifestly a distinct affection from the desquamative disease of the renal epithelium, regarded by Mr Simon as the necessary cause of cyst-formation. So much is this the case, that nothing is more common than to meet with kidneys extensively occupied by cysts, in which all the intervening cortical substance presents strictly normal characters, both to the naked eye and the microscope; and, on the other hand, I am fully satisfied that desquamative disease of the kidneys may run through all its stages without a trace of cyst-formation. My observations are, accordingly, completely at variance with much of the reasoning in Mr Simon's paper, and especially with these two propositions, which lie at the root of the whole question of the connection of subacute inflammation of the kidney and cystic disease. "The cyst-formation does not occur idiopathically, but only in connection with diseases which are themselves fully adequate to account for the blocking and bursting of the tubes."[1] "I am inclined to view this process (cyst-formation) as *inseparable* from any such acute inflammation of the gland as may terminate in its contraction."[2]

3. Differing from Mr Simon as to these propositions, I am still less disposed to agree with him in his further generalisation, that the "mottled" and the contracted kidney belong to "different morbid actions, and not to different stages of the same." I have, indeed, been quite unable to identify any form of large or mottled kidney exclusively with fatty degeneration, or to determine the dependence of either upon the scrofulous diathesis. Nor has the contracted and atrophied kidney appeared to me (whether originating in cystic disease or not) to have any such special and exclusive affinity with the "irritative blood-diseases," as is assigned to it both by Mr Simon and by Dr Johnstone. I could adduce instances which are in glaring opposition to both these theories, as well as to other recent hypothetical views on renal disease. To go no farther than the case now before the Society, it illustrates, *firstly*, the combination of the cystic with the fatty degeneration, which, according to Mr Simon, belong, in almost every instance, to different morbid actions; *secondly*, the existence of the cystic disease without evidence of epithelial engorgement, which is considered by Mr Simon essential to its production; *thirdly*, the coincidence of the two hypothetically different and opposed forms of renal degeneration, with pure phthisis, which is presumed to be a cause of the fatty degeneration alone. It would be beyond the scope of my present intention to follow out this very wide subject; but this case is only one of many which occur to me as illustrating these positions.

To sum up these remarks, I would state as my very strong impression, de-

[1] Medico-Chirurgical Transactions. Vol. 30, p. 152. [2] Ibid, p. 157.

rived from the examination of many cases of cystic disease of the kidney, that while it may, as in the present case, be accidentally combined with one or other collateral form of anatomical lesion, it may likewise occur quite uncombined, the cysts springing up and running their course in greater or less numbers in an organ otherwise healthy. The occurrence of tubular engorgement or epithelial extravasation, is therefore in no way essential to their origin. Mr Simon attaches great importance to this last condition; but the bursting of the tubuli uriniferi and the displacement of their cells into the surrounding interspaces, is a condition which I have never been able to verify by observation; and of which it would be not a little difficult, in my opinion, to obtain satisfactory proof.

These observations and deductions seem to throw great doubt on the theory suggested by Mr Simon as to the origin of the renal cysts from normal epithelium. That the development of these structures is extra-tubular, is admitted by Mr Simon; and though here, too, doubts might be raised, yet this remark seems on the whole consistent with observation. The extravasation of the epithelium-cell, therefore, is a necessary portion of this hypothesis of their origin, which derives no support from direct observation, except what arises from the similarity in size and form between the incipient cysts and epithelium-cells. But this similarity is more apparent than real; for I have frequently seen cysts of apparently even smaller size than renal epithelium cells; and when it is considered that the cysts are always fully distended, while the renal epithelium-cell is never found in this condition, the cysts in question must have been *in reality* much smaller than epithelium-cells. Whether this is the ultimate limit, it is impossible to say without still more minute examination. But surely the asserted likeness between the cyst-germ and the epithelium-cell is no greater than the similarity found between all nascent cell-structures; while the differences, even at this early period, and still more in their mode of development, are sufficiently striking to suggest a different origin. That these cyst-germs are parasitic, or entirely foreign to the normal structure of the kidney, appears, from the progress of microscopic inquiry, to be rendered very probable; and, at all events, I believe that the attempts to connect them with morbid processes of the tubuli uriniferi and epithelium have been founded on imperfect observation.

The granular degeneration of the contents of the renal cyst, of which an example is laid before the Society, has not, I believe, been before described, and is interesting as the solution of a form of pathological lesion often recognised by the unaided eye, and not unfrequently mistaken by inexperienced observers for the granulations of Bright in an exaggerated form. As in the present instance, the two lesions may coincide, but this is by no means necessarily the case. I have never yet seen this degeneration (except in one doubtful instance) in cysts of larger size than a sago grain.

ON MORBID ALTERATIONS IN THE MALPIGHIAN BODIES OF THE HUMAN SPLEEN.

3. *Dr Sanders* exhibited and described prepared specimens of the transparent condition of the Malpighian bodies in the human spleen; and remarked on the connection of this state with tubercular and Bright's disease, in the cases observed. The spleens were sometimes softened, but most frequently presented the hard waxy appearance mentioned by Rokitansky, of which a specimen has been described by Dr Handfield Jones in the Reports of the Pathological Society of London for 1852. In a well-marked example of the waxy spleen with transparent Malpighian bodies, a great number of these were found filled with blood recently extravasated, giving the fresh section a peculiar dark-spotted appearance; the effusion was limited to the Malpighian bodies, and preserved their shape; no extravasation occurred elsewhere in the spleen or other organs. The case was one of advanced tubercular phthisis; the liver both waxy and fatty; ulcerations in the ileum; kidneys fatty, with

tubercle mostly on their outer surface; and a yellow tubercular deposit the size of a pea in the spleen.

PECULIAR VARIETY OF FIBROUS TUMOURS.

4. *Dr W. T. Gairdner* showed a number of soft elastic tumours from the size of a lentil to that of a bean, which had been found in the cellular tissue of a man who died of phthisis; the tumours had been very numerous, but had apparently caused no symptoms, and were not, indeed, detected till after death. Most of them were subcutaneous, and some had pushed the integuments before them so as to form projecting and almost pendulous bodies. Others were found in the cellular tissue between the abdominal muscles, and one small nervous twig entering the transversalis muscle passed through one of these tumours about three-eighths of an inch in length, the nerve-tubes being expanded and separated from each other by the tumour, which was probably developed in connection with the neurilemma. The tumours had the structure of an exceedingly fine fibrous tissue, with minute oval nuclei, not more than $\frac{1}{400}$ to $\frac{1}{200}$th of a line in diameter. The section appeared to the naked eye homogeneous and pearly white; it yielded no juice; and it was only by means of a lens that fibres could be detected in it. In this respect, as well as in its soft elastic consistence like that of a well filled down pad on a minute scale, it differed from most of the fibrous tumours commonly described.

MEETING II.—*December* 11, 1852.—Professor BENNETT, P., in the Chair.

1. *Dr Bennett* exhibited accurate copies, from some drawings of M. Bernard, for which he was much indebted to that distinguished physiologist, illustrative of the anatomy and functions of the pancreas in the rabbit and dog, as well as of the vascular connection between the vena cava and vena portæ in the horse. Mr Struthers and Mr Barlow were appointed to examine the connection thus stated to exist between the portal vein and posterior cava in the horse, and to see if any corresponding communications exist in man.

2. *Dr Bennett* exhibited the left lung of a young man who had lately died in his clinical ward of chronic phthisis. It was atrophied and contracted, so as not to be larger than the human fist, and was universally adherent to the surrounding organs by dense fibrous tissues uniting the pleuræ. On section, it presented all the characters described as dilatation of the bronchi by Reynaud and Carswell, and of cirrhosis of the lung by Corrigan; but with the exception of two small cretaceous concretions at the apex, it did not present a trace of tubercle. The opposite lung was voluminous and healthy, with the exception of miliary and infiltrated chronic tubercle at the apex, extending over a space the size of a hen's egg. In this case the patient, two years ago, had exhibited all the signs and symptoms of advanced phthisis, with an enormous cavity in the left lung; and since then careful observation had shown the gradual contraction of the cavity and of the lung, which was supposed to be slowly cicatrising. The individual died, however, from scrofulous abscesses in both kidneys, with excessive discharge of pus and suppression of urine. Dr Bennett was of opinion that the appearances now observed in the lung before the Society, were entirely owing to the absorption of the tubercle and gradual closure of the tubercular cavities. He had carefully examined the lining membrane of the apparent bronchial dilatations, and had satisfied himself that they were destitute of epithelium. He therefore regarded them as the remains of former tubercular abscesses, rather than as bronchial dilatations.

3. *Dr W. T. Gairdner* said that he held no doubt, in this instance, that tubercular cavities had formed and healed. The resemblance of the lung to Corrigan's "cirrhosis," or to what has been called "dilated bronchi," was exactly what he would be inclined to expect in such cases; and he had elsewhere stated (in his Papers on Bronchitis, *Monthly Journal* for 1851) the reasons which induced him to consider almost all the alleged cases of dilated bronchi

as having their origin, like this one, in healed ulcerative excavations. The present case he considered as a remarkably interesting one in relation to this pathological question, as well as in showing one mode of the healing of tubercular cavities. Dr G. referred to a case delineated by Cruveilheir (*Anat. Path*, vol. ii.) in which a tubercular lung had the entire upper lobe excavated and healed, without the slightest diminution in the size of the cavities, while the lower lobes presented all the intermediate stages between recent excavation and the healed cavern. Cruveilheir's remarks in this case and on the different modes in which tubercles may heal, were very instructive in relation to the present and other similar cases.

4. *Dr Dobie* showed a mamma which had been removed by Mr Syme in consequence of being affected with malignant disease. It was also found to contain a cyst bearing the usual characters of a hæmatocele. A demonstration from the malignant part of the tumour was made under the microscope. Dr Dobie also showed a large biliary calculus passed by an aged female.

Dr Bennett observed, that in the demonstration now made under the microscope, groups of cancer-cells might be seen to be embedded in a molecular matrix. These he did not regard as compound cancer-cells, but as aggregations of cells, more or less adherent together by an extra cellular blastema, as described in his work on "Cancerous and Cancroid Growths," pp. 41, 42. He had seized on every opportunity of bringing before the Society any new forms of cancerous growth which had fallen under his observation; and he now described one, which he thought had not previously been noticed. It consisted of large cells, the three-hundredth or four-hundredth of an inch in diameter, containing a varying number of round nuclei, about the three-thousandth of an inch in diameter. They closely resembled some of the large cells observed in the mesenteric glands in cases of typhoid fever, but were two or three times the size. He had seen this form of cell-growth in a soft cancerous tumour, shown to him by Dr Gairdner.

5. *Mr Barlow* exhibited under the microscope a section of the thyroid body in the horse, showing very distinctly the closed sacs in that organ, and the arrangement of the vessels around them.

ON ELASTIC COLLODION. BY M. E. LAURAS.

The following formula, given in the *Repertoire de Pharmacie*, is said to produce a highly-elastic collodion:—

Sulphuric acid, of s. g. 1·847, 300 parts; very dry nitre, 200 parts. Mix together in a stone-ware or porcelain pot, and add of carded cotton 10 parts.

Leave these substances in contact for twelve minutes; withdraw the cotton, wash it with cold water to remove the acid which it contains, and after two or three rinsings, immerse it in water containing 30 parts of subcarbonate of potass in 1000 of water; plunge it again into ordinary water, agitate well, and dry at a temperature of 77° to 86°. The cotton, thus prepared, takes the name of xyloidine, and may afterwards be mixed with the ether and the other substances which form it into elastic collodion.

Elastic Collodion.—Xyloidine, 8 parts; ordinary sulphuric ether, 125 parts. Place in a wide-mouthed flask, and add 8 parts of alcohol of s. g. ·825. Agitate, and then make a mixture of Venice turpentine, 2 parts; castor oil, 2 parts; white wax, 2 parts; sulphuric ether, 6 parts. Heat together the first three substances, add the ether, and combine the two mixtures.—*Pharm. Journal*, December 1852.

IMPROVEMENTS ON HELMHOLTZ' SPECULUM.

Professor Donders, of Utrecht, recommends the following modifications in the construction of this instrument:—First, A single reflector of mirror glass, in place of the series of plates originally proposed by Helmholtz. The mirror should have a portion of its metallic surface removed at the centre, leaving a clear aperture of three lines in diameter. Second, A tube of about five inches long, and one and a quarter inches wide, interposed between the observed eye and the mirror. Third, A light screen of a dark colour fastened to the tube, and preventing the access of artificial light (except through the tube) to the eye of the patient or observer. Finally, The attachment of the whole apparatus to a stand, which can be screwed, at the requisite height, fast to a table. Donders has made comparative experiments with Helmholtz' original apparatus, and the instrument made under his own directions by Epkens of Amsterdam, and finds the latter to be by far the more convenient.—*Nederlandsch Lancet*, Junij 739.

PUBLICATIONS RECEIVED.

On the Action of Medicines. By F. W. Headland, B.A., M.R.C.S. London: Churchill. 1852.

The Emigrants' Medical Guide. By James Fraser, Surgeon; Fellow of Faculty of Physicians and Surgeons, Glasgow; and Government Medical Inspector of Emigrants for the Clyde. Glasgow: Hedderwick and Sons. 1853.

The Ohio Medical and Surgical Journal. November, 1852, Vol. V., No. II.

Skoda on Auscultation and Percussion. Translated from Fourth German Edition, by W. O. Markham, M.D. London: Highley. 1853.

Moral-Sanatory Economy. By Henry M'Cormac, M.D. London: Longmans. 1853.

An Essay on the Poison of the Cobra di Capello. By John Cockle, A.M., M.D. London: Highley. 1852.

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The Resources of King's College, London, for Medical Education. By Robert Bentley Todd, M.D., F.R.S. London: Parker. 1852.

The Dictionary of Domestic Medicine and Household Surgery. By Spencer Thomson, M.D. London: Groombridge. The 12th and concluding Part.

Syphilitic Diseases: their Pathology, Diagnosis, and Treatment. By John C. Egan, M.D., M.R.I.A. London: Churchill. 1853.

CANADA MEDICAL JOURNAL.

We have received a letter from the Editor, inviting us to make arrangements for the transmission of our exchange copy by post. This course we should certainly follow, were it possible to ensure regularity of transmission by a moderate pre-payment. But, unfortunately, the existing postal regulations are of such a nature, that the rate of payment would amount to five or six times the value of each Number sent. We have further to assure the American Editor, that there must be some capricious obstacle to the proposed arrangement, applying to Numbers of the Canada Journal posted in America. Some of these reach us at a reasonable rate of postage, while for others twelve or fourteen shillings are demanded.

In future, our Numbers shall be transmitted through some of the American booksellers in London; and we believe Dr Macdonell will find this the surest and least expensive mode of sending his exchange Numbers of the Canada Journal to us.

EXCHANGE LIST.

British and Foreign Medico-Chirurgical Review,—regularly received.
Chemical Gazette,—ditto.
Medical Times and Gazette,—ditto.
Pharmaceutical Journal,—ditto.
Psychological Journal,—ditto.
The Veterinarian,—ditto.
Provincial Medical Journal,—ditto.
Quarterly Journal of Microscopical Science,—ditto.
Dublin Quarterly Journal of Science,—ditto.
Dublin Medical Press,—ditto.
American Journal of Medical Science,—ditto.
Boston Medical and Surgical Journal,—of late, irregular.
Buffalo Medical Journal,—irregular.
New York Journal of Medicine,—ditto.
Philadelphia Medical Examiner,—ditto.
Canada Medical Journal,—should *not* be sent by post.
Annales Médico-Psychologiques,—regular.
L'Union Médicale,—ditto.
Journal de Médecine et de Chirurgie Pratique,—irregular.
Gazette Médicale de Paris,—regular.
Gazette des Hôpitaux,—ditto.
Révue Médico-Chirurgicale,—irregular.
Bulletin Général de Thérapeutique,—of late, irregular.
Vierteljahrschrift für die Practische Heilkunde,—regular.
Zeitschrift der k. k. Gesellschaft zu Wien,—ditto.
Henke's Zeitschrift für die Staatsarzneikunde,—ditto.
Journal für Kinderkrankheiten von Behrend und Hildebrand,—ditto.
Zeitschrift für Rationelle Medicin, von Henle and Pfeufer,—ditto.
Verhandlungen der Physicalisch-Medicinischen Gesellschaft in Wurzburg,—ditto.
Casper's Vierteljahrschrift für Gerichtliche und Oeffentliche Medicin,—ditto.
Nederlandsch Weekblad voor Geneeskundigen,—ditto.
Bibliothek for Læger,—irregular.
Hygiea,—irregular, and sometimes sent by post.
Illustrite Medicinische Zeitung,—regular.
Nederlandsch Lancet,—ditto.

We beg our foreign correspondents, and the Editors of foreign journals, to address their communications, for this Journal, to the care of Messrs Williams and Norgate, foreign booksellers, Henrietta Street, Covent Garden, London. No parcels sent by post can be received, unless the carriage be prepaid.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Pathology of the Bronchio-Pulmonary Mucous Membrane.* By C. BLACK, M.D., Chesterfield, Bachelor of Medicine, and formerly Medical Scholar in Physiology and Comparative Anatomy in the University of London; Fellow of the Royal College of Surgeons of England, etc. etc.—(*Continued from page* 11.)

I HAVE now taken a view of the structure and composition of the different tissues which enter into the formation of the bronchio-pulmonary membrane; of the purposes which these various tissues serve, in reference to each other and to the membrane which they form; of the use of the latter, with respect to the important function of respiration; of the structure, form, and chemical composition of the bronchio-pulmonary secretion; and of the particular action of certain re-agents in connection with the subjects under examination.

I have next to consider those deflections from health, in the bronchio-pulmonary membrane, which are continually occurring, and which so frequently compromise life. The diseases and lesions to which this membrane is liable may be arranged under—

1. Inflammatory diseases.
2. Diseases for the most part non-inflammatory.
3. Lesions of structure.

Inflammation of the bronchio-pulmonary membrane may be

1. Simple, acute, or chronic.
2. Sthenic or asthenic.
3. Specific.

Acute Inflammation of Bronchio-Pulmonary Membrane.

This form of disease admits of subdivision into—

1. Simple acute epithelial bronchitis.
2. Bronchitis involving the submucous tissue.
3. Cellulitis, or inflammation of the epithelium of the pulmonary cells.

It is not my intention to detail at length the history of individual symptoms of the particular diseases to which the bronchio-pulmonary membrane is liable, but to notice them only so far as they appear to depend immediately on the pathological condition of the membrane, and to indicate that condition. By adopting this method, I hope to divest these papers of matter which, in the detailed history of all diseases, is more or less superfluous; to trace, step by step, the pathological condition of the membrane and its secretion, and thence to infer the proper method of treatment.

First Pathological Condition of Epithelial Bronchitis.

Now, in simple acute epithelial bronchitis, the first pathological change which takes place in the bronchial membrane, is that of inordinate congestion of its blood-vessels. This increased bulk of the vascular system must occupy increased space; and, as the pressure will necessarily be in the direction in which there is the least resistance, the caliber of the air-tubes is encroached upon, and thus a degree of narrowing occurs. Hence arises the sensation of a tightening of the breathing, and of oppression in the chest, referable in general to the sternum and to a space on each side of it. As another consequence of this increased bulk of the vascular system of the membrane, there is pressure on the accompanying nerves, which thus become irritated, and, by reflex action, induce cough. The cough, however, is at this stage of the disease dry, which is owing to a temporary suspension of the supply of the nutritive blastema furnished by the blood-vessels, the vital tonicity of which is excited to the utmost by their distension. This vital tonicity of the capillaries, acting in opposition to the pressure of the blood urged onward by the heart's action, approximates, within the nearest possible distance, the molecules of which their walls are composed; whilst the outward pressure of their blood has a tendency to flatten and to expand such molecules, so that the two causes, acting at the same time, yet in a direction opposite to each other, obliterate for a while the intervening molecular spaces of the capillary walls, and thus prevent the transudation of fluid. In a short time the continued pressure of the blood overcomes the vital tonicity of the capillary walls, the molecules of which are, as it were, pushed apart, and even to a greater degree than in the healthy condition of the vessels, the intervening molecular spaces are restored and enlarged, and exudation is thus poured out more quickly than in the healthy nutrition of the part. But before this state of matters occurs, the absence of all exudation produces a dryness of the epithelial surface of the bronchio-pulmonary membrane, which suffers in proportion to the length of time it is deprived of its nutritive blastema, and which gives evidence of the amount of injury it has sustained so soon as exudation actually takes place.

Inspection of the membrane, at this stage of the disease, detects

increased coloration, which varies from a bright pink to a deep scarlet, the depth of colour being in proportion to the quantity of blood contained in the part.

It may justly be asked, whether this turgidity of the bronchial membrane can produce any alteration in the natural percussive signs of the chest.

Experience determines in the negative; and the explanation is, that the air, notwithstanding the narrowing of the bronchi, is still admitted into the remote pulmonary cells in sufficient quantity to distend them, and to maintain the healthy resonance of the chest. But were our powers of distinguishing between the finest differences of sound more acute than they are, we should, in accordance with the general law which regulates the production and transmission of sound, at least detect some slight increase of dulness over the site of the affected bronchi. This indeed is even now done where the extent of surface affected is great in one or both lungs, or where inflammation attacks the capillary bronchi. In the latter case the tubes are so narrowed by the congested state of their lining membrane, and by the contraction of their circular fibres, that air is not admitted into the corresponding pulmonary cells in sufficient quantity to distend them; in consequence of which there is a partial collapse of their walls, and thus the natural sound on percussion is deadened. This fact may be verified by attention more particularly to the bronchitic attacks of children, in whom a very palpable deadening of the percussion-note occurs over the back part of the lower lobes of the lungs, as a consequence of partial collapse of the pulmonary cells in inflammation of the capillary bronchi in connection with them. Here I may remark, that children are particularly prone to capillary bronchitis in the lower lobes of the lungs; and that the complete, or even partial, collapse of the pulmonary tissue beyond, to which such inflammation may lead, is not unfrequently confounded with pneumonic consolidation. But to this I shall again return, when treating of the pathology of the next variety of bronchitis, on which the lesion in question more particularly depends.

Although, in simple acute epithelial bronchitis, there is no appreciable difference between the percussion-note and that of health, there is a manifest modification of the natural auscultatory signs. The pathological condition of the membrane already described, and the consequent diminution in the diameter of the affected bronchi, cause the inspiratory and expiratory air to be thrown into sonorous vibrations, and hence the production of certain rhonchi.

The smaller the opening through which the air has to pass, the sharper the edges of this opening, provided they are sufficiently tense and rigid, and the more rapid the moving column of air, the louder and shriller is the note produced.

In the application of these conditions to the present pathological state of the bronchial membrane, we find that they are observed at the points or angles of division of the bronchi; for here the mucous

membrane forms a prominent fold, which narrows the diameter of the corresponding tube, and offers resistance to the inward and outward currents of air. To these points, therefore, and to the smallest bronchi, are to be referred the sibilant rhonchi which attend this pathological stage; whilst to the larger tubes, and particularly to the angles of division of such bronchi, the deeper, rougher, and more sonorous rhonchi are to be ascribed.

Such, then, are the phenomena which immediately proceed from the pathological state of the bronchial membrane during the first stage of inflammation. The more general symptoms are those which indicate more or less excitement of the system, but of which I do not here take any special note.

Before proceeding further, it will be proper to glance at the curative indications which the pathological condition of the membrane at this stage affords.

It has been shown that the membrane is congested, dry at its epithelial surface, and that the vital tonicity of its blood-vessels is excited by the unusual distension to which they are subject. The rational indication of treatment, therefore, is comprised in an endeavour—

1. To unload the overburdened capillaries.
2. To prevent their subsequent distension.

These objects may be attained, by reducing the volume of blood in the system, by soliciting its free distribution through the skin, and by acting directly on the affected membrane. The volume of blood is reduced by general depletion (which, in this variety of bronchitis, is scarcely ever necessary), by purgation, and by increasing the secretions generally. The circulation through the skin is augmented by warm clothing, whilst the tonicity of the capillary walls is excited and maintained by the constant breathing of *cold* air. These means, judiciously adopted and continued, will seldom fail to restore the healthy condition of the membrane in from twelve to twenty-four hours. But, to be successful, no intermission of the remedies must take place; nevertheless, it is not necessary, for the perfect success of the case, that there should be continued purgation for twenty-four hours. One brisk purgative, which shall produce four or five liquid evacuations, will in general be sufficient, in conjunction with an uninterrupted maintenance of a vigorous circulation through the skin and the constant breathing of *cold* air. No contra-indications to the use of these remedies can arise; because epithelial bronchitis never occurs in an asthenic form; but, on the contrary, it is invariably associated with sufficient constitutional power to bear well the effects of the above means. The beneficial effect of *cold* air may be inferred by any one who has paid the least attention to the effects of temperature in the same affection of the nasal membrane, constituting, in ordinary language, a "common cold." If a person, whilst experiencing that most unpleasant sensation termed a "stuffing of the nostrils," expose himself, warmly clad, to a cold, dry air, the sen-

sation will, in a few minutes, subside, and he will be able to breathe with perfect ease and comfort; but if he return *immediately* to a warm room, the "stuffing," with all its unpleasantness, very quickly returns. Now, the explanation of these effects of temperature is, that the cold air, by exciting the vital tonicity of the nasal capillary vessels, causes them to contract, to overcome the inordinate pressure of the blood within them, and thus to restore their proper diameter, with which condition all sensation of "stuffing" at once disappears; and that a return to a warm air destroys, in a great measure, this tonicity, relaxes the capillary vessels, and thus favours the re-admission of blood in unusual quantity, whence the immediate return of all the morbid sensations.

Second Pathological Condition of Epithelial Bronchitis.

After the first or congestive stage of epithelial bronchitis has existed for some time, the vital tonicity of the capillary vessels is overcome, and their engorged condition is relieved by the escape, through the intervening molecular spaces of their walls, of an exudation consisting of the watery part of the blood, holding in solution fibrine, albumen, and various inorganic salts. The exudation is, in this particular variety of bronchitis, only slightly in excess of the natural demand. We find, therefore, that the little excess is readily exhausted by the basement membrane and epithelium, which have till now been deprived of their natural supply, and which again observe an activity commensurate with the quantity of nutritive elements which they receive. The recurrence of exudation produces certain changes in the pathological condition of the membrane, out of which particular phenomena again arise.

Thus, by exudation, the conditions necessary for epithelial growth are again restored, mucus is eliminated, and the surface of the membrane is covered with an excess of its natural secretion. At the same time, owing to a reduction in the size of the capillary vessels, pressure is removed from the corresponding nervous filaments, and cough is thereby less troublesome than in the previous stage of the disease. It is now chiefly excited by the presence of an excess of mucus in the bronchi, in consequence of which, although pretty frequent, it becomes more paroxysmal. The partial removal of the congestion restores, to a corresponding extent, the normal diameter of the bronchi, and hence an equivalent reduction in the sensation of tightness and oppression of the breathing.

If, however, the secretion of mucus rapidly takes place, or if accumulation occurs, as generally happens during sleep, the oppression of breathing, until the mucus has been expelled, is even greater than in the first stage.

The percussion-note of the chest still remains as clear as in the previous stage; but, with the pathological change which has taken place in the membrane, there is a particular modification of the

auscultatory signs, indicative of its occurrence. Thus, secretion having taken place into the bronchi, the air, on entering and emerging from the lungs, must necessarily pass through it, in doing which portions of it become enveloped in thin films of mucus, constituting bubbles, which, breaking on the surface of the fluid, generate particular moist sounds, recognised as the mucous and submucous rhonchi. These sounds gradually take the place of the dry, or sonorous and sibilant, rhonchi of the previous stage, and in this respect afford satisfactory evidence of the progress of the disease.

The quantity of mucus at first discharged, directly after the establishment of secretion, is small; but it rapidly increases until four, six, or eight ounces are expelled in twenty-four hours. As the congestion of the membrane is in this way removed, the colour of the latter declines, until with the complete cessation of disease the natural hue is regained. The microscopic examination of the secretion shows this variety of bronchitis to be of the mildest form, and confined to the epithelial coat.

From the absence of all inflammatory exudation in the bronchial secretion, there is, I think, sufficient ground to conclude, that the submucous tissue is not affected, and that the disease consists in an excessive nutrition, arising out of an overplus of nutritive blastema supplied to the basement membrane, and consequently exciting inordinate cell-growth in its epithelial covering. Hence the opinion of Dr Williams, that—"The more intense form of acute bronchitis differs from the milder kind already described in the greater extent of the bronchial tubes which it occupies rather than in pathological character," is not strictly correct, inasmuch as, in the milder kind (epithelial bronchitis), there is presumptive evidence that the submucous tissue is not the seat of exudation; whereas, in the severe kinds of bronchitis, inflammatory exudation invariably occupies that structure.

The secretion, in its simply physical character, resembles at first a thinly viscid fluid, similar to the white of egg considerably diluted by admixture with the solution of an alkali. When, however, a quantity of it is collected together in a vessel, it is sufficiently tenacious to form a mass, which adheres to its sides, and which quits them, on inverting the vessel, in long, ropy, stringy portions or bands.

Microscopically examined, it consists of well-formed mucus-corpuscles, mingled with epithelial or basement patches, floating in a viscid fluid menstruum—the contents of cells which have already liquified. The epithelial or basement patches are chiefly present in the first portions of the secretion, and are caused by a blighting of their cells by the suspension of the natural fluid transudations from the blood through the basement membrane during the previous stage of the disease. Sometimes these patches are formed entirely of epithelial cells which have almost attained a perfect development, but which yet cohere by their edges, as is shown in the adjoining figure.

Fig. 7.

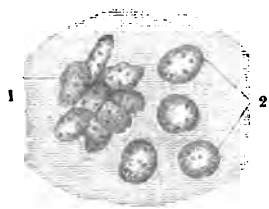

Fig. 7.—1. Epithelial patch.
2. Mucus-corpuscles.

Had such patches of cells retained their position on the basement membrane a short time longer, they would have undergone a further trifling increase of growth, at the expense of the shred of basement membrane which yet intervenes between their edges, after which they would have fallen apart and become separate, isolated, and distinct cells. While thus united, they, in general, measure from $\frac{1}{2800}$th to $\frac{1}{3100}$th of an inch; but when they have attained full growth, and exist as separate and distinct cells, they have a diameter from $\frac{1}{2000}$th to $\frac{1}{2500}$th of an inch.

Their outline is now well-defined, and their surface, which presents a full and globular appearance, shows numerous granules, which have an average diameter of $\frac{1}{18.000}$th of an inch. Acetic acid and liquor potassæ produce the particular effects upon these structures before described.

Occasionally, although not very often, the action of the cilia can be seen on the surface of mucus-corpuscles which have just been expelled. In one instance of epithelial bronchitis, I watched this movement, on the surface of a fully developed mucus-corpuscle, for twenty minutes, at the expiration of which time the action entirely ceased. During this time the movement did not appear to take place exclusively in one direction. For some minutes the action of the cilia was rapid, and in a direction which caused the extremity of each cilium to move in a circle; then this kind of motion ceased for a minute or two, and was succeeded by a backward and forward movement, the cilia during the whole of this time exhibiting a vermicular motion from base to apex. This alternation of movement continued for fifteen minutes, the whirl movement predominating at the commencement, and the to-and-fro towards the close of this period. Both were succeeded, during the last five minutes, by a slow oscillatory motion, which gradually subsided, and at length entirely ceased.

If we compare the chemical composition of the secretion in epithelial bronchitis with that of the secretion discharged by the healthy action of the bronchial membrane, we find that, after the full establishment of the second stage of the disease, it is exactly

similar. When expectoration is just commencing, the secretion contains a less amount of solid constituents than healthy mucus, apart from all admixture with saliva; but after the first flush of discharge is over, the composition of the two secretions is as nearly alike as possible. But the quantity of mucus discharged in epithelial bronchitis, within a given time, greatly exceeds that of health; and in this fact consists the essential difference between the healthy action and the pathological condition of the bronchial membrane now described. The curative indications to be thence drawn have for their object—

1. To diminish the supply of nutritive blastema to the basement membrane, and thereby to limit the amount of cell growth upon its free surface.

2. To restore the vital tonicity of the bronchial capillaries, and thus to enable them to reduce their quantity of blood to their healthy standard.

The first indication is to be fulfilled by as much abstinence as is practicable from food of every description, as well as from drinks, however simple; and by exciting other secretions, by which the afflux of blood to the bronchial membrane is not only lessened, but the actual quantity of blood in the system is diminished. By nice management of this kind, a few days' treatment will effect a cure; and hence the statements of different authors, that the duration of bronchitis is from four or five days to as many weeks. The former period is applicable to the epithelial bronchitis—the latter to that in which exudation involves the submucous tissue; for when once exudation has taken place into the last-mentioned structure, it is impossible for the whole of it to be removed in a few days.

The second indication of treatment is fulfilled by the breathing of a light, dry, *cool* air, which gradually excites the relaxed capillaries to a healthy tonicity. Very cold air, so beneficial in the previous pathological condition, would in this be a great evil; because the vital tonicity of the bronchial capillaries is, in a great measure, exhausted; and the excessive stimulus of cold air, like the excessive application of heat in frost-bite, would only add to the exhaustion, under which they (the capillaries) would again become excessively distended with blood, and consequent partial suppression of secretion and corresponding stricture of the bronchial tubes would arise. To the latter effect spasm of the muscular fibres of the bronchi would contribute, owing to their retaining sufficient excitability to respond to the stimulus of cold air. From the foregoing considerations, we may deduce the general law:—

That, for the healthy manifestation of a function, the capability of action must be proportionate to the amount of stimulus employed.

On this principle, then, moderately cool air is applicable to the second pathological stage of epithelial bronchitis, in which it gradually excites the yet inherent power of the capillaries, and thus

favours their return to a normal condition. To this end blisters likewise contribute, by (1) the derivation, or rather the diversion, of blood to the surface which they excite, and by the quantity of fluid abstracted by exudation, by which the quantity of blood passing through the lungs is lessened; and (2) by a reflex motor impulse transmitted to the bronchial capillaries, by which the contractility of these vessels is again roused to activity. With this view also, mild tonics, coupled with sedatives, are beneficially exhibited, when the general excitement of the system has subsided; because they not only give tone to the general system, and to the bronchial capillaries, by which the latter are enabled to regain their natural diameter, and thereby to furnish a healthy amount only of blastema to the basement membrane, but they at the same time allay irritation, control inordinate cough, and thus prevent sudden congestions of the lung so frequently arising out of the latter cause.

It has already been shown that the alkalies have the property of liquifying mucus, of destroying to a certain extent its viscidity, and consequently of rendering it more easy of expulsion from the bronchi. Their use, therefore, is indicated in every case of bronchitis from the moment of its passing into the second pathological stage. Potassio-tartrate of antimony, on the other hand, when mixed with mucus, clots it in the same manner as do acids, and, like them, it partly destroys the tenacity of mucus. It would, consequently, be a very proper remedy for the second stage of bronchitis, were it not that its powerfully depressing and relaxing influence on the capillary system of vessels would tend to maintain the very condition of the bronchial capillaries which it is the object of the treatment, during the second pathological stage, to rectify. As soon, therefore, as expectoration is fully established, the tartrate of antimony should be discontinued; but, until such is the case, no remedy in the whole range of the materia medica can exert a more beneficial effect in this direction.

For the use of expectorants, strictly considered as such, there is no pathological indication in this variety of bronchitis. Their consideration may, therefore, be deferred until I treat of those pathological conditions to which they are suitable.

Other and minor, yet essential, points in the treatment refer to cough, exercise, use of the voice, and food and drink, during the latter part of this stage, in the management of which the object should be—to keep down vascular action, to prevent pulmonary congestion, and to restore the vital tonicity of the bronchial capillaries. By inducing this last-mentioned condition, a normal quantity of blood only is supplied to the bronchial membrane, the healthy functions of which are consequently restored.

(To be continued.)

ARTICLE II.—*Contributions to Rational Medicine.—Spasmodic Diseases.* By ALEXANDER WOOD, M.D., F.R.C.P., Lecturer on Practice of Medicine, etc., Edinburgh.

Is our treatment of spasmodic diseases rational? A serious question truly, and one which is well worthy of consideration. A practical question also of pressing importance. Judged by the frequency of spasmodic diseases, one which must often force itself on the attention of the practitioner. Judged by the insufficiency of our treatment of these diseases, one to which he is scarcely entitled to give a very decided answer in the affirmative. And yet your routine practitioner is the most obstinate of creatures, once give him an idea that a disease ought to be treated in a particular way—no matter why—and he will go on in this invariable method, never stopping to inquire whether after all it be a right one, and never startled from his dream of security by the small success, or even total failure of the means he most diligently employs. And your philosophical practitioner is somehow now-a-days far too much engaged about the adjuncts of medicine to bestow the attention which he ought on its practical bearings. He is too exclusively occupied with diagnosis, or with morbid anatomy, forgetting that the one is only a means to an end, and that the other is the product, not the essence of disease. I do not undervalue the vast advances which the *organicien*, whether anatomist or chemist, has made in medicine, but like all other good things, his study has its peculiar dangers. Something more is wanting in medicine than *mere* observation. If his be a laborious task who patiently examines minute structure, and acquaints us with the results; still more laborious and of a far higher order of mental elaboration is his, who, classifying the facts thus discovered, seeks to deduce from them practical laws. The tendency at present manifested to overestimate mere structural change, and to undervalue the vital laws which are equally important in the maintenance of life, and in the manifestation of disease, is bringing forth its legitimate fruit. A class of minds is springing up in our schools of a very material order—the scalpel, the microscope, or the chemical reagent, are with them omnipotent, and are looked to as the sole means of revealing the arcana of disease. They believe in nothing which is not demonstrable to their senses; they have a gross appreciation of what the eye can see, or the ear hear, or the hands handle, but by this faith and knowledge are to them limited, and vital laws are only recognised, in so far as they can be explained, by changes in physical properties. Neither the pure Materialist, nor the pure Vitalist is likely to be a successful practitioner. We must avail ourselves to the utmost extent of the information which each supplies, and estimate that information according to its more or less direct connection with practice.

What are called spasmodic diseases, have little to attract the mere materialist. Though alarming in their appearance, violent in their symptoms, and too often fatal in their results, they yet leave little trace after death. The muscles, in the action of which the principal symptoms display themselves, are little susceptible of those changes which, as the product of morbid action, so much delight the morbid anatomist, and are so often regarded by him as the very personification of the disease; and though there is little doubt that the inordinate motions which display themselves, are most usually the result of nervous influences, yet we are still unable to determine whether a change of nervous influence implies a change of nervous structure, and have hitherto failed to discover any modifications of their delicate fibrils at all capable of explaining the symptoms observed during life. In this dilemma, we have only one course to follow. It is a very safe one, and one which might with advantage be oftener pursued, even in those diseases where morbid anatomy can do most for us. It is to examine the natural action of the parts affected by disease in its various relations. These are chiefly muscle and nerve. We are too apt to regard spasmodic and nervous diseases as convertible terms, and to look upon the nervous system as that in which the derangement is exclusively seated. But is this the case? If it be, why does a spiculum of bone produce epilepsy in one case and not in another? Why is a wound followed by tetanus in one case and not in another? Why do acrid secretions in the bowels produce chorea in one patient and not in another? or why does the pressure of a tooth on the gum at one time cause the infant no inconvenience, and at another plunge it in violent convulsions? It is evident that the exciting cause is not all—a predisposition must exist, and where is this predisposition to be sought for? "In the nervous system" answers the routine practitioner. It may be so; the proposition is one that scarcely admits of dispute. Through it certainly most of the exciting causes operate, and, often with the rapidity of the lightning's flash, transmit their direful effects to the whole flesh (muscular system) of the body. But are we to allow nothing to the muscle itself? Is it—the only constituent of the body which has an inherent power of motion—a mere passive instrument to be played upon at will by the nerves? All analogy forbids the supposition, and I am inclined to think that the predisposition to many spasmodic diseases exists in the muscle, while the exciting causes are chiefly to be sought for in the nerves.

Let us then endeavour to study the pathology of convulsive diseases in connection with the physiology of muscular action, arranging our ideas under the three following heads:—

I. TERMINUS—*Effect.* Contractility. Its nature.
II. MOTUS—*Power.* Agents exciting contractility.
III. MODUS—*Manner.* How it is produced.

The advantages of such an arrangement will appear from the following considerations:—

First, Disease is never an entity—a separate existence—an individuality, which we can examine and analyse, and describe as we do a plant, or mineral, or animal; but,

Second, Disease is in every case a modification of some vital action or series of actions, and these must be understood in their healthy condition before their alterations under disease can be comprehended.

Hence the necessity for the study of the *Modus*.

Third, All vital processes may be resolved into reactions occurring on the application of stimuli; each organ having a stimulus peculiar to itself. In disease the *Modus* or reaction is often normal, as regards the stimulus applied, but the stimulus itself is abnormal, and hence the reaction excessive. This can only be understood by a proper appreciation of the natural *Motus*.

Fourth, Disease is not an act but a work;—not a single operation but a complicated process. Why is this? Evidently because vitality manifests itself in a series of processes. Life, as a whole, is a series of processes, and each of these processes (functions) is a series too. Wheels within wheels. Touch a part of any one of these processes, so as to modify it, the effect will not be limited to the single action with which you have interfered, but will be transmitted through a long series, till it shows itself in some modification of the ultimate effect to which they are subservient.

Hence the *Terminus* demands study, and with it we shall commence. Analysis suits the subject better than synthesis.

I. TERMINUS—EFFECT—MUSCULAR CONTRACTILITY.

A. *Physiological Proem.*

I. *What it is.*—It is an endowment of muscle, as muscle—a *vis insita*—its own its independent function. Moreover, it is the property of living muscle, like all other vital properties—the more living the structure the more energetic the function.

II. *What it requires.*—1st, In its performance it exhausts nourishment; therefore in its performance it must demand nourishment. This it receives from the blood, and the blood in its turn obtains supplies by *nutrition*.

2d, Muscle also demands a vital stimulus—oxygen. This too the blood supplies by its red particles, and this the blood receives by *respiration*. Hence, where much muscular exertion is required, much blood is demanded, and that blood in a higher state of oxygenation. This is seen (*a*) in the individual, and (*b*) in classes of animals. *a*. In the individual, violent muscular exercise accelerates respiration, and subsequently demands an increase of food. *b*. In classes of animals we find that a direct relation subsists between muscular power, the quantity of blood sent to the muscles, and the degree of oxygenation of that blood. Thus animals may be arranged in this respect as in the following table:—

I. Birds.	IV. Reptiles.
II. Insects.	V. Fish.
III. Mammals.	VI. Crustacea.

3d, The more strongly an animal, or part of an animal lives, the more readily it dies, *i. e.*, the more dependent it is on rapid nutrition, the more quickly does it perish when that nutrition is arrested. Stop the nutrition or respiration of an animal, and it will be found that muscular contractility disappears exactly according to the place it would hold in the foregoing table—birds losing the power soonest and crustacea last.

4th, The law of irritability, or that of re-action on a stimulant, is just the counterpart of the law of contractility, and, therefore, as has been well shown by Dr Marshall Hall, is inversely to the rapidity of the circulation, and of the respiration. The less blood and the less oxygen a muscle receives, the more easily is it excited to contraction. "Such is the irritability of the muscular fibre of the frog that it and the galvanic agency are mutual tests of each other.[1]"

These four physiological laws will be found to have important pathological analogues, especially in reference to the predisposition to convulsive diseases.

B. *Pathological Deductions.*

To keep a muscle healthy, it has been shown that it must have nourishment and oxygen from the blood, and that it is therefore dependent on nutrition for the one, and on respiration for the other. 1st, In almost every spasmodic disease, assimilation is interrupted. Take, for example, CHOREA.—"In the advanced period of the disease," says Dr Hamilton, "flaccidity and wasting away of the muscular flesh take place, the consequence of constant irritation, of abating appetite, and impaired digestion, the common attendants of protracted Chorea."[2] Or take LARYNGISMUS STRIDULUS. How commonly is it associated with impaired nutrition and with well marked derangements of the digestive organs, and how impotent do all drugs prove for its cure, unless associated with pure air, and carefully regulated diet. Or take SPASMODIC ASTHMA. "Of all the predisposing causes of asthma," says Dr Forbes, "dyspepsia in some of its forms or consequences is by far the most frequent. * * * * It is only, however, in what may be termed secondary dyspepsia, or that general disorder of the system which is often the consequence of long continued irritation of the chylopoutic organs, that results of the kind now contemplated arise."[3] Or in INFANTILE CONVULSIONS. The opinion is gradually gaining ground, and has been prominently advocated by Dr W. L., Mauthner of Vienna, that convulsions comparatively seldom occur

[1] Bostock, quoted by Dr Marshall Hall.
[2] Observations on the Utility of Purgatives, p. 69.
[3] Cyclopædia of Medicine. Article, "Asthma."

in children as the primary results of disorder of the nervous system, but are very frequently to be traced to an interruption between the nutrient parts and the parts nourished, inducing irritability of the muscles.[1] In corroboration of this opinion, we may cite the facts collected by M. Bouchut, which clearly shows that in ten out of eleven children who died at different periods after convulsive seizures, the brain presented no morbid appearance whatever.[2]

2*d*. Impeded respiration, and consequent deficiency of the red particles of the blood, is often associated with spasmodic diseases. How often is the bronchitis of infancy closed with convulsions! and how frequently do we fail to detect any adequate cause for this in the state of the nervous centres! "It goes to the head," we say, but where is our proof? It goes to the blood would perhaps be a more satisfactory exclamation. Again, a careful examination of the Parliamentary reports on the state of large towns, serves to show that the habitual inhalation of the impure air of these localities arrests the muscular development, and gives a greater liability to spasmodic diseases.

3*d*. The third physiological law went to show that the more rapid the nutrition, the more sudden the exhaustion consequent on its arrest. All muscular action demands nutrition, and as the supply is not constant it is soon exhausted, which explains (*a*), the excessive exhaustion produced by all spasmodic seizures, and (*b*) the great and sudden collapse by which the paroxysm is so often followed. And perhaps also (*c*) the enormous appetite which we often see in convulsive diseases after the attack, as, for example, epilepsy.

4*th*. The fourth law showed that the irritability of a muscle was inversely as its contractility—that as its strength declined, its irritability increased. From this, several pathological inferences may be deduced—(*a*). Why muscular weakness should predispose to convulsive disease—(*b*). Why convulsive diseases should be especially frequent in the young—(*c*). Why losses of blood and anæmia generally, should so often give rise to convulsive affections—(*d*). Why the frequency of convulsive seizures, and therefore the severity of the disease, is not a sign of the patient's strength but of his weakness.

C. *Etiological Deductions.*

I limit the inquiry, of course, to predispositions. Laws 1 and 2 teach us the effects of mal-nutrition, impure air, and diseases of respiration, in producing convulsions. Law 3 explains why full health wards off convulsive diseases, but renders them more fatal when they do occur. Law 4 explains how excessive losses of blood and weakness generally, serve to induce convulsive diseases, and how in persons of lax muscular fibre, spasmodic affections are liable to occur on the application of very slight irritants.

[1] See Br. and For. Med. Rev. Vol. xxi. p. 391.

[2] Man. Prat des Malad. des Nouveaux Nés, etc., etc. Par E. Bouchut. Paris, 1845.

D. *Therapeutical Considerations.*

These, of course, are restricted to the subject of prophylaxis.

1*st.* The impaired nutrition suggests the propriety of attention to the digestive organs in all cases of anticipated spasmodic seizure. 2*d.* The state of the red particles, the carriers of oxygen, explains the action of the salts of iron so successful in some of these diseases in the hands of Elliotson and others; the researches of Andral and Gavarret having shown that by their use the red particles of the blood are increased. It also teaches us the value of change of air. 3*d.* We learn from this law how cautious we should be in the use of the lancet in diseases whose danger often seems to consist in the rapidity with which they exhaust the nutritive supply, and the advantage of pouring in nutriment as fast as it can be assimilated in all cases where this exhaustive process is at work. 4*th.* A knowledge of the existence of increased irritability, and of its augmentation as the exhaustion of the disease proceeds, should make us very carefully guard our patients against every source of irritation, whether mental or corporeal. From it we also learn the importance of exercise, the great strengthener of muscular fibre, in the intervals between spasmodic seizures. "The epileptic," says Dr Cheyne, ought to be trained so as to be in good wind, or, in other words, his muscles ought to be in a state of the utmost strength and firmness. If we permit ourselves to take a lesson from empirics, we may surely avail ourselves of such information as may be derived from a brotherhood at least as respectable, namely, the "gentlemen of the fancy," as they are called, or we may have epileptics trained as the athletæ in ancient times."[1] I shall attempt to reduce this to a Table:—

	Physiological.	Pathological.	Etiological.	Therapeutical.
1.	Dependence on nutrition.	Connection with malassimilation.	Effect of indigestion.	Attention to diet.
2.	Do. do. on oxygen.	Do. with diseases of respiration.	Do. of impure air.	Use of iron. Change of air.
3.	Power exhausted in proportion to the amount of nutriment demanded.	Speedy exhaustion and hunger by which attack often followed.	Full health wards off attacks, but makes them more dangerous.	Caution in depletion. Attention to prevent exhaustion.
4.	Irritability increases as strength (contractility) declines.	Why so often associated with weakness, and occurring in the young.	Why arise after losses of blood, and in persons of lax fibre.	Avoid all irritation during paroxysm. Importance of exercise in the interval.

II.—Motus Power.—Agents exciting Contractility.

In the first division of the subject it has been attempted to show, that whatever may be the exciting causes of spasmodic disease, to effect the production of one of these diseases, a peculiar state of the muscular system must exist; and that as these three—muscle, blood, nerve—

[1] Cyclop. of Practical Medicine—art. Epilepsy.

concur in the production of healthy muscular action, so are they equally concerned in that which is diseased. Passing then from the consideration of the predisponent, and taking a survey of the exciting causes, the first thing with which we are struck, is the vast variety and apparently opposing character of these. Some of these causes are mechanical, some chemical, some vital, and another class mental, and this affords a groundwork for division. But again, when we have grouped these causes under the several heads so indicated, it may be observed, that those ranged under each head admit of a two-fold division, according to the nature of its action. They must all act by modifying the vitality of the tissue to which their operation is directed. It is the office of one class to exalt, and of another to depress, that vitality, which serves as a foundation for another division of each class into stimulants and sedatives. These terms are not used with reference to the effect of the agents on the vascular system; primarily, they seem to affect the nervous tissue, though subsequently through it they affect the contractility of the heart, and thus the circulation at large.

Our further investigations into the modus will be facilitated by a tabular arrangement of these ascertained excitants of spasm.

Mechanical....	Stimulant.....	*a.* Insolation. *b.* Tumours or spicula. *c.* Chronic alterations. *d.* Electricity (in small doses).
	Sedative.....	*e.* Shock from violent injuries, etc. *f.* Electricity (in large doses). *g.* Loss of blood.
Chemical.....	Stimulant— in small doses.	*a.* Alcohol, ether, chloroform. *b.* Strychnia, brucea, etc.
	Sedative— in large doses.	*c.* Lead, arsenic, mercury. *d.* Opium, camphor, etc.
	Sedative.....	*e.* Aconite, hyoscyamus, prussic acid. *f.* Belladonna, stramonium, tobacco. *g.* Digitalis, antimony, ipecacuan.
Vital.....	Urea in blood— Sedative.....	*a.* Gout and rheumatism. *b.* Ischuria renalis. *c.* Bright's disease. *d.* Cholera. *e.* Puerperal convulsions.
Mental.....	Stimulant.....	*a.* Emotions.
	Sedative.....	*b.* Emotions.
	Neutral.....	*c.* Volition. *d.* Imagination. *e.* Imitation.

III.—MODUS—HOW THEY ACT—OPERATION OF CAUSES.

A Physiological Proem.

Examining the foregoing table, it will be found that, except the vital agents, which are all sedative, each of the other divisions is

divisible into two classes—stimulants and sedatives. Is it possible to explain the manner in which these operate by a consideration of physiological laws? The attempt must at least be made.

1st. *What is an Irritant?*—The consideration of this is a necessary prelude to that of exciting causes. Every agent, external or internal, mechanical or chemical, corporeal or mental, which, when applied in any way to the living body, directly or indirectly affects its vital actions, is an irritant. Each organ has its own irritants, modifying its own vital actions. Some irritants are general, increasing the vitality of the whole system.

2d. *Re-action on the Application of an Irritant.*—Every organ in a healthy condition has a certain amount of tone or power of action, which is modified by the application of the irritant. It has also the power of preserving the vital composition of its structure when disturbed by an external agent. The operation of the irritant is therefore followed,—first, by a change in the condition in the same way as in an inorganic body; and, secondly, by a return to its normal condition which is peculiar to the organised body.

3d. *Relation of Re-action to the Circulation.*—These changes are of a vital character, and imply therefore changes in the composition of the organ. To supply the combinations and decompositions which are caused by this exercise of vitality, oxygen and nutriment are demanded. We may illustrate this by the comparison of Richerand, who represents the vital actions as combustion requiring oxygen to unite with the burning body, and combustible matter to supply the flame, the whole resulting in the evolution of caloric. In the same way, when the vital force is consumed by increased action, a demand for vital nutriment is made, and an increased afflux of blood, and a more abundant conversion of blood into organised matter, takes place at the seat of the vital action. "Tiedemann remarks, that an organ in an excited state undergoes more rapid changes in its material composition, and therefore attracts more quickly, and in greater quantity, the blood, which alone is able to render an organ capable of increased vital action."[1]

4th. *The kind of Re-action depends on the Function of the Organ.*—Every excitant, even the same, will produce motion when applied to a muscle; pain in a sentient nerve; the perception of light in the optic nerve; and of sound in the auditory nerve; hence we see why the vast number of excitants contained in the foregoing table produce the same effect. It is sufficient that they all act on muscle. The vital property of muscle is contractility; hence whatever agent alters the vitality produces contraction.

5th. *Contractility a Re-action, and how it is produced.*—Muscular contractility is a re-action, normal or abnormal, as the case may be. The excitor being some irritant—mechanical, chemical, vital, or mental, the part on which it operates being usually the peripheral distri-

[1] Physiologie, tom. i., p. 326.

bution of the sensitive nerves; the agent in its production, the spinal cord, acting on a peculiarly excitable condition of the muscle.

6th. *The Irritant may be either a Stimulant or Sedative—attempt to ascertain why both operate alike.*—An examination of the causes, as set down in the foregoing table, proves this; but it also proves what I believe to be of vast practical importance, that the great majority of the exciting causes are sedatives, and not stimulants, as is generally supposed. However opposed these two classes of agents may apparently be, and although they are usually regarded as antagonistic, yet it is a singular fact, that their secondary effects on the vital actions is very similar. By both, the relation between the energy of the organ and the nutritive supply is altered. By the one, the vital energy is increased, so that it is in the relation of *plus* to the nutrition. By the other, the vital energy is diminished, so that the nutrition is *plus* to it. But in the former case, after a short time, the nutritive supply becomes exhausted, and the vital energy becomes *minus*. The final result is thus the same; in both cases diminished vital energy, the nutritive material, however, being *minus* in the one and *plus* in the other. It is evident, then, that every excitant, whether stimulant or sedative, changes the relation of the part on which it acts to the blood.

7th. The locomotive actions (functions of the voluntary muscles) in health, are under the control of the will, and only incidentally affected by reflex agency. They are thus controlled by the cerebrum. On the other hand, the excito-motory phenomena are not connected with the will, and in fact are most perfectly manifested where the power of the cerebrum is interrupted, and that of the spinal system alone retained. Thus various experiments by Müller, Marshall Hall, and other physiologists, have shown, that when the power of the brain proper is suspended by narcotisation, or cut off by division, the animals so treated have their muscles thrown into violent convulsions by the very slightest irritant.

8th. Two sources of action, almost antagonistic, are thus seen, one mental—Spontaneity; the other corporeal—capability of being affected by stimulus, Receptivity; I say almost antagonistic, because, as the former declines, the action of the latter is manifested.

9th. Excito-motory phenomena are peculiarly easily produced in cold blooded animals, or in the young of warm blooded animals. In short, in those in which, as has been formerly shown, the irritability of the muscles is great, and their contractility, and therefore their dependence upon vital stimuli small.

10th. The spinal cord is the essential link between the centripetal impression conveyed by sensory fibres, and the centrifugal influence of the motor nerves. It may excite to action,—

a. By direct irritation.

b. By irritation of sensory fibres reflected through the cord on motor nerves.

c. By sympathy.

B. *Pathological Deductions.*

1st. Any agent which has power to modify the action of a muscle, either directly by application to its tissue, or indirectly through the nervous or vascular systems, may act as the excitant of disease in it. Of these two indirect channels, we are inclined to think that the nervous is the one through which the influence is most frequently conveyed. Indeed, even admitting all that has been alleged, in reference to the absorption of various poisons by the blood, it seems evident that the blood can only act as the channel of their conveyance, either to the nervous centres or to the affected muscles themselves. The whole phenomena of spasmodic diseases seem, however, to point to the nerves as the sources of the disordered action.

2d. The effect of the modification so produced displays itself by an attempt on the part of the organ affected to return to its normal state, and the manifestation of that attempt is convulsive motion.

3d. As the disturbing agent can only operate by affecting the vitality of the part,—that is, by altering the relation between it and its supply of nutriment and stimulation, so it produces disturbance of the circulation. This may be a cause as well as a consequence of the perturbation; for it is plain that a preternatural afflux of blood, either to the nervous centres or nervous peripheries, may itself act as an irritant, and prove the cause of the subsequent changes; or, on the other hand, the irritation, if persistent, must sooner or later affect the blood, and through it every organ depending on it for nutrition. To strengthen the weak and flabby flesh of an indolent patient, you insist on a due amount of exercise being taken. The vital energies being called into play, more of the sustaining element is demanded, and it is received; but the heart thus transmitting it is only a retail dealer; and it must call upon the manufacturers (digestive and respiratory organs) for an increased supply. This briskness of trade again demands increase of raw material. For this the appetite loudly calls; and thus throughout the whole body signs of renewed impetus are manifested. A picture the very reverse of this might be drawn of those cases in which the muscles, having acquired unnatural irritability, respond to the most trifling stimulus. Their tone is weakened, and the demand for the nutritive interchange increased; the supply, however, being deficient, the blood suffers, and thus every organ of the body, but especially the nervous system, becomes affected, and ultimately a painful state of nervous exhaustion, with muscular irritability, is produced. Sad examples of this kind must be familiar to every practitioner.

4th. The effect of any irritant being, as has already been shown, only to rouse the vital actions of the part to which it is applied, it is obvious that whichever of the irritants enumerated under the head of "Motus" be employed, its sole effect will be to produce muscular contractility more or less developed, either in degree or in extent. On the other hand, whatever curative agents we employ, if they

have any virtue at all, it can only be manifested by their acting as irritants also, and thus producing in healthy persons modifications of the vital force similar to those which induce disease. This accounts for all the supposed examples of the law "similia similibus curantur," which Hahnemann has so diligently culled from the writings of the orthodox school, many of which will be found to have reference to those very agents which have been discussed under the head of Motus.

5th. The nutritive interchange is so essential to all vital action, that it is modified by the slightest increase or diminution of functional activity in any organ. This is an important fact; for, *a*, It serves to show on what foundation the opinion of those rest who maintain, that all convulsive diseases arise from congestion, or the reverse. *b*, It often enables us to measure the amount of disturbance which exists. *c*, It shows us that, although congestion may be present, yet that, as that congestion is not the cause but the consequence, we are not to expect that its removal will effect a cure. *d*, It teaches us that that afflux of blood, in whatever way it may be produced, will of itself act as an irritant; and, *e*, It explains the force of the old Hippocratic maxim, "that convulsions arise from too much or too little blood in the cranium."

6th. Those experiments appear peculiarly interesting which show that, in order to the full manifestation of excito-motory phenomena, the connection between the cerebrum (organ of volition) and the spinal chord (organ of excito-motion) must be suspended. They also show how this mere suspension serves at once to make the body peculiarly susceptible of every unnatural irritation,—a fact which appears to explain the operation of many of those agents included in the table of the Motus. Does not this also show how, in the less severe spasmodic diseases,—hysteria, for example,—a powerful effort of the will may overcome the spasms?

7th. Another fact seems also brought out by the same experiments: that the ordinary sensibility of parts seems to be suspended prior to the manifestation of the excito-motory phenomena. This indeed, is involved in the interruption of the cerebral functions, with which what is called mental in sensation must, of course, be associated.

8th. While, however, it appears essential to the manifestation of these phenomena that volition and sensation should be suspended, it is equally evident that the mind occasionally performs the part of the irritant, as indeed is seen by reference to the mental causes of these diseases.

9th. It has also been shown, that these experiments took more effect on cold-blooded animals, or on the young of warm-blooded animals,—a fact which is easily explained by the greater irritability of their muscles, and which refers us back to what has already been said under the head of "Terminus," and brings into beautiful harmony the predisponent and the exciting causes of spasmodic diseases.

10th. It would thus appear that, when muscular irritability has been induced by any of the predispositions, it is only further necessary that volition and sensation should be temporarily suspended, in order that any irritant, however innocuous at other times, should throw the muscles into spasmodic action. It only remains, therefore, to complete this branch of the subject by showing that the agents enumerated in the table of motus have this effect. I confine this table at present to the sedatives:—

Effect of the various Sedative Agents exciting Convulsions.

AGENT.	EFFECT ON VOLITION	EFFECT ON SENSATION.	AUTHORITY.
Shock from severe injuries.	Impairs volition.	Impairs sensation.	Dr Alison; Dr Copland.
Electricity.	Suspends it; coma.	Suspension of; insensibility.	Dr Alison; Dr Hartmann.
Loss of blood.	Functions of brain, consciousness suspended.	Ditto.	Dr Marshall Hall.
Alcohol, etc.	Depression of functions, especially those of external relation.	Insensibility.	Dr Christison.
Strychnia.	Stupor.	Stupor.	Dr Bally.
Lead.	Delirium, etc.	Coma.	Case of Dr Corsin, etc.
Arsenic.	Stupor.	Insensibility.	Dr Dymock, etc.
Opium, etc.	Suspension of volition.	Ditto.	Dr Pereira.
Aconite.	Muscular debility.	Impaired sensibility.	Dr Fleming.
Hyoscyamus.	Paralysis.	Insensibility.	Dr Pereira.
Prussic acid.	Extremities paralysed.	Extinguishes sensibility.	Dr A. T. Thomson.
Belladonna.	Placed on legs cannot stand.	Insensibility.	Dr Taylor.
Tobacco, Stramonium, etc.	Paralyses the heart, etc.	Operates on the sensibility.	Dr A. T. Thomson.
Digitalis, etc.	Diminished muscular power.	Insensibility.	Dr Pereira.
Sedative emotions.	Syncope. Absence of volition.	Syncope. Insensibility.	Dr Alison; Dr Carpenter.

11th. To sum up these observations on the Modus or manner in which the known exciting causes of spasmodic diseases operate, it is to be observed,—*a*, That the ascertained predispositions infer a state of the muscular system implying diminished contractility with increased irritability. *b*, That almost the entire exciting causes are sedative in their operation. *c*, That those causes which act as stimulants produce, like other stimulants of the nervous system,—*First*, a brief period of excitement; *Second*, a state of exhaustion, with excitability; *Thirdly*, a state of atonic exhaustion; and that it is not until the second or third of these effects have been produced, that the convul-

sions are displayed. *d*, That it is on the nerves of sensation chiefly that the sedatives operate.

12th. It may thus be assumed, that the best recognised causes of convulsive diseases act as sedatives; and by suspending volition and sensation, bring the patient into a state resembling that of the narcotised or decapitated animals in Dr M. Hall's experiments.

13th. That while common sensibility is thus blunted, there is a morbid susceptibility to those impressions which operate through the excito-motory system.

14th. It thus appears that the name exciting causes is scarcely properly applied to the majority of those which we have so termed; for their action only tends to bring the muscles under the operation of the excito-motory system, leaving it to other irritants to operate through it on the muscles. The channels by which these act we attempt to show in the following table, chiefly compiled from the valuable memoirs of Dr Marshall Hall:—

Excito-Motory Origin of Convulsive Diseases.

IRRITANT.	SEAT OF IRRITATION.	EFFECT.	DISEASE.
Ipecacuan vapour, cold air. Indigestible substances. Effete matters.	Muc. memb. of bronchi. Do. of stomach Do. of bowels.	Contraction of bronchi.	Asthma.
Emetics. Rotatory motion. Irritation. Do. Do.	Brain. Of fauces. Of stomach and bowels. Of uterus.	Contraction of stomach and abdominal muscles.	Vomiting.
Poison in blood.	Brain.	Conv. of vol. muscles, especially of throat.	Hydrophobia.
Do.	Do.	Do.	Convulsions.
Irritation. Indigestible matter Effete matter.	Muc. memb. of gums. Do. of stomach. Do. of intestines.	Conv. of glottis, diaphragm, etc., muscles of hand & foot	Laryngismus stridulus.
Injury. Irritation.	Incident nerve. Brain.	General spasms. Contraction of sphincters.	Tetanus.

C. *Therapeutical Considerations.*

If the foregoing conclusions are admitted, they show our present treatment to be sadly irrational, and furnish important indications for its improvement. But, indeed, whether admitted or not, the

treatment ordinarily pursued is so very contradictory as to prove, that it at least cannot be right. In the severer convulsive diseases, as tetanus and hydrophobia, wine, brandy, and opium,—stimulants, —are conjoined with the cold affusion or cold bath,—sedatives. Opium in small doses, which as a stimulant and soothing remedy might prove useful, is counteracted by tobacco, a sedative, or by irritating and depressing purgatives. We are told to administer the most violent purgatives in these diseases, and are encouraged to persevere in their employment to an enormous extent, by the hope of eventually obtaining from the bowels an assortment of strange and heterogeneous matters.

In these diseases, where the violent muscular effort demands a large supply of blood, and where the want of that supply increases the irritability of the system, we are taught to bleed, beginning with from thirty to forty ounces, and repeating it if need be. What end can blood-letting possibly subserve, unless in those exceptional cases, to be afterwards specially considered, where urea exists in that fluid? There is no morbid matter in the circulating fluid which bleeding can remove! There is no inflammation which it can subdue! On the contrary, that terrible muscular action, like a consuming fire, is draining away the nutriment of the system even faster than your relentless lancet, and when the supply is exhausted the flickering taper will expire.

Or, in the case of an unhappy infant,—ill thriven, ill fed,—who has imbibed irritability of constitution with the milk of a gay, luxurious, self-indulgent, and nervous mother, and who has had that irritability confirmed by a long course of ill-regulated management, —why will you, when the irritation of a tooth has proved the cause of convulsions, not rest satisfied with removing the immediate pressure, and then set about improving the general system, in which the real danger lies, but apply cold to a head where no symptom of congestion or inflammation is to be found, harrow the gums with daily scarifications, and administer a powerful purgative, glorying in the expulsion of depraved secretions, which only indicate the impaired state of nutrition, and the necessity for a very opposite kind of treatment?

But enough of these comments. Let me very humbly but very earnestly solicit the attention of my professional brethren to the following therapeutical considerations, based on the physiology and pathology of spasmodic affections:—

1st. The importance of great attention to a proper supply of nutriment and of air in all circumstances where either hereditary tendency or other circumstances are likely to develop convulsive diseases. Trismus nascentium is epidemic in the West Indies, from the absurd way in which infants are there treated. The same disease was banished from the Dublin Lying-in Hospital by proper attention to ventilation and cleanliness.

Laryngismus stridulus is never so satisfactorily treated as by change

of air. The mortality of tetanus in our naval stations in the West Indies has been very much reduced, mainly, according to Dr Dickson, by improved hygiene.—(*Med.-Chir. Trans.*, vol. vii. p. 765.)

2d. Let us divest our minds of the idea that it is necessary to treat the fit in any of these diseases. It is only a part of a train of morbid phenomena, and though the part most striking to the by-stander, ought not to make the same impression on the intelligent physician. No one now thinks of treating the fit, either in epilepsy or in chorea, and why should we think it necessary in tetanus or hydrophobia?

3d. Is it not worthy of consideration whether the obstinate constipation in tetanus and lead poisoning may not be a spasm of the muscular coat of the intestines analogous to that of the voluntary muscles, and, like it, not to be overcome by brute force (*i. e.*, purgatives)? Certain it is, that in lead Colic the finger, introduced into the rectum, is held by the sphincter as by a vice, and pressed tightly upon by the gut, and that this pressure returns at each paroxysm.[1] Certain it is also, that the retraction and hardness of the abdomen, associated by Merat[2] with that internal constriction, is found also to exist in tetanus, though I am not aware that any one has ever explored the internal state of the bowel in that disease. It may be necessary to remove the morbid secretions in the bowels, though every intelligent physician will have to balance the amount of irritation produced by their presence with that caused by the drastic purgatives necessary for their removal. The spasm which retains them is the effect of the disease. It is to treat symptoms, to attack it alone; and when we succeed in overcoming the disease, the bowels will spontaneously relieve themselves. Tralles[3] found opium succeed in a case of ileus where purgatives had failed. I have seen the same. Lentilius[4] has confirmed this; and in a severe form of colic, Bonn[5] became convinced by experience that it was the most powerful remedy.

4th. The most efficient and the most frequent agents in the production of these diseases are sedatives. Blood-letting is a most powerful sedative, and if carried to any extent in a healthy person produces convulsions. Is blood-letting, then, a suitable remedy in these affections? In chorea it was formerly practised, and is now abandoned from the injurious effects which it produced. In delirium tremens and hysteria, its use has also been given up. In epilepsy it is rarely used by intelligent physicians, except to meet the requirements of secondary affections; and if we still retain its employment in the more severe convulsive diseases, it is probably only because their rapid course and frequently fatal issue makes it very difficult for us to ascertain the effect of any treatment.

[1] Tanquerel—Traité des Maladies de Plomb., etc., p. 210.
[2] Traité de la Colique Metallique. [3] Opii. Usus et Abusus, sect. 2. p. 260.
[4] Eph. Mat. Cur, dec 3, ann. 1, p. 131. [5] De Officio Medici.

5th. In one class of convulsive diseases blood-letting seems, in the present state of our knowledge, to be indispensable. Where urea exists in the blood and produces convulsions, it must either be expelled or counteracted. We scarcely know how to accomplish the latter indication, and therefore are driven back on the former; but, even while seeking by blood-letting to get rid of the urea which is mixed with that fluid, we must never forget that it "both acts on the nervous system as a narcotic poison and impoverishes the blood, inducing degeneration of the tissues;" [1] and that, therefore, while we take blood to remove the poison, we must do our utmost to replace the nutriment which we are unavoidably compelled to abstract along with it.

6th. Our views on this subject would become more definite and precise, could we avoid imagining that spasm implies augmented strength. It is not easy for us, when seeing the violent agitation of the system which prevails, to divest our minds of the idea of great power being developed, but the same remarks apply to mania, in which disease, thanks to the exertions of Dr W. A. F. Browne,[2] general depletion, tartar emetic, brisk cathartics, and ice to the shaved scalp, are no longer in such vogue as they once were.

If such are the objections to the routine practice, what course ought to be pursued? It is easier to point out error than to demonstrate truth. But I venture to suggest,—

7th. That sedatives should be cautiously used. Chloroform and cold affusion have each proved fatal in delirium tremens.

8th. That every effort should be made to put into the system as much nourishment as it is capable of beneficially employing.

9th. But, if there is any faith to be placed in antipathic treatment, it is to stimulants we must most trust.

10th. It is worth observing, that most of those chemical agents which produce convulsions, acting, as has been already said, in large doses as sedatives, do in small doses act as stimulants.

11th. We find also, that where the minor spasms, as cramp, have been excited by irritation of the peripheral distribution of one class of nerves, as those of the mucous lining of the bowels, they are often relieved by irritation of the peripheral distribution of another class of nerves, as by friction on the skin.

12th. The cerebral functions, more especially volition and sensation, being much in abeyance, any stimuli, whether mental or physical, by which they can be excited, should be freely given. It was on this principle that Boerhaave prevented the recurrence of epileptic attacks, by directing a red-hot iron to be applied to any who might be seized.[3]

13th. The extreme sensitiveness to all irritations which exists

[1] Williams' Principles of Medicine, p. 150. [2] Monthly Journal, 1841, p. 75.
[3] Falconer on the Influence of the Passions, p. 100.

whenever the Spinal predominates over the Cerebral system, suggests the propriety of enforcing the most absolute quiet, and preventing the access of all bodily and mental stimuli. In tetanus and hydrophobia the creaking of a shoe, the slamming of a door, the sight or even the thought of water, or the gentlest zephyr playing on the surface of the body, excites a fit. Hence Armstrong tells us, that in tetanus those patients recover best who get little active treatment, but are nursed, as it were, through the fit.[1]

14th. And if sources of irritation ought to be prevented, those actually existing ought to be removed, but never by irritants which are more powerful than themselves. This surely gives sufficient latitude to the most devoted admirer of the gum lancet, the bolus, or the bag and pipe, while it suggests to him a little caution in these somewhat coarse though most popular remedies.

15th. And lastly, if I have not succeeded in pointing out any one remedy which stimulates the cerebral without exciting the spinal system, it is because few, if any, such are known. It is a subject of investigation well worth attention. And if no particular plan of treatment has been announced, which can be unhesitatingly recommended, I am perfectly satisfied if I have created any doubt as to the course which at present is too unhesitatingly followed. I think, at least, something has been done to show how much more constitutional these affections are than is usually supposed, and thus to open up new plans for prevention, if not for cure. And if in acute mania, in puerperal insanity, in delirium tremens, in chorea, in laryngismus, and may I add in epilepsy also, empiricism or the influence of authority has induced us to abandon antiphlogistic treatment and to adopt stimulant and soothing remedies, I do not despair of a sounder pathology soon prevailing in regard to this whole class of affections, and of the discovery yet being made, that they are much more amenable to treatment than has hitherto been supposed.

ARTICLE III.—*Illustrations of Thoracic Aneurism, with Remarks.* By W. T. GAIRDNER, M.D., Assistant-Physician and Superintendent of Morbid Anatomy in the Royal Infirmary of Edinburgh.

AT a meeting of the Medico-Chirurgical Society, in June 1851, I brought forward a case of aneurism of the aorta (reported in the "Monthly Journal of Medical Science" for August, pp. 137 and 184), in which, as in some other published cases, the symptoms were almost exclusively referred to the larynx; spasmodic dyspnœa with stridulous respiration forming a complication so serious as apparently to warrant the operation of tracheotomy. Although, owing to the objections of the patient, the operation was delayed till too late to be

[1] Lectures, edited by T. G. Thomson, p. 572.

of any practical service in this case, I took the opportunity of justifying the recommendation and the practice, by appealing both to the theoretical explanation of the laryngeal symptoms which attend upon aneurism, and to the experience of other cases, which show,—1st, that dyspnœa is the direct cause of death in a large proportion of aneurisms of the arch of the aorta, especially those arising from its back part; 2d, that the dyspnœa produced under these circumstances has generally a well-marked spasmodic character, and often distinctly the laryngeal type; 3d, that the aneurismal dyspnœa is, therefore, not necessarily connected with direct mechanical obstruction of the air-passages, but (as was long ago hinted by Willis) is caused in many instances exclusively by interference with the nervous trunks, and especially the pneumo-gastric and recurrent nerves; 4th, that the nature of the laryngeal affection is proved in some cases of aneurism (as first pointed out by Dr Todd) by the atrophy of the internal muscles of the larynx, on the side on which the recurrent is interfered with; 5th, that the operation of tracheotomy is known, in at least one remarkable instance, to have prolonged life (for thirteen days) under the peculiarly unfavourable circumstances of a *very large* aneurismal tumour, not only *compressing* the trachea, but having opened into it, and caused considerable hemorrhage. From these data I argued, that if tracheotomy, performed *in articulo mortis*, under the circumstances to which I have just alluded, could produce even the most temporary relief, there must undoubtedly be cases of aneurism in which life may be materially prolonged, and very much suffering avoided, by means of this small but important operation; and that the current rule, sanctioned by many of the best authorities in medicine, which regards the performance of tracheotomy in aneurism of the aorta as a practical error, must receive some considerable modification. The case which I laid before the Society appeared to me to be one in which the operation was strongly called for, all the more so that the existence of aneurism was uncertain and almost unsuspected (its physical signs being absent), while the laryngeal spasm was apparent, and the absence of mechanical obstruction was rendered certain by stethoscopic examination.

Since this communication was read to the Society, several other cases have come under my observation bearing upon the subject of the functional diagnosis of thoracic aneurism, and especially its relation to disorders of the respiratory function. The peculiar importance of this inquiry must not only be evident to every practical physician acquainted with the history of the subject, but is more peculiarly illustrated by the case which I have already laid before the Society. In that case it was remarked, that there was "no dull percussion, abnormal pulsation, or tremor at upper sternum; normal and symmetrical percussion over the lungs in every part; no abnormal respiratory sound over the trachea in front or at the root of the lung behind; abundant and symmetrical respiratory murmur

in both lungs, and over every part of them," etc. At a period, therefore, in the growth of the aneurism at which no physical sign could be discovered, its diagnosis might have been made, or at least its existence might have been strongly suspected, from the functional disturbance of which it was the cause, conjoined with other circumstances in the history and progress of the affection.

The following case, though not recorded in detail during the life of the patient, will tend further to show that I have not exaggerated the obscurities of physical diagnosis, as applied to small aneurisms within the thorax, and producing serious interference with the respiration :—

CASE I.—Shortly before my former communication was read to the Society, and while the impressions derived from the case to which it referred were in full force, a robust labourer, æt. about thirty-five, came under my observation in the Royal Infirmary, while acting for Dr Andrew. He had very much the same history and symptoms as the former, the symptoms of his case being frequent attacks of dyspnœa, evidently spasmodic, and accompanied by laryngeal stridor, but without fever, tenderness over the larynx, or any ascertainable local laryngeal affection, except slight and doubtful redness about the arches of the palate. The expectoration had been at various periods considerable, muco-purulent, and intermittingly tinged, streaked, or stained with blood; there was no trace or sign of any considerable affection of the lungs or bronchi, and the heart and great vessels appeared in all respects healthy. The examination was in this case made with the express suspicion of aneurism on the mind, and every known physical evidence of thoracic aneurism was sought for with the greatest care. In particular, the vessels at the root of the neck were explored with the fingers, by the stethoscope, and by percussion, without giving a trace of anything abnormal; and the trachea and bronchi, examined both at the front and over the root of the lung behind, showed the air-passages to be free from material obstruction, except at the larynx. Being thus foiled in discovering proof of an aneurism, and yet by no means satisfied of its absence, while, at the same time, the laryngeal symptoms appeared frequently to approach the point of threatening life, I told the patient that the last resource for him would be to open the windpipe, but that all other means should be tried first. This conclusion was steadily kept before him for some days; and though strongly protesting against the extreme measure, he submitted very attentively and quietly to sedative and antispasmodic treatment, which, in a week or two, seemed to have somewhat diminished the violence of the spasms, and reduced the frequency of their occurrence. On one occasion I requested Dr Robertson to see him with me, with a view to the complete re-investigation of the question of aneurism; and the physical examination of the chest and root of the neck was again made with the utmost care by Dr R., with a negative result. The sputa were still sanguinolent, though often slightly so, and very slight tenderness could be apparently elicited over the larynx. Tracheotomy was never insisted on with urgency, owing partly to the strong suspicion which still remained, that the laryngeal affection was dependent on an aneurism within the thorax, and partly to the slight abatement in the symptoms, which removed the principal apparent reason for urging the operation on an unwilling patient. On handing over the patient to Dr Andrew on his return, the same course was pursued, and the chest was again repeatedly examined without any further discoveries to which importance could be attached as indicating aneurism. Shortly afterwards, I requested a physician, who was at that time on a short visit to Edinburgh, and who has a large special practice in diseases of the throat and larynx, to examine this patient, along with Dr Bennett and myself, telling him, at the same time, the doubt which appeared to hang over the case. This gentleman pronounced the epiglottis and the neighbouring parts to be ulcerated and

thickened; and his opinion was given so confidently, as the result of examination by touch, and even by sight, that the lingering doubt of aneurism was for the time dissipated; and I believe Dr Andrew was even induced to make a few trials of the solution of nitrate of silver, applied with a sponge to the glottis. No very material change took place in the symptoms; but the difficulty of breathing underwent no diminution, the sputa remained purulent and sanguinolent, and a few weeks afterwards the patient expired suddenly from profuse hæmoptysis.

Dissection discovered an aneurism, the size of a large date, communicating with nearly the entire length of the trunk of the innominata, and projecting from its orifice backwards towards the lower third of the trachea, into which it opened near the mesial line. The aneurism pressed on the right recurrent nerve, which was flattened and involved in the sac. The internal muscles of the larynx, on the right side, were slightly atrophied. The mucous membrane of the larynx and epiglottis was perfectly pale and normal, unless a very slight and scarcely appreciable development of its mucous follicles be considered a disease. There was certainly no trace of ulceration, even the most superficial. The right carotid and subclavian were of normal caliber; the arch of the aorta was not dilated; the heart was nine and a half ounces in weight, and normal; the aneurismal sac was nearly full of coagulum; and this circumstance, together with its deep position, may probably account for the expansion being so slight as not to be appreciable at the root of the neck during life.

This case will, I trust, be considered to demonstrate clearly the extreme importance of the functional symptoms of aneurism within the thorax, even where the tumour, from its size and situation, may elude the most careful physical diagnosis. It is, moreover, a case where, as it appears to me, the operation of tracheotomy might with great propriety have been performed, had the laryngeal symptoms attained the same intensity as in the former instance, or had they even remained as threatening as they were for a short time after the patient's admission. From the anatomical relation of the aneurism in this as well as in the former instance, there is little doubt that the recurrent laryngeal was the only important nervous trunk implicated in either case; and I cannot doubt, therefore, that the frightful and agonising dyspnœa which attended both cases, and especially the former, would have been materially relieved, and the tenure of life rendered for a time less precarious, had the operation been performed.

Since the preceding case was read to the Medico-Chirurgical Society, I have received from Dr Gibson, of Dundee, the following very interesting account of a case of aortic aneurism, which occurred under his care, and of which he has kindly permitted me to publish the details. I have great pleasure in directing attention to it, as an instructive instance of the successful application of what may be called a physiological diagnosis in thoracic aneurism. The performance of tracheotomy, with a certain measure of temporary success, under circumstances apparently the most desperate, was at once a bold and a well-considered application of the principles which I ventured to deduce from my first case in the "Monthly Journal of Medical Science" for August 1851:—

Case II.—W. R., an athletic seaman, aged thirty-four, on the 31st of October 1851, fell to the ground from a height of several feet, suffering what appeared,

from a careful examination which I made shortly afterwards, to be simply a severe muscular contusion of the right shoulder and side of the chest.

From the effects of this accident he had to all appearance, with the exception of some remaining stiffness and weakness of the limb, quite recovered at the end of three weeks, when he returned to duty; and I accordingly lost sight of him till he called on me again, on the 22d of January 1852, to consult me respecting what he thought a bad cold, which had been hanging about him, he said, for ten days or a fortnight; and I confess that, at the time, I thought he was right. He had the muffled husky voice, the sharp ringing cough, the dyspnœa, and stridulous inspiration, of a sufferer from acute laryngitis. He confessed to an uneasy sensation at the top of the windpipe, which pressure over the larynx increased, though it scarcely amounted to pain; and as a careful examination, by percussion and the stethoscope, failed to detect anything wrong with either the heart or the lungs, and he had no pain of chest, I at once concluded laryngitis to be the disease under which he was labouring.

He told me, moreover, that he had, again and again, while at work, been suddenly seized with a feeling of instant suffocation, which he referred to his throat, and under which he had more than once come to the ground in a momentary state of unconsciousness. All this seemed still further to point to the glottis as the chief seat of the mischief; and as his pulse at the same time was hard, frequent, and jerking, my treatment was correspondingly active.

Into any details on this point, however, it is needless to enter. Suffice it to say, that it was only after the successive adoption and failure of every remedial measure suggested by the belief that the case was one simply of laryngeal disease (a view of its nature in which my friends Drs Munro and Matthew Nimmo entirely concurred), that I began to entertain serious doubts of the accuracy of my diagnosis; and that the suspicion that this was perhaps, after all, an instance of aortic aneurism, simulating, through its pressure on the recurrent nerves, disease of the larynx, now first struck my mind on my meeting, in the course of my reading during this my dilemma, with a paper by Dr W. T. Gairdner, on an analogous case, and the details of another by Dr W. H. Gooch; the former in the "Monthly Journal of Medical Science" for August 1851, the latter in the "Provincial Medical and Surgical Journal" for February 1852. These together threw quite a new light on the case. Nor did the universally acknowledged obscurity in the diagnosis of intra-thoracic aneurism during life in some instances appear to me to render less probable the accuracy of this view of the case. For, though auscultation still failed, at the end of two months of close observation, to detect in the chest of my patient the least signs of aortic disease, it was nevertheless easy, with the evidence of similarly obscure cases before me, both to surmise its existence, though too deep for detection, and to find a likely cause in the accident he had so recently met with.

I ought to add that Dr Alison, whom my patient now consulted in Edinburgh, took, on the whole, the same view of the case.

The treatment was now for some weeks longer directed accordingly, in the hope that, under the effects of complete bodily and mental repose, extremely low and spare diet, the occasional abstraction of blood by venesection and cupping, and the administration of such sedatives as digitalis and opium, the disease might be at least kept in check, and time afforded for Nature's attempts at a cure. But his condition, on the contrary, became progressively worse; his dyspnœa, and stridulous breathing, and cough, more distressing and constant, and the paroxysms of laryngeal suffocation in particular, so frequent and alarming upon the slightest exertion, that, with the concurrence of the two professional friends whom I formerly mentioned, I determined on opening the windpipe as the only means left of prolonging his life; though indications which the stethoscope had latterly given of pressure upon, and consequent narrowing of, the left bronchus especially, made this hope somewhat doubtful. I ought also to mention, that a very slight *whizz* was now to be detected, we thought, with each systole of the heart, at the upper part of the sternum.

Such was the patient's state when, at midnight of the 6th of last May, and with the kind assistance of these gentlemen and Dr James Drummond of Edinburgh, I performed tracheotomy; the suffocative agony, and the whole aspect of the patient indicating that there was no time to be lost.

It saved his life at the moment, and he lived for twelve days thereafter; for although, probably owing to pressure at the root of the lungs, respiration continued still very laboured and panting, yet he managed to breathe through the opening, and while he also got rid of large quantities of ropy mixed purulent matter by coughing, he remained free from those paroxysms of laryngeal suffocation under which, previous to the operation, he had so often nearly expired. He at last sank from sheer exhaustion apparently, scarcely having closed his eyes in sleep for a fortnight or more.

Permission was given us to open the body; but I deeply regret to say that, having carefully removed the whole parts involved with a view to their being examined with more leisure at home than we then could command, and unavoidable delay having occurred in so doing, they unfortunately went so fast into a state of complete putrefaction in the keeping of another professional friend, who was present, and who undertook to preserve them, as to be found quite unfit for the purpose. Nevertheless, we saw enough at the time of our first somewhat hurried inspection to satisfy us, that a tumour of about half the size of a fist, which we found closely attached to the under part of the aortic arch, was aneurismal; the left recurrent nerve being, at the same time, seen passing into the condensed areolar tissue, with which it was closely invested.

I think it will be admitted, that the result of the operation in this case, while it illustrates very well the circumstances which may often be expected to interfere with the relief which tracheotomy is calculated to afford, justifies the bold and decided practice of Dr Gibson, and holds out a strong inducement to the performance of this small operation in cases of aneurism, accompanied by well-marked laryngeal symptoms. Had the aneurism not been so situated as seriously to interfere with the caliber of the left bronchus, it is probable that the relief to suffering by the suspension of the laryngeal symptoms, and even the prolongation of life, would have been more decided; and, with a fair chance of such a result on one side, and certain agony and rapid death on the other, the duty of the medical practitioner appears to me in no way doubtful.

The following case occurred under my own care, and as it involves numerous important practical considerations, it is given in detail almost in the words in which it was originally recorded:—

Case III.—W. G., porter, robust, full-blooded, aged about sixty. Admitted into the Royal Infirmary, August 9th, 1851, at three P.M., suffering under excessive dyspnœa.

Has been ill since Monday last (five days) with cough, attended with little or no difficulty of breathing until three days ago, when he was compelled to give up working. He ascribes his illness to carrying a heavy load, for a great distance, last week. He never had any pain in the chest (he was repeatedly questioned on this point with the above result; pain in the back was not specially inquired after). His cough was accompanied by expectoration, mixed with blood, ever since it came on. He never brought up blood in any quantity; and has always up to this illness enjoyed uninterrupted good health. Habits not temperate.

Orthopnœa extreme; marked lividity of face and lips; surface of body covered with large drops of perspiration. Pulse tolerably full, regular, hurried. Has an expression of extreme anxiety. Sputa consist of mucus, rather viscid, deeply

and uniformly stained with blood. Chest expands pretty freely on both sides. Vocal thrill and resonance feebler on left side. Percussion on left side, both in front and behind, almost absolutely dull. On right side, respiration is sonorous and wheezing; on left side, comparatively feeble. Inspiration, accompanied by numerous rather coarse mucous râles; and throughout the chest sibilant and sonorous râles are audible. Heart sounds appear natural.

Bled to ℥xx., with the effect of producing an approach to faintness. Blood slightly buffed. During the venesection, perspiration, in excessively large drops, poured from the forehead and the whole body.

Vespere.—After an hour, breathing became easy, lividity of lips diminished; face was slightly flushed; pulse about 90, full. Had still cough, sometimes in paroxysms. Ordered sulphuric ether, in ℥ss. doses, with a little whisky. At half past six P.M., Ipecac. gr. iij., and Squill gr. ij., in powder, every four hours.

August 10th.—*Vespere.*—Slept well last night, and continued well during this day. Took his meals heartily, and made no complaint. Sputa as before.

Nine P.M.—A new attack of orthopnœa; condition as on admission.

Again bled to ℥xx., without relief, and died between one and two A.M. on the 11th.

Supplementary Note on the 11th.—(From memory.)—A very plethoric man, robust, neither emaciated nor corpulent.

Chest large, well developed. Dulness of percussion (as above-mentioned) nearly absolute over left side; traces of pulmonary resonance only over upper lobe; respiratory murmur much enfeebled, but still audible at back; vocal resonance and thrill present over greater part, but enfeebled in lower lateral region. On right side percussion good; respiration very loud and wheezing, obscuring both the heart's sounds and those of the left lung. On this account the examination of the heart was not satisfactory; but nothing abnormal could be detected over it, or over the great vessels. No pulsation of veins, nor abnormal pulsation of arteries in neck.

Expansion of the two sides appeared not visibly different, but was not accurately estimated. The right side exceeded the left slightly (quarter of an inch) in circumference below the nipple. Heart's sounds not displaced. Position of apex indistinct.

Skin cold on admission. Afterwards no trace of fever. Countenance livid and congested on admission; lividity had almost disappeared, though some congestion remained, at second visit.

On the 10th, was observed eating freely and heartily, without apparent dysphagia.

Examination of the Body, August 12th.—On opening the thorax, the right lung appeared voluminous and distended; the left retracted, and comparatively small. In the left pleura there were about twenty-four ounces of serum, mostly clear, with a few flakes of lymph. The right pleura contained only a few ounces of fluid. No adhesions in either pleura, except at the root of the left lung at its upper part. The heart was not displaced.

The whole of the organs in the neck and thorax were removed together, when it became evident that there was a tumour pressing upon the bodies of three or four dorsal vertebræ, and connected with the descending aorta. The pharynx and œsophagus being slit open, it appeared that the œsophagus, about its middle, was in the close neighbourhood of the tumour. The muscular coat of the œsophagus was at this point slightly atrophied, and there was a little sub-mucous ecchymosis, but the mucous membrane was normal. The larynx and trachea were normal, except that the latter contained a quantity of blood-streaked mucus.

The heart weighed fourteen ounces; all the valves normal. The aorta at its commencement was not enlarged, but its inner membrane was rough and uneven, with much opaque deposit, not calcareous. The arch of the aorta was slightly dilated near the origin of the innominata, and this vessel was also slightly enlarged. The carotid and subclavian arteries on the right side were normal in

size, the former studded internally with yellow opacities. The left carotid and subclavian also contained slight atheromatous deposit, but were otherwise normal. The internal coat of the whole thoracic aorta abounded in atheromatous deposit, and was extremely uneven, but without any induration or calcareous matter. In the descending aorta, there were two considerable abnormal openings in the wall of the vessel. One was situated about an inch beyond the origin of the left subclavian artery; it was oval, about an inch by three quarters of an inch in diameter; its smooth and rather sharp edge formed by the internal coat of the vessel. This opening led into an aneurismal sac, about the size of a large orange, which, arising from the posterior wall of the artery, projected upwards and backwards, and was adherent to the periosteum of the third and fourth dorsal vertebræ, to their left transverse processes, to the articulations of the ribs, and, through the medium of the parietal pleura, to the inner and back part of the upper lobe of the left lung. The bodies of the above-mentioned vertebræ were considerably deformed by atrophy of their anterior part. This sac was about half-full of stratified and decolorised coagula.

Another aneurismal sac arose from the second opening above mentioned in the wall of the descending aorta. This opening was round, somewhat less than a sovereign, and quite smooth-edged, like the former, from which it was distant about three-quarters of an inch downward and inward. The sac connected with this opening arose from the right wall of the artery, and passed inwards towards the left bronchus, to which it was firmly adherent, and into which it opened by a very wide communication. This aneurism was likewise half-full of laminated coagula, and was about the size of a large plum. The two sacs above-described were so close together as to form what at first sight appeared as a single tumour. They did not, however, communicate at any point.

The left bronchus had its posterior wall deficient for about an inch; and its caliber was almost completely occupied at this point by a firm, gray coagulum of blood, which projected out of the aneurismal sac.

The left lung was almost completely condensed; crepitation remained only at a few points in the apex, and still fewer in the lower lobe. Its tissue had a nodulated and dense feeling externally, and presented a remarkably variegated appearance, some portions being violet-coloured, and others of an almost sandstone-gray tint. The interlobular spaces were everywhere well marked, depressed, and often dividing sharply the red from the gray parts. On section, the same varieties of colour prevailed throughout the lung; the tissue was dense, opaque, resistant, very obscurely granulated to the eye, and here and there dotted with distinct yellowish points, which presented somewhat the appearance of tubercles, but on examination proved to be the smaller bronchial ramifications, completely plugged with opaque, viscid, muco-purulent matter. The greater bronchi contained a similar stringy mucus, more or less mingled with pus and blood; their mucous membrane was stained, but little congested.

The right bronchus and its principal branches contained a little blood-coloured mucus, but were otherwise natural. The corresponding lung was distended, and almost emphysematous in appearance; on section, its tissue stained here and there with blood, but highly crepitant throughout.

The only other morbid appearances of importance were, incipient cirrhosis of the liver, atheroma, with calcareous degeneration of the abdominal aorta, and slight opacity of the cerebral arteries in some points.

The case just related is an excellent example of aneurism giving rise to spasmodic dyspnœa of an *asthmatic* character, without the slightest tendency to laryngeal spasm. In this last particular it is strongly contrasted with Cases I. and II., as well as with the one recorded in the former paper. The situation of the present aneurism, and the freedom from compression of the laryngeal nerves on

both sides, sufficiently explain the absence of phenomena connected with the glottis. But the positive facts of the case, and especially the symptoms of disorder of the respiration, demand to be considered in relation to the position of the aneurism and its effect on the left lung.

The tumour pressed on the greater part of the left bronchus, obstructing it almost completely, and opening into it by a considerable aperture, which, however, was greatly diminished as an outlet for blood by coagula, and by the almost complete flattening of the bronchus at the point of opening. Hence the hemorrhage, though sufficient to attract attention, was never more considerable in amount than might have been accounted for by a more ordinary cause, such as hemorrhagic condensation of the lung, or even ordinary acute pneumonia. The blood, confined in its passage outwards, appears to have pressed backwards towards the lung with considerable force, and was found in all the bronchial ramifications of the left lung, mingled with a large quantity of glairy, viscid mucus, which completely obstructed the greater number of the air-passages. The condition of the pulmonary texture found in connection with this form of bronchial obstruction, is well worthy of remark. It was a very striking, peculiar, and rare form of condensation, equally distinct from hepatisation and hemorrhagic condensation in their more ordinary forms, as well as from that simple collapse of the air-cells which I have elsewhere described[1] as supervening on bronchial obstruction in other circumstances. Yet it had characters in common with all these lesions. That the left lung was diminished in volume was evident, not only from the effusion of a certain amount of serum into its pleural cavity, but from the voluminous and almost emphysematous condition of the opposite lung, which was evidently over-distended, and had crossed the mesial plane of the chest from the collapse of its fellow. Yet this incomplete collapse of the left lung was not accompanied by an empty state of the air-vesicles, and the pulmonary tissue, although perfectly non-crepitant, was by no means flaccid, dry, and smooth on section, as usual in bronchitic collapse. On the contrary, many of the air-cells were filled with blood, and others with a soft grayish exudation, being a mixture of fibrin, in various proportions, with blood and pus, which, however, did not give to the section the distinctly granular appearance usual in pneumonic condensation, although in density and resistance the lung resembled the condition of that organ in hepatisation or hemorrhagic condensation from disease of the heart. In fact, I believe that this curious and unusual pathological condition of the lung, of which I have preserved a very accurate drawing, can only be explained as follows:—The obstruction of the main bronchus produced in the first instance, a considerable degree of pulmonary collapse; and

[1] Papers on the Pathology of Bronchitis,—"Monthly Journal of Medical Science," 1850-51.

after this condition had lasted some time, the bronchial hemorrhage forced itself into the collapsed air-vesicles, much in the same manner as an artificial injection through the bronchi in a partially atrophied lung; the lobules thus assuming a distended appearance, the intervening septa remaining depressed. The remarkable distinctness with which the surface of the lung showed the various lobules in their different degrees of distension, and the very firm plugs in most of the bronchi, seem to prove the correctness of this solution of the anatomical phenomena.

I have next to consider the *dyspnœa*. That a certain considerable amount of dyspnœa is fully accounted for by the condition of the left lung, it is impossible to deny. But the peculiarly exquisite character of the symptom in this case, its very perfect intermission within an hour or two after the first bleeding, as well as during the whole of the second day of observation, and its equally sudden re-accession on the next evening, are phenomena much more allied in character to spasmodic asthma than to any other disease of the respiration, and certainly not due to the condensation of the lung, or to the permanent obstruction of the air-passages. Bearing in mind the analogy of the cases of compression and irritation of the laryngeal nerves, and the recent experiments of Volkmann[1] and others, which show that the nervous system has a powerful influence over the contraction of the bronchial muscles, it is impossible to avoid the conclusion, that the spasmodic asthma in this case was due to the pressure of the aneurism on some of the branches of the left pulmonary plexus of nerves, which must have lain in great part between the tumour and the bronchus. The main trunk of the pneumogastric was not involved by the tumour, and accordingly there was no dysphagia; nor were any of the other important nervous structures in the thorax at all implicated. The facts of the case, therefore, both positive and negative, seem to be perfectly in harmony with the morbid appearances as thus explained.

In relation to *diagnosis*, this case is not a little instructive. The idea that the patient was the subject of aneurism was strongly presented to my mind at the first examination, and was mentioned at the dissection as the most probable solution of the case. Notwithstanding the absence of the elements of a physical diagnosis, this opinion seemed to be justified by the following circumstances:—The almost complete dulness on percussion over the right lung, and the feebleness of respiration, vocal fremitus, etc., on that side of the chest, could only be produced either by considerable pleural accumulation, or by pulmonary consolidation, accompanied by nearly complete obstruction of the bronchi.[2] The former supposition was ex-

[1] Handworterbuch der Physiologie, vol, ii., p. 586.

[2] I have been for several years in the habit of observing, that pneumonic consolidation so accompanied will give rise to all the signs of a large pleuritic effusion, except distension of the side. Of this I have now seen many instances besides the present.

cluded by the absence of dilatation of the left side of the thorax; the latter was entirely in harmony with the supposition of an aneurism of the descending aorta. The presence of spasmodic dyspnœa, and the absence of laryngeal spasm, seemed equally to point to this as the probable situation of the aneurism, if present. The presence of blood in the sputa lent strength to the idea of aneurism, as it has been shown in numerous other cases that this disease does not necessarily lead to profuse hemorrhage by bursting in a mucous surface. Although the amount of blood observed in the expectoration in this case was not greater than is often observed in heart disease with pulmonary hemorrhage, yet as evidences of valvular disease were wanting, and as the consolidation of the lung in pulmonary hemorrhage is usually double, I was induced to regard this idea as less probable than that of aneurism; and was thus led, among various circumstances suggestive of doubt, to the correct diagnosis. Admitting the idea of aneurism, there could be little doubt as to the situation of the tumour. The absence of laryngeal symptoms and of dysphagia, the signs of obstruction of the left bronchus, the absence of dull percussion or abnormal sounds on the front of the chest, the normal condition of the vessels in the neck, combined to give a correct idea on this point. If a more accurate history had been procurable, the presence of pain between the shoulders would have probably formed an additional element in the diagnosis.

The present case illustrates very clearly a respiratory symptom dependant on aneurism, probably due, like the laryngeal dyspnœa before mentioned, to nervous irritation, but of a kind beyond the reach of surgical relief. It may easily be believed that these two symptoms may be combined in one case, especially if the aneurism be of large size; and in such instances little or no relief would be procured from tracheotomy. I do not, however, regard the possible existence of this, or any other unascertained complication, as a sufficient reason for withholding the operation; but only as suggesting the propriety of a very guarded prognosis, great care being taken in all cases to ascertain as nearly as possible the relations of the tumour when tracheotomy is proposed. The cases which hold out most encouragement to the operation are evidently those of small tumours of the innominate artery, or of the lower and back part of the arch; and every part of the air-passages ought to be accurately explored, with a view to the discovery of evidences of obstruction.

The catalogue of symptoms due to the pressure of intra-thoracic aneurisms on the nerves in their neighbourhood, is not exhausted by those to which I have hitherto alluded. Various kinds and degrees of uneasiness or positive pain arise from this cause; although the most severe and constant painful sensations produced by aneurism are no doubt due to the erosion of the bones and cartilages on which, as in the above case, they so often exercise deleterious pressure. Dysphagia, although frequently due to direct pressure on the œso-

phagus, is sometimes so distinctly spasmodic in character as to suggest the idea of its having its source in pressure on the pneumogastric nerve; and the respiration sometimes suffers (as I believe I have seen in one instance) from lesion of the phrenic nerve in large aneurisms. But there is one symptom of by no means unfrequent occurrence, and which I believe will be found to be almost confined to aneurisms of the ascending aorta, especially in the immediate neighbourhood of the heart. This is a greater or less degree of *angina pectoris*, as described by Heberden, a form of nervous irritation, which may well be referred to compression of the great plexuses of nerves ramifying on either side of the ascending aorta, and communicating freely with the cardiac ganglia and plexuses of the ventricles. The following case affords a good illustration of this symptom in an almost uncomplicated form:—

CASE IV.—A man, aged about 45, affected with manifest signs of aneurism of the ascending aorta pressing on the costal cartilages, came under my notice repeatedly at intervals during last summer. He had been ill for many months, when first seen; had been formerly under medical observation; had borne his fatal complaint to Ireland and back, and by watching the gradual course of his disease had acquired spontaneously so complete and intelligent a conviction of its nature and certain termination, that his whole manner and habit of life conveyed the impression of a man prepared for instant death. As he was very patient and calm, he often related to me, at considerable length and with great clearness, the narrative of his sufferings. These were very variable in amount; he had almost constantly a certain degree of gnawing pain, corresponding to the site at which the aneurism had produced a pneumonia at the front of the chest. He had no dysphagia, and little if any feeling of dyspnœa; the lips were, however, somewhat livid, and the venous return by the superior cava was manifestly impeded. The most severe symptoms arose from a feeling, which seized him from time to time, and the minor degrees of which were present, to a greater or less extent, every day. It was a sensation of pain rarely amounting to agony, but attended by extreme distress, and an indefinable species of uneasiness, which he could only describe as driving him to desperation, and as being quite unrelieved by change of posture, or by any kind of repose or exertion. This sensation was not distinctly localised within the chest; he often compared it to the approach of death; it usually passed off, at least in its intense form, after a few hours, but a certain degree of it was almost constantly present with him. The pulse was little affected by this condition; the respiration always free. Percussion revealed a large tumour, occupying the position of the ascending aorta, and projecting to the right side of the sternum; a double murmur existed over the tumour, and the heart presented distinct signs of hypertrophy. This patient died in the middle of August, during my temporary absence from Edinburgh. Many remedies had been used, but none were of any avail as palliatives, except the exhibition of large doses of opium.

The dissection was performed by my friend Dr Sanders. A large aneurismal sac was found adhering to the parietes on right side at the fourth and fifth costal cartilages. The pericardium contained some serum, deeply stained with blood; and it presented reticulated lymph on both its surfaces. The aortic valves were incompetent; the heart much hypertrophied.

The aneurismal dilatation, on being laid open, showed a large pouch, consisting of the entire vessel, dilated from its very origin to the descending portion, where it resumed nearly its normal diameter, but presented much disease from atheromatous and calcareous deposits. From this dilated arch of the aorta there opened laterally on the right side, through a ring-shaped aperture of two inches

and a-half in diameter, another sac, lined only by thin fibrous walls, and adherent, as above mentioned, to the third and fourth right costal cartilages. The pneumogastric and recurrent nerves were carefully traced, but neither of them appeared to have been involved in the tumour or stretched over it.

In the present and in the former paper I have adduced illustrations of three distinct kinds of spasmodic or paroxysmal affection liable to be connected with aneurism of the thoracic aorta, and of its branches within the chest. *Firstly*, Spasmodic *laryngeal* dyspnœa or *laryngismus*, attended frequently, but not invariably, by alterations of voice or cough, and sometimes by habitually harsh stridulous respiration. *Secondly*, spasmodic *bronchial* dyspnœa or *asthma*, which may be accompanied by disease of the lung, or signs of pressure on a bronchus. *Thirdly*, Paroxysmal suffering referred to the heart, of the character now well known to practical physicians by Heberden's name of *angina pectoris*. The view, that these affections have their origin in the interference of aneurisms with the thoracic nerves, seems to have so much theoretical probability as to claim from future observers more attention than it has yet received in the instance of the last two; and the very decided manner in which observation has corroborated this view in the instance of the laryngeal spasms, suggests the propriety of increased accuracy of anatomical and physiological investigation in relation to this subject. As the instances which have occurred to myself have not presented a sufficient basis for induction, I endeavoured to obtain a broader ground for generalisation, by consulting the preparations of aneurism in numerous museums here and elsewhere, and the records of older observation on this subject; but the result was far from satisfactory, and it soon became evident that very little was to be added to our knowledge in this way. The museums to which I have had access abound in preparations of aneurism in which every relation of the slightest physiological interest has been laboriously cleared away by the knife of the dissector; and the guesses that might have been formed as to these relations are, in the vast majority of instances, rendered valueless by the negligence which has allowed the records of the case to perish. On the other hand, the literature of aortic aneurism has not appeared to me to yield many cases in which the symptoms I have described above admitted of precise analysis in connection with anatomical details of the position of the aneurism. It is sufficiently evident that the subject requires renewed observations, conducted with a view to the elucidation of these symptoms; and I trust this paper may prove the means of attracting additional attention to the relations of aneurism with the nervous system, especially in the thorax, where results of considerable importance to diagnosis and treatment have been shown to depend upon these relations.

Not a few cases are now on record, in which the sufferings caused by aneurism have been of so variable and capricious a character, as to suggest the idea of a purely nervous affection; and the complete

remissions which occurred in such cases, naturally surprised the older anatomists, who could not conceive "but that the effects of a permanent cause, such as an aneurism is, must be permanent."[1] These paroxysmal attacks are in some cases described as resembling suffocation or orthopnœa, the breathing being sometimes accompanied with stertor (stridor?), or with a sensation of a cord binding the trachea; in other instances, they have evidently none of the characters of angina, being accompanied by palpitation of the heart, swooning, etc. Frequently these symptoms are mixed, and very often details are wanting to distinguish between them. In the 17th and 18th Letters of the second book of Morgagni's work, the reader will find many interesting cases and most able discussions, which reflect the entire information of his age upon the subject.[2] In my former paper on this subject, I have alluded to the later observations of compression of the recurrent nerves by aneurism: on the other departments of the subject I have failed to discover any more precise data. Anatomical knowledge would lead us theoretically to look to compression of the cardiac nerves by aneurisms of the ascending aorta immediately above the valves, as the probable cause of angina; and, on the other hand, to refer the asthmatic paroxysm to aneurisms having the relations of the one in Case III. So far as my reading extends, I have found those views corroborated by the few cases from which any approach to precise deductions can be formed. It is certain, at least, that aneurisms of the ascending aorta are found not to produce dyspnœa in the same large proportion of cases as those of the descending and transverse part of the arch, except where this symptom occurs as a sequela of cardiac hypertrophy and valvular disease; and these are, again, more frequent in connection with aneurisms of the ascending aorta than with those of any other part of the vessel. I desire, however, to avoid laying too much stress on these imperfect observations, and to leave the subject to more mature and careful investigation.

[1] Morgagni, de Sedibus et Causis Morborum, Lib. 2, Epist. xvii. 27.

[2] See especially Epist. xvii. Sect. 14 (angina and spasmodic dyspnœa); Sect. 25 (spasmodic dyspnœa); Sect. 26 (do., interesting remarks on treatment by warm water fomentations of arms; in this case there was probably angina); Epist. xviii. Sect. 17 (spasmodic dyspnœa, probably with angina, in a physician of Modena). He was seen by Ramazzini and Malpighi, and, with the exception of the latter, no one who saw him suspected an organic disease. A case of great interest; the dissection, however, very imperfectly recorded. With these examples contrast the case in Sect. 25, in which dyspnœa, non-paroxysmal, accompanied an aneurism pressing directly on the lung.

ARTICLE IV.—*Homœopathic Cure of the Ocular Disease of Marshal the Count Radetsky.* By FREDERICK JAEGER, M.D., Professor of Ophthalmology in the Josephine Academy, Vienna.[1]

IN the course of the month of January 1841, Dr Hartung, physician to the army, and physician in ordinary to his excellency Count Radetsky, addressed a letter to me from Milan, in which he expressed a desire that I should give advice regarding a serious disease of the eye, with which the illustrious Marshal was affected. To this letter was added the following account of the case:—

"The Marshal's age is seventy, and he reckons fifty-six years of military service. His corporeal frame is vigorous and robust; his constitution good; his temperament lively and irritable; he has retained all the force of mind, all the activity, and, one might almost say, all the bodily strength of youth. Incessantly active, and always exposed to detrimental influences of every sort, as well as to the fatigues of the camp and of the military life, he has not escaped suffering various diseases, for the most part catarrhal and rheumatic, presenting themselves generally under the form of diarrhœa, or of pulmonary catarrh, accompanied by fever, to which affections the peculiarly vulnerable organisation of his tegumentary system seemed to dispose him. A few days of rest and strict regimen almost always sufficed promptly to dissipate these complaints, without leaving any bad consequences; but at an advanced age these affections, occurring pretty frequently, were at length followed by continual irritation of the Schneiderian membrane and of the windpipe, attended by very troublesome cough, and also by chronic weeping of the right eye, with inflammatory swelling and partial eversion of the lower eyelid. To these symptoms were added, from time to time, deranged digestion, determination of blood to the brain, with violent vertigo and headach.

"The Marshal was in this more or less annoying state of ill-health when, on the 9th of October 1840, during the manœuvres of the camp at Pardenone, he exposed himself for six hours consecutively, and on horseback, to the overpowering heat of the valleys and the currents of air on the heights. He was suddenly seized, in consequence, with violent fever, accompanied by severe pain in the forehead and temples; there arose, at the same time, an inflammation of the right eye and of the soft parts around it; and the globe of the eye was pushed considerably from the orbit. The pain was so severe that the patient could scarcely endure it, and was obliged to

[1] The present article is a communication from Professor Jäger to the Royal Academy of Medicine of Belgium, which was read to that learned body by Dr Cunier of Brussels, and published in the "Bulletin de l'Académie," Tome x. No. 9. For its present appearance in English, we are indebted to Dr Mackenzie of Glasgow.—*Ed. Mon. Journ. of Med. Sc.*

go to bed. During the night the intensity of the affection diminished, so that his excellency was able next day to attend a military parade, and some days after to return to Milan.

"The inflammation of the eye, as well as the fever and pain, soon disappeared; but, in addition to the old infirmities, there remained a greater degree of redness of the eyelids and swelling of the conjunctiva than formerly, and a tumour was now perceptible to sight and touch in the external angle of the eye, which caused the globe to protrude from the orbit.

"This aggravation, however, did not prevent his excellency from retaining the command-in-chief, nor from occupying himself, as before, with the affairs of the service.

"The treatment was, from the commencement of this attack, as it had been previously, strictly homœopathic, the remedies having been administered in the following order:—

"1. Aconit. 2. Baryt. carbon. 3. Zincum metall. 4. Anacardium orient. 5. Calc. carbon. 6. Euphrasia. 7. Mercurius Hahnemannii. 8. Merc. sublim. corros. 9. Antimon. crud. 10. Digitalis.

"All were administered in the greatest degree of dilution, according to the homœopathic precepts, and with the utmost precaution, on account of the great irritability of the patient.

"But as the disease of the eye, in spite of all these remedies, did not diminish, but, on the contrary, made progress, and as, besides the swelling at the inner angle of the eye, there had arisen another similar one at the outer canthus,[1] in shape like a bean, whilst the globe advanced more and more from out the orbit; and as pain and decided congestion were from time to time renewed, with fits of vertigo, the disease at length began to appear serious, and the ordinary physician, Dr Hartung, saw clearly that a fungus had formed in the orbit, and threatened to increase. In order to lessen the inflammation, he therefore ordered poultices, which, it is said, had a good effect; but, lest too prolonged an application of this means might injure the eye, they were abandoned, and attention directed to other indications—namely, to check as much as possible the growth of the fungus, to preserve the vigour of the system, and to restore serenity to the patient's mind.

"At the same time, Professor Flarer was called in from Pavia. He diagnosticated a scirrhous tumour in the orbit, recommended corrosive sublimate, and foretold an unfavourable termination of the case."

The narrative of which the above is a faithful summary, bore the signature of his excellency's ordinary physician, Dr Hartung.

[1] From some carelessness, probably typographic, it is impossible to know where the first swelling took place, whether at the outer or at the inner canthus. Formerly it was described as being at the outer, now it is said to be at the inner.—*Tr.*

I received at the same time a letter from Professor Flarer, who also had been desired to ask my opinion on the case. He gave a minute description of it, and considered it a *scirrhus of the peri-orbita.*

Before I could answer these two applications, I received from Count Hardegg, then minister of war, an order to pass without delay to Milan, in order to express to the Marshal the deep interest which his Majesty the Emperor took in his case. Although his excellency was not wanting for medical advisers, I was charged by his Majesty to confer with these, and to transmit to Vienna, with the least possible delay, my report on the result of the consultation. I hastened to obey this order, and the day after my arrival at Milan, a conference took place between Dr Hartung, Professor Flarer, and me. Dr Hartung explained before his excellency the whole course of the disease, in complete accordance with the narrative he had sent me in writing; while Professor Flarer, on his part, communicated the observations he had made, and mentioned the changes he had noted at the two visits he had paid the illustrious patient. I then proceeded to investigate the disease, in order to assure myself of its symptoms with my own eyes, and found them to be as follows:—

The globe of the right eye, sound in all its parts, is completely pushed from its cavity by a tumour, which not only entirely fills the orbit, but even greatly extends beyond it; the projection of the eye and the extraordinary retraction of the eyelids being such, that, could they close, they would do so behind the globe. Felt through the eyelids, the tumour upon which the bulb rests motionless as upon a cushion is hard as a stone, unequal, and bossulated; pressing it causes pain. The eyelids, surmounted by this tumour, are pushed forward, and enormously stretched in every direction, swollen, and motionless; they are strewed over with varicose vessels; the colour of their external surface is changed, as well as that of the parts of the face which surround them, into a deep blue, almost black; their conjunctival surface, of a dirty red colour, is covered with bloody mucus and pus, and presents equally with the external surface a dense network of varicose vessels distended with blood. The lacrymal apparatus appears as much affected as the other soft parts. The gland is displaced like the globe of the eye; it is pushed into the duplicature of the upper eyelid, close to the upper and outer edge of the orbit, where it forms a smooth, oval swelling; the caruncle and sac are prodigiously enlarged, hard, and painful to the touch. The sensibility of the diseased parts is much deranged; they are subject to pain, sometimes sharp and transient, at other times burning and continued; the heat of the parts is increased.

The whole presents the frightful aspect of a scirrhous degeneration of the soft parts within the orbit, developed to the greatest degree, and on the eve of passing into the condition of undeniable cancer. This form of disease, which is by no means rare in prac-

tice, cannot have failed to have been often observed in its progress by every surgeon of experience.

As to the general health of the patient, the contrast is striking. The Marshal eats and drinks with appetite; his functions are natural; the strength of his body corresponds to his age; sleep and sensibility[1] are good, except when interrupted by periodical local pain. The mien of the patient, and the colour of his delicately organised skin, are those of a man enjoying perfect health; the colour does not present the waxy hue, nor do we remark in the physiognomy, nor in the habit of body, the deep-seated air of suffering, the depression, the loss of strength, nor the wasting of the soft parts, which are generally the effect of an advanced carcinomatous affection, although yet local, or of a general dyscrasia.

When I thought myself sufficiently informed, and was on the point of retiring with my colleagues to consult, his excellency addressed me in these remarkable words:—"Although you may discuss, and whatever be the result of your conference, I formally tell you I shall be treated by no other physician than Dr Hartung, and in no other way than according to the homœopathic system." This declaration was peremptory, and rendered my position, already sufficiently embarrassing, still more difficult. Nevertheless these words did not prevent me from pursuing the task confided to me, and fulfilling my duties as physician, yet without departing from the consideration due to the not less difficult position of the ordinary medical attendant.

At length, finding ourselves alone, I repeated anew to Professor Flarer and to Dr Hartung my questions on the nature of the disease, and on the possibility of curing it. The professor considered it, as he had previously done, as the result of a *scirrhous action* in the orbit, and, founding upon daily experience, doubted therefore the possibility of a cure. Dr Hartung denominated the disease a *fungus*, in the stage of transition into decided cancer, and said that, although up to that moment he had employed all his science in vain, he purposed still to try certain homœopathic remedies, and in particular *carbo animalis*. Having interrupted a useless discussion between Dr Flarer and Dr Hartung regarding animal charcoal, and having declared that, after having heard the intention of his excellency plainly announced, any discussion as to treatment seemed no longer to form an object for consultation, while it remained for us only to decide on the nature and form of the disease in question, and on the natural and necessary results of it, I explained, in my turn, after a recapitulation of the origin and course of the disease, my views of its nature. I said, that I regarded the tumour which pressed the globe out of the orbit as the pseudo-critical product of a *rheumatic periorbitis* which had preceded, and which, having taken

[1] *Cénesthèse* is the word here rendered sensibility. Συναίσθησις, however, whence I presume is cénethèse, signifies rather consciousness of disease.—*Tr.*

an unfavourable turn, must, according to my opinion, and judging from the form and characteristic aspect which the local affection presented, have degenerated into a scirrhous disease, which, passing into the state of actual carcinoma, must, according to the experience of all medical men, end inevitably in the death of the patient.

I sided then with the opinion of the ordinary physician as to the manner of regarding the affection and its consequences; but I said, in concluding, that the disease, however malignant it appeared in form, had not the less remained local, and confined to the soft parts which surround the eye; that the patient was relatively in good health in every other respect, while no actual symptom authorised us to admit the existence of a primitive cancerous dyscrasia, or of a general dyscrasia produced by the local disease; so that, upon the whole, I believed myself bound to point out, that we might still cherish the hope of a completely favourable issue, and of a perfect cure. This would be the case, if an increase of the inflammatory reaction should occur in the tumour; that then it might be possible for the hardness to soften and a healthy suppuration to take place; and that a cure might thus supervene after an evacuation of pus.

As this view of the disease, and of its future development, did not seem to meet the assent of Dr Hartung, who pronounced neither for nor against it, and as I wished to avoid raising the smallest contention, it was decided, with one accord, that the original diagnosis—viz., that the disease consisted in a scirrhous degeneration of the soft parts within the orbit, threatening to pass into the state of cancer, and which would necessarily end by undermining the life of the patient—should be considered the result of our consultation, and form the basis of my report to Vienna.

Having returned to the Marshal, I apprised him of the result of our consultation, without, however, making mention of the diagnosis which we had fixed upon, nor of the consequences we had contemplated, adding that I was, as he well knew, no homœopath, and that consequently we had abstained from entering on any discussion relative to the treatment, but that, conformably to his express wish, we had left this to his ordinary physician. Still, to tranquillise the patient, I believed that, with respect to the future course and the possible issue of the disease, I should communicate to him the same opinion which I had announced in the consultation, and I therefore added, word for word, "that I hoped the inflammation of the tumour might speedily increase, and sharper pain be felt in it, seeing that that would not fail to lead to a suppuration, which, by means of a discharge of pus, might effect a cure."

I did not communicate this expectation to any one else, except to a lady, a very near relation of the Marshal, and to her husband; for faithful to my duty and to what had been decided on in the consultation, I made mention of it, neither in my report, nor in my conversations with those forming part of the civil or military authorities, then present in Milan. I relied, should suppuration actually

happen, on the veracity, and the fidelity of a man of honour and a colleague.

But what took place?

Some weeks after my return to Vienna, the rumour ran that the Marshal was better. I received at the same time a letter from the lady in Milan, of whom I have spoken, in which she acquainted me that my hope seemed on the point of being realised, for that an increase of pain had occurred, and had occasioned a copious discharge of pus from the diseased organ. In a report addresed to the minister of war, we read:—"I have again seen, for the first time these three weeks, the illustrious patient, and I have been surprised at the considerable amelioration of his state; I abandon myself, therefore, completely to the hope of his perfect cure. Suppuration has taken place; the pus is yellow, and what is called healthy; the eye has completely receded into its cavity."[1]

While this favourable change was going on, and yet Dr Hartung, as the lady referred to wrote me, continued by his contradictory hints to keep up anxiety, Professor Flarer was once more called to the patient. This gentleman saw the Marshal; but he saw neither the diseased eye nor Dr Hartung; it was necessary to defer the consultation till next day. But who was it who did not arrive at the hour fixed on? Dr Hartung. He excused himself on the score of indisposition, giving at the same time precise orders that the dressing was not to be removed from the eye during his absence, nor in presence of Dr Flarer. Without, then, having been able to examine anything, without having been able to collect any information whatever from Dr Hartung, relatively to the progress of the disease, since I had quitted Milan, Dr Flarer had the trouble of a useless journey, and thus returned to Pavia. While this went on, Dr Hartung wrote me at Vienna, "that the Marshal, having continued to be treated according to the principles of homœopathy, had got well; that the fungous tumour of the orbit had disappeared; that the pain had ceased; that the eye had recovered its wonted mobility, and that the change of fungus into carcinoma, which had been supposed at the consultation was no more to be dreaded; and, lastly, he expressed how much he regretted that Dr Flarer had not had an opportunity of convincing himself, with his own eyes, of the favourable change." What still more characterises the procedure and the policy of our colleague, Dr Hartung, is the report which he addressed at the same time to the minister of war, and in which it is said in these very words, "that although Professor Jäeger had pronounced the Marshal incurable and lost without resource, he notwithstanding was already much better, and that he (Hartung) would cure him not the less, entirely by homœopathy." To this report was appended, apparently to display to the eyes of the non-initiated all

[1] It would have been satisfactory to have known by whom this report was made, but this is not mentioned.—*Tr.*

the superiority of his art, a coloured design of a diseased eye, pushed by a tumour out of its cavity, and supposed to represent that of the Marshal. It is worthy of remark, however, that the Marshal had never permitted a drawing of his diseased eye to be made, and that the design in question was only a bad copy taken from the work of Panizza, "Sul Fongo Midollare dell' Occhio," and that, probably by mistake, in place of the right eye of the Marshal, which was the one diseased, a left eye was represented. Dr Griesselich, of Carlsruh, to whom this design had been sent for publication in his Homœopathic Journal, writes me, "the miserable design of the diseased eye of Count Radetsky remains in its original state in my hands. I received it along with a piece of bombast inviting me to insert it in my Journal; but I cannot afford room for things so contrary to sound pathology."

The letter of Dr Hartung, to which I have just referred, was his last communication to me. It was only by the newspapers and verbal reports, that I learned the recovery of Count Radetsky. The sensation and the talk which the news of the cure of the Marshal every where excited, are easily understood. The cure was calculated to surprise and to astonish the more, as, according to the result of the consultation by this time made public, and always with the authority of my name, every one expected to hear precisely the reverse.

It is well known, with what songs of triumph, in the drawing-rooms, as well as in the political and literary journals, this pretended homœopathic cure has been puffed off, and spread abroad by all the homœopaths and their fanatical adherents, with a designed concealment all the while of the circumstances, and the appearances, under which this favourable change had taken place. Such triumph should astonish no one; the contrary only would have been surprising and incredible.

The effect which such a perversion of the truth, and so culpable a manner of disseminating this affair, had upon me, was that of indignation and of contempt for all who were implicated in it; and I felt such a disgust at them that I thought it unworthy of me to resolve publicly to raise my voice against frauds, calumnies, and falsehoods, so odious. But having afterwards been accused, in the most hateful way, of being the author of a paper entitled, "Homœopathic Baseness," which had appeared in the "Salzburg Medico-Chirurgical Gazette," a paper designed, it is true, to establish the truth, but which, having been drawn up by some one who was not an actor in the affair, contained several inexactitudes, then it was that I believed myself obliged, for the honour of our art and of science, to expose matters such as they had passed in reality, and to make them public by means of the "Universal Gazette of Medicine of Berlin," and the Homœopathic Journal of Griesselich — the "Hygiea of Carlsruh."

From that time all controversy ceased, at least as far as I know,

till the period when the victories of Marshal Radetsky[1] aroused in Dr Hartung and his partisans the idea of profiting thereby to gather a new triumph. The pretension, that he who had preserved from premature death the "saviour of his country," had, as well as the hero himself, a most just title to public gratitude, was neither so very absurd nor so ill advised! This pretension, with all its conclusions, was actually produced in the political journal of Agram; but the article relating to it again contained premises and conclusions so false in regard to the cure of the celebrated Marshal, and advanced imputations so injurious to me, that it seemed again to be my duty to interfere, and to re-establish the truth by some remarks which I caused to be inserted in the "Vienna Gazette," the official organ of government, and in which it was easy for me, by referring to the illustrious Marshal himself, to unmask falsehood and calumny.

Such is the history of the disease and of the cure of Marshal the Count Radetsky.

In conclusion, and by no means to excuse myself for having been mistaken, but to offer a salutary advice to my young colleagues, I shall finish by making known my opinion.

The course and the issue of the disease remove all doubt as to its true nature and its characteristic form; it was from the first nothing more than a simple *rheumatic periorbitis*. In consequence of bad treatment, or rather in consequence of the absence of all treatment, the disease proceeded in its course; a pseudo-critical deposit, always augmenting in bulk, pushed the globe of the eye more and more from its natural place in the orbit, making it pass through the opening of the eyelids, and thus occasioned, as so many secondary effects, all the symptoms narrated in the preceding description. The intra-orbital exudation passed into a state of suppuration, and ended in an actual abscess, which, at the time of my visit to Milan, had probably already formed in part, as I suspected for reasons which I have explained. It was upon this very supposition that, at the consultation, and in the presence of the Marshal, I pronounced the possibility of a cure by such termination, without having, it is true, sustained this declaration with certainty and precision. The circumstances which most contributed to the great difficulty of recognising perfectly the true state of disease, were chiefly the age of the patient and the chronic course of the complaint, along with the entire external aspect of the case, and the striking likeness it presented, in form and symptoms, to a scirrhous affection of the intra-orbital parts, on the eve of passing into a state of decided carcinoma. Besides the difficulty of ascertaining the presence of an abscess in the orbit by sight merely, or even by touch, on account of the peculiarity of the parts surrounding the orbit, to say nothing of the deceitful sensation of fluctuation which often attends a fungous

[1] The victories of the Austrians over the Sardinians in 1849.—*Tr.*

degeneration, it must also be observed that I was obliged to make up my mind as to the case from a single examination of it, the patient absolutely refusing to submit to any sort of treatment different from the homœopathic, or to any such ulterior scrutiny as might assist me in forming a more deliberate judgment.

All those who have had occasion to observe and to treat diseases of this kind in the periorbital structures (and every practical surgeon knows them by experience, as they are by no means rare in their occurrence), will be able fully to appreciate the difficulties, which, in cases of the sort, render the diagnosis so doubtful; and if they are honest, they will frankly confess how easy it is for us to deceive ourselves. How frequently has it happened that, convinced he was about to open an abscess, the surgeon has plunged his bistoury into a fungus!

ARTICLE V.—*Monthly Report on Cases treated in the Regimental Hospital, 79th Highlanders, during the month of December* 1852. By JOHN GRANT, Esq., Surgeon.

Syphilis.

TWELVE cases of primary and one of secondary syphilis were treated during the month.

The exhibition of mercury in any form for the treatment of primary syphilis has now been entirely abandoned, superficial sores readily healing under the local application of nitrate of silver, or sulphate of copper, with the ordinary lotions of acetate of lead and sulphate of zinc.

Chancres with an indurated base are treated with the daily application of solid lunar caustic, and where the surface is foul, or indisposed to heal, black wash is sometimes employed.

The practice of administering gray powder or blue-pill for the purpose of expediting the resolution of buboes, and preventing the appearance of secondary symptoms, has been found so unsatisfactory, that for some time past I have ceased to employ any persistent efforts for the discussion of *suppurating* buboes; and as soon as matter has formed, a poultice is applied, and the abscess opened with potassa fusa.

The dispersion of a bubo by the absorption of its contents into the system does not appear, *à priori*, to be a very desirable result, or one unattended with danger; and recent observation has led me to trace a great proportion of cases of secondary syphilis to subjects in whom buboes had been discussed.

It is not to be inferred, that every glandular enlargement in the groin is on its first appearance fomented and poulticed, with a view to promote suppuration; on the contrary, the discutient system is

tried at the commencement, and this object is often attained by the application of poultices, and vesicating the surface by nitrate of silver or cantharides; but as soon as matter has formed, the usual means are employed to bring it to the surface, without further attempts to promote resolution.

I subjoin a tabular view of all the cases of secondary syphilis treated during the year ending 31st December, from which it will be observed, that the antecedents were generally discussed buboes, and that, out of all the admissions with syphilis primitiva in 1852, the only two instances of secondary symptoms occurred in persons who were salivated for the primary affection.

No doubt, the experience afforded by a single regiment is not very extensive; but should the comparative frequency of secondary syphilis, after the opening or dispersing of buboes, appear to be a question of any interest, the aggregate practice of military hospitals affords peculiar facilities for its elucidation.

Return of Admissions with Secondary Syphilis during 1852.

No.	Name.	Primary symptoms.	Primary symptoms appeared.	Remarks.
1	W. B.	Bubo discussed.	1850	Secondary symptoms in 1850, 1851, and 1852.
2	D. A.	Not known.	1851	
3	A. D.			
4	J. S.	Bubo discussed.	1851	
5	A. A.	Ditto.	1851	
6	J. W.	Ditto.	1851	
7	D. M'B.	Bubo unopened and concealed for a month.	1851	
8	W. W.	Bubo discussed.	1852	Salivated for primary symptoms.
9	C. C.	No bubo.	1852	

No mercury has been administered of late for the cure of syphilis consecutiva; and it is proposed to treat cases occurring during the present year with sarsaparilla, iodide of potasium, etc.

Hepat. Acut.

This case is noticed, not from any peculiarity in the symptoms, but on account of the infrequency of its appearance, being the first that has occurred in the regiment for several years.

The patient complained chiefly of pain in the right hypochondrium and top of the shoulder; but decubitus was easiest on the left side.

Repeated general and topical venesection, with calomel and

opium to salivation, were the remedial means employed; and he is now convalescent.

Empyema.

As stated in the report for last month, paracentesis thoracis was performed, and the operation has unquestionably had the effect of protracting the patient's existence, seven weeks having now elapsed since its performance; but as there is a fistulous opening in the lung, and matter continues to be discharged in considerable quantity through the external incision, a favourable prognosis can hardly be entertained.

Ulcers.

During the quarter ending 31st ult., unusual difficulty has been experienced in the healing of sores; incisions of the cupping scarificator have degenerated into superficial ulcerations; and wounds generally have evinced a disposition to spread;—a condition which I am inclined to attribute to the unseasonable character of the weather, the average temperature of which during December has been higher than for many years past, the prevailing winds being westerly, and attended with almost incessant rain.

EDINBURGH CASTLE, 15th January 1853.

Part Second.

REVIEWS.

An Essay on the Action of Medicines in the System, or "on the Mode in which Therapeutic Agents introduced into the Stomach produce their peculiar Effects on the Animal Economy," being the Prize Essay to which the Medical Society of London awarded the Fothergillian Gold Medal for 1851. By FREDERICK WILLIAM HEADLAND, B.A., M.R.C.S., etc. 8vo. London: Churchill. 1852.

WE congratulate Mr Headland on his appearance as the author of this work. We do not mean merely because it is an *ouvrage couronné*, as our French neighbours say, though that may be an enviable distinction, but because it shows the writer to be a thinker, a man of some observation, and a good deal of reflection, and one who has willingly devoted time and talent to the praiseworthy object of endeavouring to throw some light into one of the dark corners of medical science. And if we should say no more in his favour than this, we should, we believe, be awarding to Mr Headland no mean praise. In these days when many books are written with less regard to the

furtherance of medical science, or to the solution of medical difficulties, than to the advancement of the authors in practice, it is refreshing to find a gentleman able and willing to devote himself to the investigation of medical theory. It is not a money-making speculation, but rather, to our eyes, an act of professional patriotism, the merit of which is not in the least diminished, we apprehend, by the circumstance of its having been performed under the stimulus of a prize competition. Ten men will enter the lists for a prize when the subject of competition is what is called practical, for one who will do so when it is theoretical and speculative. Mr Headland, however, may rest assured, that such writings as his, will, in the end, be found not unprofitable, either to himself or others. The book may not proclaim him to the world as an authority on this or that disease, and thus directly lead to consultations and honoraria; but his reflections on therapeutics, whether in the meantime right or wrong, will have tended to enlighten his own mind, and if studied and thought over by others, will enlighten them, whether their reading results in their giving an intelligent assent to his doctrines, or in their rejecting them for others which they think more consistent with truth. We fear, however, that there are many persons in the profession to whom such a book is not much better than "caviare to the general;" it is not practical enough for them, and so they will have none of it. It is well for such persons that they have others to think for them. Some day or other they will reap the benefit of this, in getting clearer views as to what they are about when they are administering remedies. We don't profess to say that their harvest is yet come, but it never will arrive, unless people like Mr Headland turn up the soil and sow the seed for them. We entirely sympathise with the following passage from Mr Headland's introductory remarks :—

"There have been, more or less, in all ages, two systems or schools of medical treatment, of which the one prevails among ignorant men, and in rude states of society, but the other requires a higher degree of enlightenment. These are the Empirical and the Rational systems. The first is founded on simple induction. By accident or by experience it is found that a certain medicine is of use in the treatment of a certain disorder; it is henceforth administered in that disorder; and on a number of such separate data an empirical system is constructed. It naturally requires for its elaboration a comparatively small degree of knowledge.

"Now this observation of facts is indispensable as a beginning, but something more is required. We must not be satisfied with taking them separately, but we must proceed to compare together a large number of facts, and draw inferences from this comparison. And our plan of treatment will become rational, when, on the one hand, from an accurate knowledge of the symptoms of diseases, we are better enabled to meet each by its appropriate remedy; and, on the other hand, from some acquaintance with the general action of a medicine, we are fitted to wield it with more skill and effect, and to apply it even in cases where it has not yet been proved beneficial. Thus, for the proper perfection of medicine as a rational science, two things are in the main needed; the first is a right understanding of the causes and symptoms of disease; the second, a correct knowledge of the action of medicines. Should our acquaintance with these two subjects be complete, we should then be able to do all that man could

by any possibility effect in the alleviation of human suffering. This sublime problem is already being unravelled at one end. Diagnosis and nosology are making rapid strides; and perhaps we shall soon know what we have to cure. But at the other end our medical system is in a less satisfactory condition; and though some impatient men have essayed, as it were, to cut the Gordian knot, and have declared boldly on subjects of which they are ignorant; yet it must be confessed, that in the understanding of the action of medicines, and of their agency in the cure of diseases, we do not so much excel our ancestors. While other sciences are moving, and other inquiries progressing fast, this subject, so momentous in its applications, has, in spite of the earnest labours of a few talented investigators, made after all but small progress. Let but those who feel this want bestir themselves to remove it, and it will soon be done. Those doubts and difficulties, which are now slowly clearing away before the efforts of a few, will then be finally dispelled by the united energies of all; and instead of our present indecision and uncertainty on many points, we shall find ourselves eminently qualified to wage the conflict with disease, being skilled in that science whose name bespeaks its peculiar importance, the science of *therapeutics*."—Pp. 2, 3.

We hold it with Mr Headland to be an undoubted fact, that the science of therapeutics has made but little progress compared with other branches of medical knowledge. Can we assign good reasons for this? The stereotyped reply is, that it is owing to the inherent difficulty of the subject, or, as our author expresses it, "the variety and complexity of the proof required to establish any one point with absolute certainty." Anybody who has taken the least trouble to reflect upon the *modus operandi* of medicines, must soon become aware that there is truth in this. Before we can make up our minds on the action of a remedy, we must make up our minds also, to some extent at least, as to the pathology, and as a preliminary to this, to the physiology of the organs which it may concern, and this no doubt makes a very complex business of it. But is this the whole truth as to the causes of the slow progress of therapeutics? We suspect not. It is not the characteristic of modern science to be deterred from the pursuit of truth by the mere difficulty of the chase. We appeal to physiology, pathology, and chemistry for proof of the contrary. How comes it that therapeutics is an inglorious exception? It is, we believe, in great measure, because it has as yet received very scanty justice at the hands of the profession generally. Therapeutics, in short, is not fashionable; and nine-tenths of the profession either "cut it dead," as an unsuitable acquaintance, or, if it is pressed upon their recognition, "tip it the cold shoulder." Start a new doctrine, or open up a new subject of inquiry in chemistry, and a hundred crucibles are hot to put it to the ordeal. Declare a fresh fact or theory in pathology, and a hundred scalpels are sharpened to dissect it out. Discover a new globule or corpuscle, and as many microscopes are illuminated to pore over it, and draw it, and measure it. Hint at a new doctrine in physyiology, and the blood of a hecatomb of curs flows to float it before the eyes of the profession. This is all well so far as it goes. But how fares it with therapeutics? Propound a new view as to the *modus operandi* of a class of medicines, and a hundred heads are shaken at it, as a thing "not in their way"—like a bad

lot in an auction room. We do not wish to concentrate the blame of this upon that mere matter-of-fact section of the profession who think nothing worth knowing, but whether opium should be given here or calomel there. We fear that throughout the profession there is a too prevalent idea that the study of the *rationale* of the action of remedies is a matter of little importance to the physician as a man of art. We hold this to be entirely erroneous, believing, on the contrary, that just in proportion as we extend our knowledge of therapeutics, we help to raise medicine from the position of an empirical art to the status of a practical science. Therapeutical science has always appeared to us to bear nearly the same relation to the physician that mathematical science does to the engineer, civil or military. We don't deny that an enemy's magazine may be blown up by a bombardier unconscious of mathematics. An artillery sergeant, with a mortar and shells at his command, would certainly undertake to do it, and would in the end probably succeed. To be sure his first shell might fly fifty yards too far, and burst high in the air; and his second might fall as much too short, and be three feet under ground before it exploded at all; but his third might be more lucky, and we should then have a good example of the successful empirical application of what Mr Headland calls a catalytic remedy (καταλυω—I demolish or destroy). But if the sergeant were to call his scientific captain into consultation, he would probably be informed that there was a much better way of accomplishing the catalysis of the enemy, than by making a series of random shots. He would tell his subordinate that his shell, like other projectiles, as it flew through the air, was acted on by two forces; one the propelling power of his charge of powder, the other gravity. If the former only acted on the shell, it would fly on in a straight line, perhaps till it knocked out the eye of the Ursa Major, and at any moment the distance travelled by the shell would be proportionate to the time occupied. If the latter alone acted on it, it would bring it vertically downwards, with a motion not uniform, but constantly increasing, the distances passed over in any given time being proportional, not to the times, but to their squares; but as both propulsion and gravity were acting on the shell, it would neither fly straight on nor fall straight down, but would mark out for itself a path, which would prove to be a parabola. Now all this might appear to the sergeant mere unpractical theory. But he would be further told, that previous physical (we shall call it physiological) experiment had shown what the explosive power of gunpowder is, and calculation had determined all these lines and rates of motion; and what he had to do was to make an accurate diagnosis of the distance of his enemy, and to use such and such a dose of gunpowder and size of fusee, and then he would find that he would probably hit his mark at the first shot, to the great saving of time, trouble, and ordnance stores.

True physiology and pathology, the bases of our theories, are not exact sciences, like physics and mathematics; and therefore we must be cau-

tious in our rationalism, and be prepared in many instances, like the sergeant, to act without any guiding theory, and to meet with many failures. Experience is invaluable, and it is absurd to attempt to underrate it. British medicine is justly proud of the practical skill of its professors; but we fear that this feeling has engendered among us too great a disposition to rest contented with our present condition, and to look upon attempts to make medicine scientific, as something which will be accomplished only at the risk of rendering it less successful. We do not know upon what other ground to explain the fact, that in this country the *rationale* of the *modus operandi* of medicines has been comparatively too much neglected. We suspect that the profession generally must plead guilty to having been unkind to therapeutics, and slow to afford it that amount of encouragement and aid which its present embarrassed circumstances entitle it to receive. To this it may be replied, that the fault lies with therapeutical writers themselves, that they have themselves to blame for the little attention which has been accorded to them; nay, that the very author now before us brings this charge against them in the following terms:—

"On no question, perhaps, have scientific men differed more than on the theory of the action of medicines. Either facts essentially opposed and incompatible have been adduced by the disagreeing parties; or, which is nearly as common, the same fact has received two distinct and opposite interpretations. Many hypotheses, when tested, are seen to be grounded on mere assertions, and to be destitute of logical proof; many others are attempted to be established on ill-sustained analogies. Analogy, in such a case as this, may be used to increase a probability already evidenced, but by itself it is no proof; for we find often that medicines are capable of producing the same result in very dissimilar ways."—P. 5.

We do not deny that there is some truth in this. We admit that therapeutics has put into circulation much that is bad paper; and there is some excuse for people being shy of accepting its bills. But we are not prepared to allow that it is utterly bankrupt. It has its embarrassments, but these are not owing to sheer reckless trading. It is still but young in business, and has many obstacles to contend with. It has often been left in difficulties by the house of Physiology, Pathology, and Co., with whom it has extensive dealings; and though they have been doing well in the world of late, they have not always fulfilled their engagements to therapeutics so promptly as they ought to have done. It is in fact one of the "suffering interests," of which we have all heard elsewhere. It has peculiar burdens of its own. The articles in which it deals are to some extent of a precarious nature, and require great labour to produce them. It needs protection. Let it then be relieved by a more equitable adjustment of professional favour, and it will hold up its head yet. Most happy shall we, as the friends of the therapeutical interest, be, if we can, on further inquiry, find that this is a mistake; that therapeutics is a thriving concern after all; that amidst our lamentations over it, in spite of unrestricted importation from other more favoured sources, it has begun to produce a greater amount of market-

able commodity than appears in any former returns. We shall be too glad when this occurs to abandon the cry of distress altogether, and leave it to fight its own way in the world; but in the meantime we must bespeak for it a moderate protection from the profession. But before we quit our political metaphor, we must, as sound protectionists, couple our petition for aid from without, with a piece of advice to the suffering therapeutic class, whose friends we profess to be. We admit that you require protection, but do not trust to this alone. You must help yourselves if you wish others to help you. You must not restrict the cultivation of your arable land, but, on the contrary, take to higher farming. Here is the advice which one of yourselves gives, and we hold the advice to be sound:—

"How then are we to arrive at the truth? The best and surest way is to be extremely careful in the means which we employ in its discovery.

"It is, I think, impossible to overrate the importance of *exact precision of language and thought* in scientific details, and in the deduction of conclusions from them. A subject so interesting as this requires to be treated in a logical way. Facts, when ascertained, should be ranged together and compared, and exact inferences made, without ever straining a point. And when we are inclined to hazard a theory that is barely supported, we should take care to state it as a theory, and not to bring it forward as a truth. It has not been an uncommon habit among scientific authors, who should be of all men the most careful and exact, to confound assertion with fact—to mistake hypothesis for truth. In such illogical and incorrect reasoning is to be found the true source of a multitude of errors."—P. 5.

We believe, to descend from corn to physic, that the present backward state of therapeutics is owing to a combination of the following causes:—1st. That it is inherently a difficult subject. 2d. That it has suffered from the extremely hypothetical views which have too often been put forward, as Mr Headland says, "on bare assertions, and without logical proof;" and, 3d. That it has not received from the profession generally that measure of interest in its progress, without which no science can thrive.

We are, however, hopeful of the future, and have somehow or other on our minds a presentiment that a great era of therapeutical discovery is not far distant from us. The profession, we trust, is beginning to see that some good may be hoped for from this quarter, and it becomes us to acknowledge with sincere approbation, as one of the good signs of the times, that the Medical Society of London has devoted its gold medal to the subject of the theory of the action of remedies. It is also a good sign of the times, that the call should have been responded to by the production of so creditable a book as Mr Headland's, on which we beg to offer a few observations in the way of amicable discussion. We shall be glad if our remarks induce any of our readers to peruse it. We do not promise them much of what is called fine writing. Therapeutics does not readily admit of the ornate in composition. But they will find the style of Mr Headland's work perspicuous and agreeable. He expresses his own views decidedly and modestly, and from his having stated his doctrines in the form of distinct propositions, each of

which is discussed *seriatim*, it is a comparatively easy task to follow his argument. Those who have read and reflected much on this subject, will, we believe, find much in which they cannot concur, but they will also find a good deal of acceptable doctrine stated in a clear and agreeable form. Those who have not bent their attention much or at all to the theory of the *modus operandi* of medicines, here have an excellent opportunity of beginning such a course of study. They will find it neither profitless nor unpleasant. We confess to having spent many an hour in speculating as to how the medicines we had been prescribing were accomplishing the service we expected from them. We have always found such reflections an agreeable occupation in our study, and not unfrequently useful to us at the bed-side—*delectant domi, non impediunt foris.*

We have at some length pressed upon our brother practitioners the propriety of a more habitual practice of mentally discussing the *modus operandi* of their prescriptions, and we have commended to their notice Mr Headland's book, quite irrespective of any consideration on our part, as to whether the therapeutical doctrines which any one may form for himself, or those specially upheld in the present instance by Mr Headland, be true or not, because in the present unsettled state of our knowledge on this subject, victory for any particular set of opinions is not so important as free and full discussion. Mr Headland, we are sure, albeit unknown to us, save by his book, will coincide with us in this opinion. And with this impression upon our minds, we proceed shortly to analyse his work, presuming that we do not abate one jot of the general commendation we have bestowed upon it, when we venture to illustrate the present divided state of opinion on the action of remedies, by entirely dissenting from some of the more prominent opinions advanced by him.

The book consists of four chapters. The first, entitled Introductory Remarks, is a short abstract of the whole volume, with some general observations on the importance of the science of therapeutics, and the rules to be observed in studying it. We do not propose to offer any further observations either on it or on Chapter II., which is "On some of the more important classifications of medicines, and opinions of authors respecting their action."

Chapter III. constitutes the great bulk of the work, being "On the general modes of action of therapeutic agents introduced into the stomach, treated of in ten propositions."

The first of these propositions is—"*That the great majority of medicines must obtain entry into the blood or internal fluids of the body before their action can be manifested.*" The phrase "internal fluids," Mr Headland explains by stating that whilst in the "majority of instances, the medicine enters the blood directly, yet that it would be sufficient for its operation if it were to enter through the chyle or into the serous fluid which exists in the intestine of the tissues throughout the body. For by this it might be conducted to distant parts." We do not exactly see how this can be the case. Mr Head-

land does not mean to say that medicines actually are commonly absorbed along with chyle by the lacteals, on the contrary, he elsewhere denies this; but supposing a medicine were in the lacteals, or in a serous cavity, or in the cellular tissue, what are these but ready channels by which it can find its way into the blood? We apprehend, therefore, that he has unnecessarily complicated his proposition by this addition of the "internal fluids." A medicine could not reach any distant organ without in the course of its journey mingling with the blood.

We quite agree with our author in believing that, as regards medicinal doses at least, they generally are absorbed into the blood. And the proofs which he, in common with other authors, adduces in support of this are tolerably conclusive, viz.,—"That a medicine, when introduced into the system elsewhere, acts in the same way as when introduced into the stomach,"—"That the continuity of nerve is not necessary for the propagation of such effects; but vascular connection is necessary," [experiments of Magendie, Brodie, and Ehbert (Emmert?)]—"That the circulation of blood is sufficiently quick to account even for the operation of those poisons which act most rapidly by influencing the nerve centres," [experiments of Blake, Hering, and Volkmann]—and lastly, "That the great majority of medicines which have been detected in the blood, are found in the secretions formed out of it." We agree so far with Mr Headland that the preponderance of evidence is in favour of this doctrine of absorption. But although the admirable experiments of Blake lead us to hold very different views from what we once entertained as to the rapidity with which a poison may travel from one part of the system to another, we cannot put out of view the equally accurate experiments of Dr Christison with conia, where the poisonous effects were produced, as we ourselves witnessed, apparently instantaneously, but at all events in a space of time which could not exceed two or three seconds, which is less than the greatest rapidity assigned to the passage of any poison along the circulation; and even if we should set these positive observations aside, and adopt the negative results obtained by Blake, we should still be inclined to share in the opinion noticed by Mr Headland, that this, after all, is only a proof of *possibility*, and does not by itself show that a substance may not act by the nerves. And as to that which he holds to bring the proof as close as possible to a certainty, viz., the poison being found in the blood, we see nothing to prevent its passing into the circulation after its poisonous action may have begun through the nerves, nor is there anything unphilosophical in holding that a substance which in a moderate medicinal dose may require absorption to enable it to produce its effects—may, when in an overwhelming poisonous dose, act by nervous impression. Our present inquiry, however, concerns substances acting as medicines, not as poisons, and we therefore willingly concede that, as a general rule, *medicines* act by absorp-

tion. To this there is the exception, noticed by Mr Headland, of certain substances which act chiefly in virtue of a local irritant action, such as irritant emetics; but as these form a separate subject of consideration in his fourth proposition, we do not notice them at present.

Mr Headland's second proposition is, "*That the great majority of medicines are capable of solution in the gastric or intestinal secretions, and pass, without material change, by a process of absorption, through the coats of the stomach and intestines, to enter the capillaries of the portal system of veins.*" And this is followed by proposition third, "*That those medicines which are completely insoluble in water and in the gastric and intestinal juices cannot gain entrance into the circulation.*" These two propositions are inseparably connected together, and if there be any doubt as to the correctness of No. III., a great part of the argument of No. II., which goes to prove by what means some substances are dissolved in the stomach and intestines so as to become absorbable, would be unnecessary. But, without meaning to affirm that it is an established fact that minute particles of an insoluble substance actually are capable of passing into the circulation, we must say that Mr Headland's argument to prove the contrary is very far from satisfactory. He meets with the positive results of Oesterlen, who states, that after administering charcoal to rabbits for some days, when the animals were killed, globules of charcoal measuring from $\frac{1}{6000}$ to $\frac{1}{3000}$ of an inch were found in the blood of the portal circulation. "To obtain these results," says Mr Headland, "the microscope was used, a far less certain test in such matters than chemical analysis." We dispute the correctness of this remark. Seeing is believing; and we know of no chemical test which could by any possibility have proved the nature of these particles so well as the microscope, and no substance which could have furnished so undoubted an instance of an insoluble matter as charcoal. To meet these positive, and as it appears to us reliable, statements, Mr Headland presents some experiments of his own, one of which we recite in his own words:—

"EXP. I.—Ten grains of calomel were given to a large dog. It was killed after three hours, allowing this time for digestion. A considerable quantity of blood was collected from the portal vein, and submitted to analysis, to determine whether it contained any compound of mercury in an insoluble form. The blood was dried and pulverized. The result was boiled for some time in water, and the insoluble part collected. It was dissolved in a small quantity of aqua regia, and the clear acid solution placed in a test tube. A slip of zinc foil was folded round a narrow plate made of gold foil, and introduced into the solution. A galvanic current being thus set up, the minutest quantity of mercury, if present, would have been deposited on the gold, so as to tarnish it. But this did not take place, and when at last the zinc was completely dissolved, the gold remained as bright as before. Thus there was *no* calomel, or compound of mercury, present in the insoluble form."—Pp. 76, 77.

Similar trials were made with ten grains of mercurial ointment, five grains of oxide of silver, and ten grains of sulphur,—the latter being sought for in the chyle, as preferable, for several reasons, to the blood.

But upon these experiments we may remark that, besides labouring under the disadvantage of only opposing negative results to positive, they are incomplete in themselves. They do not afford proof that any of these substances was absorbed either in solution or otherwise. True, we have proof otherwise that calomel *can* be absorbed, for people can be salivated by it; but when a single dose of ten grains is given, is there, in the first place, enough absorbed in any form to be detectable by chemical analysis? and, in the second place, if any was absorbed, did Mr Headland collect the portal blood at the proper time for finding the calomel there? Considering what we have been saying as to the rapidity of transmission of matters by the circulation, and in the absence of all proof that a continued slow absorption of calomel in a soluble form was going on during these three hours, it is quite possible that if any mercury was absorbed at all, it might have mostly passed away from the portal circulation, and been so diffused throughout the body as not to be capable of being detected in the blood which he examined; or, such a dose as ten grains, which would certainly act cathartically, may either have been pushed along the intestines, along with the increased secretion from them which its presence would excite, and so not absorbed at all; or, if absorbed, may, to follow Mr Headland's own views, have begun to act cathartically by being eliminated from the bowels. To make his experiments at all conclusive, Mr Headland ought to have shown that the portal blood or chyle which he examined contained the article looked for in a soluble but not in an insoluble form. Then his experiments might have gone so far to meet Oesterlen's observations, but till that is done we cannot say that they prove anything.

With respect to that part of Proposition II., therefore, which refers to the different ways in which substances, naturally insoluble in water, may be rendered soluble in the stomach and intestines by acid, alkali, or stomach digestion, we must hold much of the doctrine to be unnecessary and unestablished, till some more satisfactory conclusions are come to, as to the possibility or impossibility of minute particles of insoluble matter passing into the circulation. We may remark, by the way, that Mr Headland makes no special allusion to M. Mialhe's theory that some insoluble matters, such as calomel and the salts of silver, are rendered soluble by alkaline chlorides.

We do not, however, attach the least importance to this doctrine of Mialhe, because it appears from several considerations, and especially from the experiments of Œttinger, that the alkaline chlorides are not present in sufficient quantity to produce the solvent effects which Mialhe ascribes to them; but, when Mr Headland was assigning to pepsine, lactic acid, biliary alkali, and pancreatic juice, their respective merits as solvents of refractory medicines, he might have allowed the chlorides their share of merit in the transaction.

We are quite at one, however, with Mr Headland in that part of his second proposition, where, in discussing the absorption of substances dissolved in water, he expresses himself as opposed to the doctrine laid

down by M. Poisseuille, that saline cathartics operate, merely by producing endosmotically a flow of serum from the vessels, through the coats of the intestine, towards the saline solution lying in the interior of the bowel. We shall allow Mr Headland to state in his own words his reasons and experiments against this doctrine. We are not quite satisfied with his explanation of why a weak solution passes off by the kidneys, while a strong one acts upon the bowels; but we assent to his conclusions in the main, and we cannot avoid expressing our astonishment that a doctrine founded upon such unsatisfactory data as M. Poisseuille's experiments with the endosmometer, should have received so ready an assent as they have done in some high quarters.

"M. Poisseuille found that the serum of the blood would pass through an animal membrane to a solution of Sulphate of Soda of greater specific gravity. He thus concluded that an ordinary saline purgative of greater specific gravity than serum (1·028) acted by causing the passage of this serum outwards into the intestine, producing a watery evacuation. The theory was taken up by Liebig and Matteucci, and by Laveran and Millon. Dr Pereira and Dr Golding Bird have drawn some practical inferences from it.

"It is said that when a saline is absorbed it produces diuresis, and no purging; that when it purges it is not absorbed; that when given to act on the kidneys, the solution should be so diluted as to contain less than five per cent. of the solid; and when administered as a purgative, the specific gravity should be considerably above 1·028. Now I do not deny that a weak solution is more likely to pass off by the kidneys, and a dense one by the bowels; but I consider this due to the fact, that the kidneys are the proper emunctories of water, and of such matters as are soluble in it. But I believe that these solutions are equally absorbed, both heavy and weak, and that the purging or diuretic effect depends mainly on the quantity of the salt, as the kidneys will not eliminate beyond a certain amount of it. I must allege, to support this view of the question, firstly, some arguments *à priori;* and secondly, some experiments which I have made to satisfy myself. To consider first the former. Analogy is certainly against this theory. Other purgatives from the vegetable kingdom produce their effect when injected into the blood, and are yet often powerfully hydragogue. What effect on the process of endosmosis can be exerted by castor or croton oils, or by calcined magnesia? On turning to what we know of the theory of digestive endosmosis, we find that the solid parts of the food are diluted down so as to be absorbed, and that the influence of pressure is to be taken into calculation. It was laid down by the great Boerhaave that a necessary requisite for the formation and absorption of chyle was the contractile force of the viscera. (*Van Swieten's Comment.* vol. i. p. 290.) This, in fact, would press the fluid into the absorbent vessels, even if against the other endosmotic forces. Further, a fundamental principle was insisted on by Dutrochet—that even when the liquids on the two sides varied in density, they would both pass through to each other, though in different proportions. The ultimate result of this would be, that a heavy saline solution would be diluted down to the specific gravity of the serum, and would then pass through. This point I have illustrated in my first experiment. Thus it seems that my view is favoured by these physical laws. But even if it were not so, I should not be inclined to admit that all the endosmotic processes in the living body were regulated by merely physical tendencies. This cannot be the case with some of the secretions. The bile is of higher specific gravity than the serum of blood. Urine in health is lower, but in diabetes often much higher. The production of these secretions, differing in density from the serum of blood, would be inconsistent with the first law of endosmosis. Lastly, we find that in many known cases the theory which I am contesting does

not hold good. Seidlitz-water and sea-water are both known to be purgative. In both the specific gravity is *below* that of the serum of blood.

"The first, according to Bergmann, has a density of 1·006. It contains in each pint 192·8 grains of solid matter. Out of this, 180 grains consist of sulphate of magnesia.

"Sea-water has a specific gravity of 1·026, and its solid contents are about 3·5 per cent. But Dr G. Bird says that less than five per cent. of saline matter constitutes a liquid diuretic.

"These, then, are strong reasons against this idea, considered merely as a theory. But it can readily be put to the proof. With this purpose I have made the following experiments. As far as they are concerned, they seem to show that salines are in all cases absorbed, and that whether they are subsequently excreted through the kidneys or by the bowels depends more on the quantity administered than on the degree of dilution of the dose.

"Exp. 1.—A sufficient quantity of white sugar was dissolved in four ounces of water to raise its specific gravity to 1·028, that of the serum of the blood. Two drachms of sulphate of magnesia were dissolved in another ounce of water, so that the solution measured nine fluid drachms, and had a density of 1·060. This heavier solution was introduced into an open wide tube, closed completely below by a clean piece of bladder. It was introduced into a small vessel containing the solution of sugar, and arranged so that the height of the two liquids should correspond. After three days, the inner solution measured two drachms more, and the specific gravity had sunk to 1.050. The outer solution, after making up exactly the loss by evaporation, was found to have risen in density to 1·040. On adding a small quantity of each of the solutions of phosphate of soda and carbonate of ammonia, a copious precipitate took place, indicating the presence of magnesia. Thus it appeared that the fluids passed both ways, some of the heavy solution of magnesia finding its way through to the lighter solution of sugar. The tendency of this process was evidently to an equalisation of their densities, both by endosmose one way, and by exosmose the other. Thus, apparently, would it be with a saline purgative, and with the serum of the blood.

"Exp. 2.—Three drachms of sulphate of magnesia (a very mild purgative dose) were dissolved in ten ounces of water, and thus administered to a healthy young man. It produced, after some time, slight purging, and some diuresis. The urine, when tested, contained only a very little more than the usual quantity of magnesia. The quantity in the dose was less than five per cent. of the solution, and thus, according to the endosmotic theory, should have produced no purging.

"Exp. 3.—Six drachms of the same salt were given in eighteen ounces of water to the same patient. After a few hours, it caused very copious and long-continued watery purging. The urine did not seem to be increased, and contained no excess of magnesia. It seemed, that in spite of the dilution, the quantity of the salt was so large that it could not pass off by the kidneys, and so was eliminated from the blood by the bowels, in the same way as other purgative medicines. (*Vide* Chap. IV.)

"Exp. 4.—This trial was the reverse of the last. Two scruples of acetate of potash were dissolved in three drachms of water, and thus administered. The solution then contained about twenty per cent. of the salt. According to the endosmotic theory, it should have caused only slight purging, on account of its density. It did not do so, but produced diuresis. The dose was so small that, after absorption, it was easily eliminated by the kidneys.

"These experiments are in favour of my views, and seem, as far as they go, to cast discredit on the theory of Poisseuille. It will be very easy to multiply them, so as to place the fact beyond a doubt."[1]

[1] It is stated by Dr Carpenter, that if a ligature be applied below the pylorus of an animal, and sulphate of magnesia then introduced into the stomach, purging is produced.

And since we have under review this endosmotic theory of the action of cathartics, we may take the opportunity of noticing an allied subject which Mr Headland discusses elsewhere :—

"It has been supposed by M. Posseuille and others, that the action of opium in confining the bowels, is to be attributed to a power of checking the process of endosmosis said to be possessed by a solution of morphia. I shall afterwards state my reasons for discrediting this explanation, (*v. Chap. iv. Art. Opium*)." —P. 86."

We find at p. 335, Mr Headland's views thus given :—

"How, we may ask in the next place, does opium produce constipation? It has none of the characters of a general astringent, and yet there is no astringent like it in its action on the bowels. An attempt has been made to explain it on mechanical grounds. Poisseuille some time ago asserted that a solution of morphia prevented the process of endosmosis through a dead animal membrane. Matteucci and others account for the operation of opium on this principle, supposing that by exerting this action on the living mucous membrane, it is able to prevent the transudation of fluids from the blood into the cavity of the bowel. But this view seems even at first sight unsatisfactory. If such a force is really exerted, and the passage of fluids through a membrane prevented by the action of opium, must it not apply to the stomach as well as to the bowel? Why is morphia absorbed at all? It should stop the digestive process altogether. And why is the secretion of the skin increased by opium, while that of the bowels is suppressed? (*Vide* Matteucci's *Lectures*, p. 74.)

"But there is reason to suppose that a serious error of observation has been made by those who first asserted that a solution of morphia tended to arrest the process of endosmosis. Dr Cogswell has lately made some careful experiments on the subject, and he finds that a solution of muriate of morphia passes readily through the bladder of an endosmometer to serum or a heavy saline solution. And when experiments are made on two other liquids of different density, the addition of morphia to one or other of them is found to effect no alteration in the direction or rapidity of the current.

"Opium is a general paralyser to muscular fibre, both of the voluntary and involuntary kind, but particularly of the latter. And the only reasonable attempt that can be made to explain the action of opium in producing constipation, is by a reference to this its paralysing influence on the coat of the bowel, taken in conjunction with the torpid condition of the general system, and suspension of the animal functions, produced by the secondary action of this narcotic on the nervous forces."

Now, we must say that we do not find that Mr Headland has done much here towards satisfactorily explaining the constipating effects of opium. In the first place, we deny that it has none of the characters of a general astringent. We maintain that it has, and that this property is possessed by its alkaloid morphia, though in a less degree than by the crude drug. It checks the mucous secretions of the bronchi and nose, as well as that of the bowels; but it is a matter of dispute, upon which we do not wish at present to express our opinion,—whether or not it diminishes the excretion of bile and urine; and all observers are at one in allowing, that so far from

This could only be by the absorption of the salt, and its subsequent action from within on the mucous membrane of the bowel below the ligature. This experiment is unauthenticated; but, if verified, would certainly be most conclusive in favour of the view adopted above.

diminishing, it increases the secretion from the skin. It is, we presume, its not diminishing the urine and sweat which induces Mr Headland and others to deny to it the right to be called an astringent. But we must affirm it to be consistent with our observation, that the secretions of sweat and urine are not much under the influence of true astringents at all; we have not data on which to say anything as to the bile. Gallic acid is a most powerful astringent, but though it stops hæmaturia and sometimes checks the passage of albumen into the urine, it does not readily diminish the amount of the urine itself; nay, we have seen it unequivocally act as a diuretic. We have not been able to see any arrestment of colliquative sweats by either tannin or gallic acid; and the effect of the mineral acids in checking this symptom is due to their action as tonics, not as astringents. The sweating induced by opium has appeared to us to be produced in two ways. Small repeated doses act pretty much as alcohol does, by merely increasing the activity of the cutaneous circulation; large narcotic doses bathe the surface in sweat by their relaxing and depressing effect, just as exhaustion from any other cause does. We hold, therefore, that opium, setting aside those effects which flow from its narcotic action, is as much entitled to be said to have astringent properties as any other substance; and that, therefore, this is the 'short and the long' of its constipating effects. Certainly the old doctrine re-promulgated by Mr Headland, that it acts by paralysing contractile fibre, especially unstriped muscle, is, *prima facie*, untenable. For upon this principle, all other narcotics and sedatives ought equally to constipate. Conium we take to be about as powerful a paralyser of unstriped muscle as any article of the materia medica; this is its *modus operandi* as a poison; yet conium certainly does not constipate. We agree with Mr Headland, however, in rejecting M. Poisseuille's endosmotic theory of the constipating action of morphia. Even irrespective of Dr Cogswell's physical observations, we could not conceive from M. Poisseuille's own experiments, that they afford the least explanation of why a minute dose, say 15 or 20 drops of solution of morphia, should diminish intestinal secretion. We apprehend that one of the causes of error against which, in the present experimental age, we require most to guard, is the applying too freely physical laws, deduced from experiments on machines, to phenomena occurring in the living body and involving vital actions.

Mr Headland's Fourth Proposition is, "*That some few remedial agents act locally on the mucous surface, either before absorption or without being absorbed at all. That they are chiefly as follows:*—a, *Irritant emetics;* b, *Stomach anæsthetics;* c, *Irritant cathartics.*" Most people will, we believe, entirely agree with Mr Headland, that certain irritant substances known to act as emetics, such as sulphates of zinc and copper, and mustard, operate in the way he describes. They produce an impression on the nerves of the stomach which is propagated to the nervous centres, and hence reflexly outwards to

the diaphragm and abdominal muscles. Mr Headland does not specifically say on which nerves he believes this impression to be made; but in illustrating a subsequent proposition we find him saying, "that the irritation of the mucous membrane of the stomach, and probably of the filaments of the vagus nerve distributed on it, may cause the contraction of the muscles of the abdomen." We may take this opportunity of stating in illustration of this point, that in some experiments which we made some years ago, we found that section of both nervi vagi deferred the action of sulphates of zinc and copper long beyond the time at which the same doses had operated on the same animals with their nerves uncut. Vomiting, at least in one experiment with sulphate of copper, though deferred, did ultimately occur with the nerves cut. This we have always accounted for by supposing that the emetic ultimately came to act by absorption. Should this view be sustained, it would lead us to differ from Mr Headland as to his explanation of how the non-irritant emetics, or, as he calls them, specific emetics, act. Like him, we believe absorption to be necessary for their action; but, whilst he supposes that they are thus carried to the gastric nerves, and excite through them the reflex actions which constitute vomiting, we are inclined, seeing that emetics can ultimately act though the vagi are cut, to believe that they are carried directly to the nervous centres, probably the medulla oblongata, and at once produce there the impression which calls the abdominal muscles into play. We have, however, an intense hatred at vivisections, and never extended our experiments to such substances as tartar emetic and ipecacuan, which would be requisite to enable us to come to any decided opinions as to how far the vagi are concerned in the action of emetics.

With respect to irritant cathartics, we reserve our remarks till we come to examine Mr Headland's chapter on eliminatives, when the action both of irritant and non-irritant cathartics will come under review.

The Fifth Proposition need not detain us long. "*That the medicine, when in the blood, must permeate the mass of the circulation so far as may be required to reach the parts on which it tends to act. That there are two possible exceptions to this rule:* a, *the production of sensation or pain at a distant part;* b, *the production of muscular contraction at a distant part.*" Mr Headland here merely states, that what he conveniently calls "the rule of local access" must in general hold good before a medicine introduced into the stomach can act on a distant organ; but that such effects as are due to reflex nervous action, of which the irritant emetics are examples, are exceptions to this rule. The production of pain reflexly, however, is more easily exemplified in pathology than in therapeutics, as our author himself admits.

"Certain impressions on the surface of the stomach may cause such a reflex pain. Swallowing a piece of ice will sometimes produce pain over the brow; and it is likely that the headache which follows over-eating, or a large dose of a

tonic medicine, may result in a similar way from mere irritation of the stomach. Any irritant, as a solution of Iron, Arsenic, or Zinc, will do the same. The action of a violent purgative will cause headache while it lasts. Although we cannot deny the possibility of other actions of the same kind, yet there are so few medicines which, in ordinary and safe doses, are capable of producing pain in any way, that it becomes difficult or impossible to adduce a satisfactory example of an agent which operates on distant parts in this manner when in the blood."—P. 89.

Proposition Sixth states, "*That while in the blood the medicine may undergo change, which in some cases may, in others may not, affect its influence,—that these changes may be,* a, *of combination;* b, *of reconstruction;* c, *of decomposition.*" As examples of which we have—*a*, Free acids taken into the stomach appearing in the urine combined with bases. *b*, Tannic acid reconstructed in the system into, and appearing in the excretions as, gallic acid; and oil of turpentine reconstructed into a matter in the urine which smells like violets. *c*, The alkaline salts of the vegetable acids, such as acetate of potash, being decomposed in the system into carbonates.

In this section, under the head of Changes of Combination, we encounter one of the most curious of the unsolved problems in therapeutics, viz., as to how far the effects of acids are explicable upon chemical grounds; but as this is more fully discussed in the chapter which specially treats of acids, we need not comment upon it here.

We have thus, in Mr Headland's company, traced his medicines from the stomach till we have got them fairly disposed of in the blood. It now remains for us to see what they do when they have got there. We are warned, however, by our printer, that we will occupy a larger amount of ground than therapeutics are entitled to, if we proceed farther at present; but, believing as we do in the importance, not of our remarks, but of the subject to which they refer, we request permission of our readers to resume the examination of Mr Headland's doctrines in a future Number. Meanwhile we again commend the book for perusal.

(*To be continued.*)

A Treatise on Operative Ophthalmic Surgery. By H. HAYNES WALTON, F.R.C.S.E., Surgeon to the Central London Ophthalmic Hospital, and Assistant-Surgeon to St Mary's Hospital. Churchill. London. 1853. Pp. 619. 8vo.

THE handsome and copiously illustrated volume before us constitutes, as we are informed in the preface, the first fruits of the author's observation and practical experience, and we beg to congratulate him on the success of his literary début. The subject is one on which a good practical treatise has of late years been a desideratum;

for in no department of operative surgery is precise and accurate information so much required, as regards manual procedure, choice of instruments, the selection of different modes of operating, and the results of operative interference, as in the surgery of the eye; and we do not feel that we cast any imputation on the standard and practical works of Travers, Lawrence, Wharton Jones, on the admirable treatise of Dr Mackenzie, or the beautifully illustrated work of Mr Dalrymple, in stating, that in the operative department of the subject, a good practical treatise is a useful addition to the surgical literature of our language.

The first chapter of Mr Walton's book consists of a history of ophthalmic surgery, which, in addition to its being tedious and uninteresting, is, we think, out of place in a work in which we seek instruction in the practice of the present day, more than of the times of Pythagoras, or of Avenzoar and Averroes. We hope to find in a future edition, that Chapter I. has been removed as a useless addition to the bulk of the volume.

The second chapter is devoted to the use of chloroform, which we think also a little superfluous. The only special information which we gain from it is, that,

"Except in the operation for the extraction of cataract, or the division of the cornea to a like, or nearly equal, extent, for any other purpose, there is no operation on the eye in which well-founded objections exist to the previous use of chloroform or ether as anæsthetic agents. In the exceptions mentioned, the objection arises from the fear of vomiting, and the loss of the vitreous humour."

A short chapter on ophthalmic instruments, in general, contains some useful practical hints. The instruments in this chapter, and throughout the volume, are accurately delineated. If it is true that an operator may be judged by the instruments which he uses, a favourable opinion will be entertained of the author's capabilities, his instruments being light, elegant, and of simple construction. The following remarks may appear trivial, but attention to such minutiæ is not unimportant.

"To small instruments, such as cataract knives, cataract needles, and the like, I prefer round handles, for when of this form they can be held with more ease and freedom, while stiffness and constraint are overcome, and a more individual independent control is thus given to the fingers. I am also of opinion, that the handles of such should be smooth, and not cross cut. . . . . I believe it to be true, and if so, it is a fact of much significance, that there is no other manual art in which rough handled instruments are used."

In treating of mechanical injuries of the eye, the subject of inflammation of the eye-ball is considered, a term which the author uses in the wide signification given to it by Dr Jacob, in his valuable papers in the "Dublin Medical Press." He points out the error, which is very general, of attempting too far to describe inflammation as isolated or confined to particular structures; and subscribes to the opinion of Dr

Jacob, that a discoloured iris and a red sclerotica are not evidences of inflammation of the iris alone, but of the whole eye-ball.

"We find in practice that these distinctions vanish, and we discover only a progressive inflammation of the whole organ, though it may have been in the commencement more conspicuous in some particular tissue. I believe that the only exception to this rule will be found in acute idiopathic inflammation of the retina, a disease of very rare occurrence."

On the subject of the treatment of inflammation of the eye-ball, the following remarks on blood-letting occur, to which we fully subscribe.

. . . . . "It is especially indicated when there is rapidly declining vision, together with congestion or effusion in the posterior part of the eye, which is rendered probable by the bulging of the lens and iris towards the cornea, even though there should not be much external evidence of inflammation. . . . . No precise rule can be given as to whether the bleeding should be general or local. The practitioner must be guided by the individual peculiarities of each case. The necessity of moderation in the abstraction of blood cannot be too energetically urged. The quantity should be regulated by the local symptoms, and the age and general condition of the patient; otherwise, destructive rather than conservative effects may result. Disease cannot be bled out of the eye, as some surgeons seem to think. At the same time, when bleeding is indicated, a decided effect must be obtained. It is seldom that a few leeches will suffice, except during the earlier periods of life. Nevertheless, we find in routine practice, that few inflamed eyes escape the application of two or three leeches. In the robust such practice is insignificant, while in the debilitated it is worse than unnecessary."

The necessity of attending, in traumatic as well as idiopathic inflammation of the eye, to the existing diathesis in each individual case, is urged; and the author confirms the principle laid down by Dr Jacob, that traumatic inflammation will assume the rheumatic, scrofulous, or even syphilitic character in patients who are suffering from these affections.

We are glad to find that, whilst due weight is given to the influence of mercury in controlling inflammation of the eyeball, the prevalent abuse of the drug is strongly reprobated.

"I regard salivation as the poisonous influence of mercury; and I believe that the highest curative effect is to be obtained short of this state. At the same time, if a disease does not yield, and its violence be not lessening under its use, I should give the mercury till the mouth was in some degree affected, to ensure that the system was fully under its influence. . . . . It must not be lauded as a specific; for disease may continue to increase, even although the system be fully under its influence. Thus an eye, which has heretofore been healthy, may, while a patient is under the influence of mercury, become diseased. This I have frequently witnessed in syphilitic disorders. Among the instances, a case occurred in one of Mr Lawrence's syphilitic wards, at St Bartholomew's, when I was dresser. The woman was salivated for syphilis, and iritis supervened; cupping and leeching were tried, but in vain, and the eye was lost."

In speaking of dislocation of the lens from mechanical injury, the author lays down the following rules as the result of his experience.

"In a subject more than forty years of age, when the lens has increased in density, and its removal by quick absorption cannot occur, I should perform the operation of extraction or depression, according to the rules given for this special occurrence in the chapter on cataract. Between the age of puberty and thirty I should not interfere, if the lens were in the anterior chamber without its capsule, and lying harmless; but where accompanied with irritation and pain, I should extract or recline it, according to circumstances. Under puberty I should trust entirely to nature. I saw a case of dislocation of the lens into the anterior chamber of a child three months old, from a fall. Absorption was accomplished without the least disturbance in the eye."

Chapter V. contains some excellent remarks on the subject of foreign bodies on the surface of the eye, and within the eye-ball. The practice, which is too prevalent, of leaving a minute foreign body imbedded in the surface of the cornea, when a first or second attempt to remove it has proved unsuccessful, is justly reprobated. Many eyes have been lost by the principle of non-interference under such circumstances having been too strongly inculcated. It is true that there is a possibility of the particle becoming encysted, or of its being discharged by the ulcerative process; and that the resulting opacity of the cornea may not be so great as materially to impair the power of vision. But such a result is a rare exception to the general rule. We are glad to find our author making the statement, that, after ample opportunities in practice, he has never regretted his persevering attempts to remove a foreign body from the cornea. The dislodgement should not be effected by a cataract needle, or other sharp-pointed instrument, by which the steadiest hand will sometimes inflict additional injury on the cornea. We have found a silver toothpick answer the purpose efficiently. Mr Walton recommends the use of a small scoop or miniature gouge (figured at p. 99), which he has found more useful than any other instrument for the performance of this sometimes troublesome and delicate operation.

The imperfect results obtained by operations for the relief of synblepharon and anchylo-blepharon, are confirmed by the author's experience, and should be fully borne in mind in the treatment of those affections of the eyelids, in which such an unfortunate occurrence as the adhesion of the lids to the globe or to each other is apt to ensue. In the former affection, operative interference appears to be fruitless, unless the connecting medium consists of long and comparatively isolated bands. In the latter, the after contraction, which tends to re-unite the lids, after their separation by the knife, though more under control than that which re-unites the globe and lids under similar circumstances, is still sufficient to present serious obstacles to the success of such operations.

"We must not operate in this affection (anchylo-blepharon) when the lids adhere to the globe, a complication easily ascertained by the movements of the organ, or by any aperture that may exist; neither must we do so when the cornea is opaque, a point less easily determined, unless it can be seen; here, however, the power of discerning light must be made the test—where one eye is perfect

it should be closed, and a comparison of the light-detecting power of the two, may decide the question."

The subject of Trichiasis is one of importance, and as such it is duly acknowledged and treated in the chapter on the affections of the eyelids. The irritation of the inverted lashes, though it does not rapidly destroy the power of vision, ultimately proves destructive to the transparency of the cornea, unless efficient means are taken to remove the source of irritation. The number of eyes which are destroyed by this cause, especially among the poor, is very considerable. This probably arises from the impairment of vision being a gradually increasing process, and from the practice of palliating the affection by plucking out the misdirected cilia being too long persevered in, whilst the opacity of the cornea is on the increase. The remarks of the author on this subject are well worthy of attention :—

"Of a means so generally adopted (evulsion of the cilia by forceps) and apparently so universally applicable, it is well to ascertain in what species of implication of the cilia it is advisable to be practised, and how long in any instance it may be persevered in. The condition of the cilia that irritate, and the state of the lid, on which they are seated, should be taken into consideration—when the irritation is occasioned by a few well-formed but inverted cilia proceeding from a healthy lid, there cannot be a doubt about the propriety of using the cilia-forceps, because there may not be a return of the inversion, and even if the hairs so treated on their reproduction show a tendency to become distorted to a like degree, their injurious effect can be anticipated, and the process of extraction repeated. It then becomes a mere matter of consideration to the patient, whether he shall submit to the periodic repetition of the process, or to a rather more severe but effectual plan, resulting in a cure. But when the extraction is followed by an increase of the number of inverted cilia, or by abortive ones, the application of the cilia-forceps has reached its limit, and, as a rule, should be discontinued. There cannot be a doubt that the continued removal of the cilia by force is very frequently hurtful to their follicles, renders the direction of them more perverse, causes abortive productions, tends to involve the contiguous follicles, and so to multiply the trichiasis, and should not be persevered in, except at the particular request of the patient. So, also, where the lid is unhealthy, and the offending cilia, although few in number, are abortive and grow directly inwards, extraction is contra-indicated; some effectual treatment only is admissible. For in the majority of such cases, with all the vigilance of both patient and surgeon, and weekly extraction practised, there will be an impossibility of keeping the globe of the eye exempt from their contact. The majority of such fine cilia, are not in reality plucked out, but are broken off, and with any accession of growth, again exert their injurious effect, long before the eye recovers from the last irritation. Notwithstanding the gradual deterioration of the state of the eye under this palliative treatment, patients will still desire a continuance of it; and the careful use of the forceps will greatly prolong the arrival of its worst effects, while their careless application will hasten it."

For the mode of operating so as to effect a radical cure of this affection, we must refer the reader to Mr Walton's description of his operation. The scalpel is preferred to the scissors as ensuring greater accuracy and a less conspicuous scar.

On the subject of idiopathic entropium, the author's observations demand much more extensive consideration than the limits of our

space will allow. He attributes all forms of the affection to one and the same cause, viz., muscular action, but differs from those, who have supported this view, as to the part of the muscular apparatus of the eyelids, on which the inversion depends. He believes, from his dissections, that the relation of the tensor tarsi muscle to the orbicularis is not accurately described by modern anatomists, that it has an extensive connection with that muscle, and may justly be considered a part of it—that part which has a bony attachment.

"If I may be allowed to speak of it as a distinct muscle, I should say it arises as a thin plane of well-marked muscular fibres, of about the fourth of an inch in breadth, from the vertical ridge on the lacrymal bone, passes forwards and outwards towards the lacrymal sac, and bifurcates in a remarkably distinct manner, the branches proceeding towards the lids, and on their edges become blended with the orbicularis muscle. They can be traced to the middle of the lid. In their passage their branches distribute fibres to the posterior surfaces of the puncta, and some few would seem to surround them. The best manner of tracing the muscle is to remove the skin from the orbicularis, then to detach the upper and outer walls of the orbit, divide the optic nerve and the muscles around, and draw the back of the eye outwards and forwards."

It is on the perverted action of these muscular fibres that the entropium is believed to depend, and conjunctival inflammation is regarded as the probable exciting cause of the affection. With regard to treatment,

"I have founded the treatment on what appears to me to be the pathological interpretation of the affection, and of which the indications are, to overcome the means of the inversion by dissecting away the thick marginal portion of the orbicularis, supposing that part of the muscle to be entirely, or nearly all that is at fault; and also to remove as much of the skin of the lid as may be necessary to produce such tension, as shall overcome the deformity which other tissues of the lid may have acquired from the irregular position, into which they have been thrown by the muscle, and which has been made more or less permanent by the changes induced by inflammation."

The steps of the operation are accurately described, and illustrations are introduced showing the successful results of the mode of procedure recommended by the author. A view of a dissection of the muscles within the orbit is also given, illustrating the muscular peculiarity to which he specially refers.

Some useful observations occur in the chapter on Strabismus, to which, however, we can only refer very briefly. It is asserted, and we believe with truth, that double convergent squint is of rare occurrence. Superficial examination often leads us to believe it to be of common occurrence, when a more careful examination will point out the source of fallacy. The deceptive appearance is explained by the author to be produced through the influence of the associated movements of the lateral recti.

"For instance, the right eye squints; to look to the right with that eye, a much greater amount of volition is needed, and more muscular power called into action, than if it were straight; and that extra exercise of power is, by the associated movements of the eyes, unavoidably transferred or directed to the associate

muscles of the other eye, the left, and especially to its internal rectus, overmatching the antagonising muscles, and turning it unduly inwards. This must hold good whether the squint arises from a defect of power in the external rectus, or an excess of it in the internal muscle. To avoid intricacy and minute detail, the lateral recti alone are here taken into consideration, although all the orbital muscles are more or less involved. When the squint would seem to be completely transferred from one eye to the other, I believe that the appearance is due to the exaggerated associated movement; and if this be correct, it fully explains the fact, that a single operation, the division of the internal rectus of one eye, will restore to parallelism eyes, in which the obliquity has appeared to alternate, while it also accounts for the common error, that one operation may rectify two squinting eyes."

To determine which is the squinting eye, and, consequently, which internal rectus muscle requires division, is occasionally a rather perplexing matter, and we have on more than one occasion seen the operation performed on the wrong eye. The author's plan is thus described:—

"I place the patient in front of me, at the distance of two or three yards, and direct him to cover one eye, say the left, and look at me with the other, keeping the head straight—the right eye will be in the centre of the orbit. I then direct him to uncover the left; now, if the right, which has not been closed, is normal, it will keep its central position, while the left is turned inwards; but if it be deformed, it will turn in, while the left will become straight. The experiment should be reversed."

In cases where there is any difficulty in determining the point, this mode should be repeated two or three times at intervals, and additional information may be obtained by looking at the position of the eyes, when the patient is off his guard, and the orbital muscles are more in their usually quiescent state than when he is under the examination of the surgeon. Additional information will be obtained by comparing the power of abduction of the two eyes. A test, too, which will be rarely found to fail, is the comparative power of vision of the two eyes. The weaker eye is the squinting eye; the weakness of vision having possibly been the original cause, but probably more frequently the effect of the convergence.

We do not much admire the mode of performing the operation (division of the rectus) described by the author. The conjunctiva is divided with the scissors vertically, to an extent *just sufficient to admit the blunt hook, and afterwards one point of the scissors.* The hook is then introduced under the muscle, and its tendinous expansion divided along with the conjunctiva that covers it. It is surely better at once to divide the conjunctiva and cellular tissue freely with one stroke of the scissors. The muscle is thus exposed to view, and its entire breadth more certainly raised by the hook, or between the blades of the forceps, and with less risk of violence than through the narrow opening in the conjunctival covering. The author objects to the use of the small sharp-pointed double hook, an instrument from which we have derived great assistance in the performance of the operation, and which, we think, if used with ordinary care, cannot be productive of injury.

Such are a few of the subjects treated of by the author, and we must content ourselves by merely referring to some of the more valuable remarks on subjects, which are, perhaps, of greater importance than those to which we have adverted.

The subject of cataract is ably treated; and in the chapter on this disease, we feel sure that the author's laudable attempt to simplify the subject will be gratefully appreciated by those who have attempted to follow out the complicated subdivisions to which cataract has been subjected by some former writers. We would refer to the remarks, in this chapter, on the diagnosis of cataract, amaurosis, and glaucoma, as of especial importance, and to the observations on the value of the catoptrical or candle test of Sanson—a means of diagnosis which we do not think is generally well understood.

The subject of malignant diseases of the eye is treated fully and, we think, with great judgment.

In the chapter on the formation of artificial pupil, the author advocates the plan of performing all such operations through the cornea. At p. 594, a sketch of two artificial pupils is given, which were formed by a mode of operation proposed and executed some years ago by the author, and which he strongly recommends. The operation he denominates, "incision with extension." The mode of performing this and the other operations, which are usually followed, are described with clearness and accuracy.

The subject of Mr Walton's treatise is one which demands more extensive consideration than we have been able to bestow upon it. Enough, however, has been said to show the scope of the work. It evinces much careful literary research, and bears the stamp of being the production of an observant surgeon; but its chief value in our eyes consists in its being a sound *practical* work—a character which the author has successfully endeavoured to bestow upon it in accordance with its title.

In conclusion, we have to congratulate the author on the excellent manner in which his publisher has fulfilled his portion of the undertaking. The type and paper are unexceptionable, and the excellence of the numerous woodcuts, with which the volume has been illustrated by Messrs Bagg, has not hitherto been surpassed.

Part Third.

CLINICAL REPORTS, LECTURES, ETC.

CLINICAL MEDICINE.—PROFESSOR BENNETT.

FUNCTIONAL AND ORGANIC DISEASES OF THE STOMACH.

CASE I.—*Dyspepsia.*

James Scott, æt. 51, admitted 27th September 1852. He states that, about two months previous to admission, he experienced severe shooting pains darting from the left scapula to the epigastrium and left hypochondrium. For many years back he has been much addicted to intemperate habits, and latterly the appetite has been considerably impaired. On admission, the tongue is furred, and cracked in the centre; he has almost constant sour taste in the mouth, worse in the morning after taking food; frequent acid eructations; bad appetite, and considerable thirst. About a quarter of an hour after meals he experiences a feeling of heat and pain in the epigastrium, with acid eructations and flatulence; the latter also troubles him during the night, when the stomach is empty. These symptoms continue generally for about an hour and a half, when they gradually abate, and soon after disappear entirely. He then again takes food, and the symptoms return in about a quarter of an hour afterwards, as already noticed. He does not think that one kind of food disagrees with him more than another. He has often much nausea and loathing of food, but no vomiting. There is some tenderness on pressure at a point about the centre of the epigastrium, where he states there is always more or less pain, generally of a dull, heavy character, but sometimes occurring in sharp twinges, shooting to the left scapula, and somewhat increased on pressure. There is no unusual hardness or tumour to be felt; and there is no dulness on percussion. There is no tenderness or enlargement of the liver; urine normal. He is of a very desponding disposition, and does not sleep well at night. Other functions normal. —℞. *Potassæ carbonatis*, ʒij.; *Tinct. gentian. co.*, ʒi.; *Infus. gentian. co.*, ʒv. *M. ft. Mist. Half a wine-glassful to be taken thrice a-day.*—Dec. 31. Still complains of flatulence and distension of the abdomen; considerable pain in the epigastrium, increased on pressure.—*Applicentur hirudines*, iv., *epigastrio, et postea, foveatur.*—January 3. Appetite improved; still acid eructations, with sour taste in the mouth; pain in the epigastrium, relieved after the application of the leeches and warm fomentations. He is very desponding about his complaints, which he much exaggerates.—January 10. The sour taste and flatulence diminished; pain and uneasiness in the stomach much relieved; no tenderness on pressure; appetite much improved; no sickness or vomiting; bowels regular; stools natural. Dismissed, in order to return to his work. The diet ordered has been of a gentle, unstimulating, but nutritious kind.

Commentary.—In this case derangement of digestion depended on intemperate habits, and was accompanied by excess of acidity in the stomach. The treatment was directed to counteract this condition by alkalies, vegetable bitters, and a regulated diet, which, to a certain extent, succeeded. But all such cases require exercise, regular habits, and moral control, without which medical treatment is unavailing.

CASE II.—*Dyspepsia—Oxaluria.*

John Millar, æt. 28, a type-founder, admitted December 26th, 1852. He states that he has always enjoyed good health, with the exception of occasional palpitation of the heart, until about eight months ago, when vertigo came on suddenly when at work, but disappeared in a few minutes. Since then, he has had many attacks of the same kind; and of late, these have been accompanied with pain and palpitation of the heart, and *tinnitus aurium*. Some years ago he was much addicted to drink, but for the last four years has been more temperate. On admission, the heart was found to be healthy, and the pulse natural. The tongue was dry in the centre, moist and white at the edges, with numerous transverse fissures. He had a disagreeable taste in his mouth in the morning, and no appetite for food; had never vomited, nor experienced pain in the stomach; bowels constipated. There was an anxious, haggard expression of countenance, and an evident tendency to exaggerate his symptoms; he complained of vertigo, tinnitus aurium, and muscæ volitantes. The urine, after standing some time, exhibited a slight deposit, in which numerous large crystals of oxalate of lime were visible on microscopic examination; sp. gr. 1028; otherwise normal. The other functions were normal.—℞. *Acid. nit.; Acid. muriat. ā. ā.* ℥iss.; *Tinct. gent. co.*, ʒi.; *Infus. gent. co.* ℥v. *M. A table-spoonful to be taken three times a-day.*—January 8. Since last report, the oxalates have disappeared, the appetite has improved, the cardiac and cerebral symptoms are removed, and he was dismissed cured.

Commentary.—Dr Golding Bird was the first to point out that oxaluria, associated with dyspepsia, was a very common disorder, and that its treatment by nitro-muriatic acid was the most successful one. The oxalic acid is probably derived from urea or uric acid, and its presence in the urine is often associated with or alternates with these compounds. No doubt the tonic treatment practised in the above case is the best mode of relief, but here also a regulated diet, with exercise and mental occupation, are necessary to render permanent any benefit that may be derived from treatment.

CASE III.—*Dyspepsia—Hypochondriasis—Oxaluria.*

Thomas Pollock, hawker, æt. 24. Admitted 25th December 1852. He says that, three years and a half ago, when stooping down in a field during a dark night to evacuate his bowels, he felt a sharp, hard body, like the stump of a shrub, penetrate his anus, causing acute pain, which continued for a fortnight, and has occasionally returned ever since. No blood passed at the time, but he has been under the care of various medical practitioners, and undergone numerous kinds of treatment. He has never had diarrhœa; but is addicted to masturbation. He has consulted the numerous works advertised in the papers on manly vigour, etc., but has derived no benefit from them. On admission, tongue moist, but furred, cracked, and fissured in the centre; says he experiences a feeling of load after taking food, with occasional nausea. He has no vomiting, but an acid and sometimes a disagreeable taste in the mouth; frequent flatulence and constipation, for which he is in the habit of taking aperient medicine. On placing the hand on the epigastrium, he says that there is soreness beneath the xiphoid cartilage, increased on pressure. Has occasional involuntary emissions of semen. The urine contains a slight sediment on standing, which is crowded with large and small crystals of oxalate of lime; sp. gr. 1020°; otherwise normal. Sleepless at night; anxious and desponding about his complaints, which he attributes to the accident formerly mentioned, although it produced no local effects at the time, nor any structural change since. Says that he has frequent vertigo, tinnitus aurium, muscæ volitantes, and cephalalgia. The other functions are normal.—℞. *Acid. nitrici; Acid. muriat. ā. ā.*, ʒi.; *Tinct. gent. co.* ʒi.; *Infus. gentian co.*, ℥v. *M. A table-spoonful three times a-day.*—January 3. He has continued to take the acid mixture, but does not admit that he is in any way better. On the 2d, the oxalates

disappeared from the urine, and were replaced by a copious deposition of amorphous lithates.—*Omitantur mist. acid.*—℞. *Liquoris potassæ*, ʒij.; *Tinct. cardamom. co.*, ʒi.; *Infus. quassiæ*, ʒvii. *M. Two table-spoonfuls night and morning.* —January 4. As he still continues to complain of pain in the sacral region, which he attributes to the accident, *a blister, three inches by four, was ordered to be applied there.*—January 10. Since the application of the blister, the pain in the sacrum has disappeared. He expresses himself as being much better, and was dismissed.

Commentary.—In this case the presence of oxalates in the urine were associated with the same class of symptoms as in the former one, but the tendency to exaggerate his complaints was more marked, and there was a firm belief in their being caused by an accident, which possibly never happened; and if it had, could not have occasioned them. The acid and tonic mixture removed the oxalates, but lithates took their place in the urine, which, in their turn, were got rid of by alkalies. Still, the fixed idea as to the cause of the disease continuing, he seemed no better. A blister was now applied to the sacrum, when he readily adopted the idea that his local complaints disappeared with the pain of the blister, and he became cheerful and well. No case could better illustrate the effects of mental depression on the digestive organs than this. For a period of three years he had been the subject of delusion and genital irritations, which had been kept up by the study of those wretched publications, which, to the disgrace of the newspaper press, are daily advertised to the people as the only means of restoring vigour to the constitution. At length, satisfied of their inefficiency, he entered the Infirmary; the error of his practices was kindly pointed out to him, nutritious diet, regular habits, and tonic treatment were obviously beneficial; and fortunately his hypochondriasis yielded to the simple expedient of substituting real for supposed pain, and leading him to suppose that the one had cured the other.

General Pathology and Treatment of Dyspepsia.

By Dyspepsia (from δυσπεπτω, to digest with difficulty) is generally understood, all those functional derangements of the stomach which are primary in their origin, that is, not dependent upon, or symptomatic of, inflammation or other disease in the economy. Such a disordered condition is exceedingly common, and often constitutes the despair of the physician, arising, as it frequently does, from causes which are often obscure, or, if discovered, are beyond his control. This will become apparent by considering, in the first place, those circumstances which require to be united to secure a healthy digestion. These are,—1st, A proper quantity and quality of the ingesta. 2d, Sufficient mastication and insalivation. 3d, Active contractility in the muscular coat of the stomach. 4th, Proper quantity and quality of the gastric, biliary, and pancreatic fluids. 5th, A consecutive and harmonious action of the intestinal canal. Dyspepsia, or indigestion, may be produced by any cause which occasions derangement of one or more of these conditions; and hence why so many different circumstances may produce somewhat similar symptoms, and why so many different remedies have been found effectual in various cases. Notwithstanding that you will frequently meet with instances which baffle all preconceived rules, there can be no doubt that a careful attention to the essential physiological conditions above enumerated will, in the great majority of cases, conduct you to a successful rational treatment. Thus,—

1. Of all the causes of dyspepsia, excesses in eating and drinking are the most common. An over-distended stomach, or too rich a meal, not unfrequently induces a feeling of weight or fulness in the epigastrium, nausea and eructation of acid, bilious or gaseous matters, with a loaded tongue, headache, and other general symptoms. This is acute dyspepsia, or the *embarras gastrique* of the French. Occasionally, there is more or less vomiting of bilious matter, when the attack is vulgarly called a *bilious seizure*. If called into such a case, immediately on its occurrence, and before the ingesta have left the

stomach, as determined by the sense of load at the epigastrium, an emetic should be given; but if vomiting be present, it should be assisted by warm diluents. As soon as the stomach is quieted, or, if you have been called in at a late period, when the ingesta have passed into the intestines, a purgative pill should be administered, consisting of four grains of calomel, with four of compound extract of colocynth, followed in a few hours by a purgative draught of salts and senna. If necessary also an enema may be given. The purging, with a day or two's confinement to farinaceous food, will generally get rid of such an attack; but their frequent repetition leads to the chronic form of dyspepsia, when careful regulation of the diet with exercise, must constitute the chief treatment. Hence the advantage of what is called "change of air," and much of the benefit which is derived from watering places. Chronic dyspepsia, however, is far more commonly caused by excess of spirituous and vinous drinks, than by eating, when abandonment of the evil habit is a *sine qua non* in the treatment. Tea drinkers are very liable to the disease, and its frequency among female servants is probably owing to this cause.

2. It may frequently be noticed, that those who have acquired the habit of eating rapidly are more or less dyspeptic. I knew a journeyman printer who was much tormented with indigestion, but who was cured by changing his residence. The cause of this was for some time a mystery, but on again changing his house the disease returned, although no apparent cause could be discovered. I ascertained, however, that it depended not on the locality *per se*, but on its distance from the printing house. When far off he eat his dinner with his family rapidly, having only just time enough to walk home and back within the hour. When he lived near, the time otherwise spent in walking was occupied in eating, or in cheerful converse with his wife and family. Since I made this observation, it has often occurred to me that the distant residences of artizans from their place of employment may be the occasional cause of the dyspeptic symptoms they frequently possess. With regard to the exact object of the saliva in the process of digestion, whether it be to convert the farinaceous compounds of the food into glucose, or by its viscidity, to mix up air with the portions swallowed, is not positively determined; but its necessity for the digestion of man is shown by cases where the under lip has been lost by accident or disease, and where salivary fistulæ has formed, in which dyspepsia is generally present, and in which the disordered digestion has been cured by operations that, by restoring the parts to their normal condition, prevent the escape of saliva. Again, persons habituated to the dirty habit of spitting, are for the most part dyspeptic; and it has been asserted that the pale countenances of the inhabitants of the United States and the leanness of their persons are owing to this cause. In all cases where dyspepsia can be traced to this source, the treatment must be obvious.

3. The contractile movements of the stomach which, by kneading the ingesta, and keeping them in constant motion, secures an intimate admixture with the gastric juice and the rapid transference to the duodenum of such portions of it as are transformed into chyme, are evidently of immense importance to a proper performance of digestion. The experiments of physiologists have shown that digestion in gastric juice out of the stomach is much slower than in it, and that section of the pneumogastric nerves, by arresting the contractile movements, only permits the circumference of the mass in contact with the secreting surface to be digested. These facts at once explain the well known influence of mental emotions upon the stomach. Contentment and hope are as favourable, as dissatisfaction and despondency are opposed to, good digestion. Nothing is more common than dyspepsia among literary men who overtask the mental faculties; among young persons of very excitable minds; and among individuals of a melancholy temperament, hypocondriacs, etc., etc. It is in such cases that cheerful society, active and appropriate occupations, change of scene, removal from mercantile or literary employments, different trains of thought, and so on, are beneficial. Hence also many of the good effects of travel, visiting watering places, etc., etc.

4. Our knowledge with regard to the offices performed by the gastric, biliary

and pancreatic juices in digestion has of late years been much advanced. Thus the gastric juice more especially operates on the albuminous, and the pancreatic juice on the fatty compounds of the food. The function of the bile is perhaps more obscure, although it probably acts as a means of precipitating or separating some of the excretory matters from chyme, and so facilitates assimilation of the nutritive portions. Digestion may be deranged by all those causes which too much increase or diminish the secretion of these three fluids. Thus excess of acidity in the stomach is one of the most common causes of dyspepsia, producing that form of it which accompanies scrofulous and tubercular diseases. It may be in such excess as to neutralise the alkaline action of the pancreatic juice, and render it difficult or impossible to emulsionise fatty matters. In such cases, alkalies, with bitter tonics and the direct introduction of animal oils in excess, are indicated. On the other hand, the gastric juice may be diminished in quantity, as frequently occurs in persons who suddenly overtask the powers of the stomach at feasts, or in old persons with feeble digestion. The sense of load after eating is generally indicative of slow digestion from this cause. In acute cases, a stimulant rouses the stomach to increased action, and hence the moderate use of drams and generous wines after dinner is occasionally useful. In old persons the sense of load and feebleness is best removed by giving up tea, and drinking at night a little weak brandy and water. In chronic cases, acids are indicated, especially muriatic acid. *The Tr. Ferri. co.* of the pharmacopœia is a useful preparation in chlorotic females. We have no distinct means, as far as I am aware, of rousing the pancreas into action, and yet many cases are on record in which fatty matters have passed undigested through the alimentary canal in consequence of obstruction to the pancreatic duct. In such cases, and all those in which fatty matters are difficult to digest, alkalies, especially the liquor potassæ with vegetable tonics, are indicated. When the bile is deficient, constipation and dyspepsia are usual results, and are to be relieved by gentle mercurial purgatives, with extract of taraxacum, and by remedies, such as rhubarb and especially the compound rhubarb pill, which, by acting on the duodenum, also favour the flow of bile into the upper part of the alimentary canal. Dr Clay of Manchester has recommended in such cases the administration of ox-gall, a remedy, which, although not extensively given, is evidently rational, and calculated by its purgative action to be highly serviceable. Excess of bile, on the other hand, ought to be treated by drastic purgatives, diuretics and diaphoretics, according to circumstances, to cause excess of excretion. Exercise should also be insisted on to call the lungs into action, and thus relieve the liver in its office of separating hydro-carbon.

5. A derangement of the consecutive and harmonious action of the alimentary canal is another frequent cause of dyspepsia, for it is as necessary that those portions of the food which are not assimilable should be removed out of the economy, as that the nutritive materials should be absorbed. Hence whatever impedes the contractility of the intestinal canal, whatever alters the structure of its mucous membrane, or whatever mechanically obstructs its calibre, may always be observed to induce dyspeptic symptoms. The removal of these various conditions, whether by stimulating the nervous centres, by appropriate diet, or by purgatives and astringents, as they may be required, need not be more particularly dwelt upon.

In many cases of dyspepsia, two or more of these classes of causes may be combined, so as to render the indications for treatment complex and apparently contradictory. In other cases, one or more causes may exist, although from the indications presented they cannot be determined, when our treatment must always be more or less vague and unsatisfactory. Lastly, there are a few instances where dyspepsia can only be explained by *idosyncrasy*, in which we find this or that particular article of diet to derange the digestive functions, and in which avoidance of the offending cause is the only plan that is attended with success.

In addition to the different kinds of dyspepsias to which I have directed your attention, it is practically important to keep in rememberance the leading symptoms which may be present, and the remedies by which they may be removed. These are anorexia, acid eructations, sense of load in the stomach, cardialgia, vomiting, flatulence, palpitations of the heart, and cephalalgia. Some persons talk of a stomach cough, which, however, is more commonly dependent on irritations in the œsophagus or pharynx, which have hitherto been overlooked. I have already alluded to the mode of treating most of these symptoms. Palpitations of the heart often occasion alarm in young dyspeptic persons, and in addition to remedies directed towards the stomach, change of scene, removing attention from the affected organ, and varied reading should be enjoined. The sense of load in the stomach is most frequently removed, as we have previously said, by acids, whilst acid eructations and cardialgia are best relieved by alkalies and bitter tonics. Vomiting and flatulence are often very troublesome symptoms, and the varied remedies which may be employed in a case of chronic vomiting may be gathered from the following history :—

CASE IV.—*Dyspepsia—Vomiting of Fermented Matter containing Sarcinæ.*

Thomas Spence, æt. 53, a weaver. Admitted September 6, 1852. He states that, for fourteen or fifteen years past, he has been subject to occasional vomiting, which generally occurred on Sundays, owing, he supposes, to want of exercise at his usual employment. On these days he scarcely ever took his meals from fear of the almost certain vomiting which would follow. For two or three years past he has been liable to frequent heartburn, water-brash, and acid eructations, but was able to continue at his usual employment till about six months ago. Since then, he has been gradually losing his appetite, and his strength has become much prostrated. He has never vomited blood or any dark-coloured matter, and has never passed any such by stool. On admission, tongue clean ; no difficulty in deglutition ; appetite capricious, but always best in the morning and early part of the day. Shortly after taking food, he begins to have uneasy sensations in the epigastrium, sickness, and a sense of weight at the stomach. When these symptoms appear, the abdomen generally begins to swell, and in about an hour to an hour and a half, the food is frequently vomited. The rejected matters consist generally of the half-digested food, with a thick, dirty, frothy scum on the surface, resembling yeast. He has also frequent pyrosis, acid eructations, and flatulence, the latter sometimes so great as to occasion a sensation of choking, especially after vomiting. These symptoms are worse after some kinds of food than others: oatmeal, especially in the form of porridge, produces them in the severest form ; broths, vegetables, or any kind of slops, do not agree with him; animal food suits him best, but when even this is taken for any length of time, the symptoms soon re-appear. The abdomen at present is much swollen, very tense, and tympanitic on percussion, with considerable tenderness over the epigastrium. The bowels are generally constipated; the stools usually of a dark colour and hard consistence. He has occasionally slight pain and difficulty in voiding his urine, which is slightly phosphatic. Other functions are normal.

On taking charge of this patient on the 1st of November, I found him vomiting from time to time large quantities of fluid mixed with undigested matters, on which there gathered, after a short time, a thick brownish scum, exactly resembling yeast. On examining this scum with the microscope, it was ascertained to contain a large number of sarcinæ ventriculi, mingled with starch corpuscles, more or less broken down, and granular matter. From the ward books, I learnt that his treatment had consisted in the successive administration of,—1. The local application of leeches; 2. Of the sulphite of soda, in scruple doses, with two grains of aromatic powder three times a-day ; 3. Of half a grain of protochloride of mercury at night; 4. Of a scruple of the sulphite of soda every three hours, which was subsequently increased to half a drachm ; 5. Of creasote mixture; 6. Of a naphtha mixture ;

7. Of bismuth and aromatic powders; and 8. Of pills of calomel and opium. These different kinds of treatment, some of which, especially that of the sulphite of soda, had been continued for several weeks without intermission, seemed to have produced no good effect.—November 11. During the last four days, he has vomited every night, four hours after dinner, that is, about 6 P.M. The ejected matter presents the same yeast-like character formerly described; but the sarcinæ, though still abundant, are not so numerous. He complains of a great sense of distension, and a feeling of "working" or "bubbling" in the stomach shortly before vomiting. ℞. *Acid. Hydrocyan. dil. M.* xviii. *Syrup. Aurant.* ʒj. *Aquæ* ʒii. *M., half an oz. three times a-day.*—November 20. The hydrocyanic acid checked the vomiting till last night, when it returned with more violence than ever.—November 24. Vomiting still continues regularly every day. *Omittantur Mist. Acid. Hydrocyan.* ℞. *Liquor. Potas.* ʒss. *Aquæ* ʒvss. *Two table-spoonfuls to be taken every four hours.*—December 2. Alkaline mixture again checked the vomiting, which, however, returned last night to a slight degree. *Applicet Vesicat.* 4 x. 5 *Epigastrio.*—December 8. Vomiting has once more returned daily since last report. ℞. *Tinct. Ferri. Muriat.* ʒj. *Sumat* ʒss. *ter in die ex aqua.*—December 16. The vomiting has been again checked, but once more returned in a slight degree at 1 A.M. this morning. The matter ejected exhibits very little of the usual frothy scum, but consists of a brown liquid like coffee, with a few shreds of undigested food. It is of intensely acid re-action, and contains only a few sarcinæ. The dose of the Acid Tincture has been reduced to M. xv. The diet during this period has been principally animal, porridge and vegetables invariably increasing his complaint. To-day he left the hospital to visit his friends in the country, expressing himself as greatly relieved.

Commentary. — The kind of chronic vomiting and dyspepsia which is above described has been long known in Scotland, and was described by Cullen as a form of pyrosis. It has been supposed to be associated with the habit of largely consuming oatmeal as a principal part of the diet, although its real pathology was unknown. In 1843 Mr Goodsir discovered in the ejected matter from the stomach, in a case of this kind, organised forms, which, from their resembling a wool pack, he denominated *sarcina*. He considered that they were of a vegetable nature, and by multiplying fissiparously, communicated to the contents of the stomach the appearance of yeast, which is also known to be dependent on the development and growth of vegetable structures. This occurrence in the stomach of course explains their frequent presence in the fæces, although, whether they ever are developed in the intestines is unknown. On one occasion, however, I have seen them in the urine, which occurred in the case of a gentleman under the care of Dr Mackay of this city. The sarcinæ vesicæ were in that case uniformly smaller in size than the sarcinæ ventriculi. They have also been discovered in an abscess of the lung by Virchow. The exact mode of formation and origin of these structures are unknown; but little doubt can exist that their presence is the real cause of the chronic vomiting and other symptoms of the individuals affected, and that the cure will depend on such means as are capable of insuring their destruction and preventing their return. It must be obvious, however, that the same means which destroy or check vegetable growth on the surface of the body (see Favus), are not applicable to the mucous lining of the stomach. Besides, we are ignorant whether these parasites grow in an exudation poured out on the mucous membrane, or are developed only in a fluid. Again, it is very possible that once introduced from without, the conditions necessary for their development may be dependent on some kinds of ingesta, a view which derives support, from the facts observed in the case before us, namely, that they were always increased by farinaceous kinds of food. On all these points, however, we are as yet ignorant, and our efforts at cure hitherto have not so much been directed to cutting off the sources of growth, as to destroying it after it has proceeded to a certain extent. With this view it has been imagined that the

sulphite of soda would destroy them, by causing, on its union with the gastric juice, the extrication of sulphurous acid, which is so destructive to vegetable life. This remedy has consequently been given, and, it is said, with success; but in the present case it was of no benefit. Subsequently a variety of medicines were given, several of which succeeded in checking the vomiting for a time. Indeed, it was remarked that the mere circumstance of changing the medicine was sufficient to stop the vomiting for several days, when it returned and continued as before. Of all the numerous remedies tried, the Tr. Ferri Muriatis seems to have done most good.

CASE V.—*Chronic Ulcer of the Stomach.*

Janet Grant, æt. 30, married, admitted 14th November 1852. She stated that she enjoyed tolerably good health till twelve months ago, when she had an attack of hæmatemesis, which returned on three successive days early in the morning. The vomiting was preceded by a sense of weight and uneasiness in the epigastrium, nausea, dimness of sight, and feeling of syncope, which actually occurred on one or two occasions. After vomiting took place, she generally suffered from severe griping pains in the bowels until they were opened. The stools were often of a dark brown colour. The quantity of matters ejected from the stomach varied from one to two pints, and contained coagula of blood. She has been twice received into the Infirmary, and on both occasions dismissed much relieved. Since January 1852, she has continued in good health, and been able to follow her usual occupation till the 3d of November 1852. When the hæmatemesis returned, having been preceded by the symptoms already enumerated, on this occasion there was less blood than formerly; but the vomited matter still contained numerous coagula of blood. She had no recurrence of vomiting for eight days, but remained very weak and confined to bed. On the 11th November she passed by stool some matters like slimy tar. Since then she has frequently vomited, sometimes as often as three or four times a-day, a quantity of matter consisting principally of dark coloured blood. The pain in head, epigastrium, and between the scapulæ, have increased since that time. On admission, tongue moist, slightly loaded; appetite bad; food is rejected from the stomach almost immediately after being taken; pain and tenderness in the epigastrium on pressure; slight tenderness over the whole abdomen, which becomes much distended after taking food. Bowels rather costive; no blood in the stools at present, and no blood corpuscles in the vomited matters when examined by the microscope. Urine normal. Other functions normal. ℞. *Sulph. Magnes.* ℥ss.; *Acid. Sulph. dil.* ℥j.; *Infus. Rosarum* ℥viii. *Sumat* ℥ij., *et repetat. Cras Mane.* ℞. *Acid. Gallic.* ʒj.; *pulv. opii* griii. *M. et divide in pulv.* xii. *Sumat* j. *ter in die.*—November 15. Had some vomiting to-day, but no blood. Still considerable pain in epigastrium. Bowels freely moved by medicine; stools very dark in colour.—November 17. *Omit. Acid. Gallic.* ℞. *Bismuthi albi* ʒss., *Pulv. Opii* gr iss. *M. et div. in pulv.* vj. *Sumat* j. *ter in die. Farinaceous diet.*—November 18. Complains of burning pain at lower part of the sternum; former pain in epigastrium somewhat easier; no vomiting since the 16th; bowels open; stools still of a dark colour; great thirst; tongue loaded and coated with brown fur in centre; with bad taste in the mouth.—November 20. Still considerable pain and tenderness in epigastrium, which is now referred to one spot about the size of a crown piece; bowels confined; pulse 75, natural. *Applicentur hirudines* iv. *Epigastrio. Repetat. Mist. Cathartic. si opus sit.*—November 23. Complains still of pain in the epigastrium, which was slightly relieved by the leeches; has had no return of vomiting; bowels open. *Appl. Vesicat.* 3 × 2 *Epigastrio.*—November 26. Pain in epigastrium considerably relieved since the application of the blister. Still bad appetite and sour taste in mouth. ℞. *Carb. Potass.* ʒj. *div. in pulv.* xii.; *sumat* j. *ter in die.*—December 9. Pain in epigastrium much relieved, and only returns at intervals, and in much less degree than formerly. She now takes her food

well; bad taste in mouth gone; bowels still costive; pulse 80, of good strength.—December 11. The Catamenia appeared on the 28th, and lasted seven days. Since then she has suffered from severe dragging pains in the hypogastrium. A vaginal examination was made by Dr Simpson, and induration and ulceration of the cervix uteri having been found to exist, she was removed to Ward 12, under his care. Her former stomach symptoms have entirely disappeared.

Commentary.—The vomiting immediately on taking food, the hæmatemesis, and the local pain increased on pressure, indicated an ulcer of the stomach, which had eaten at several times into blood-vessels, and caused extravasations of blood. Before I saw her, an astringent mixture, with gallic acid, had been ordered to check the tendency to hemorrhage. For this I substituted quietude, a farinaceous diet to be taken in small quantities at a time frequently repeated, and powders of white bismuth and opium to check acidity and relieve pain. The pain not subsiding, six leeches were ordered to the epigastrium, followed by warm fomentations, and subsequently a blister was applied there, and the result of this treatment was to cause gradual abatement, and at length complete disappearance of all her symptoms.

CASE VI. *Chronic Ulcer of the Stomach.*

Mary Reid, æt. 38, married, admitted December 20, 1852. She states that about four years ago, having been exposed to cold and wet, she was seized with shivering, followed by severe pains in the epigastrium, with uneasy sensations in the lower part of the abdomen, resembling labour pains; these were accompanied by thirst, loss of appetite, sickness, and vomiting. These symptoms disappeared, and re-occurred at intervals up to December 1851, when she had a more severe attack than before, and since then she has always been complaining more or less of the same thing. About three months ago, she felt as if something gave way in the left hypochondrium, and nearly fainted. She immediately afterwards vomited about a tea-cupful of blood; this took place four or five times during the night; but the last time the ejected matters were paler and more watery, having somewhat the appearance of finely grated carrots. The loss of appetite, thirst, pain in epigastrium and bowels, with frequent severe headache, have continued up to the present time. She has had no return of the hæmatemesis, but generally vomits her food about half an hour after it has been taken. On admission, she has a pale anæmic appearance. The tongue is furred, appetite bad, pain in epigastrium and distension, with a sense of load at the stomach after meals, which continues till relief is afforded by vomiting, which comes on generally in about half an hour. She complains also of pain in the left hypochondrium; has no uneasy sensations in the bowels, but habitual costiveness, for which she has been in the habit of taking laxative medicine. She has been much troubled with palpitation, but the heart sounds and impulse are normal; pulse 96, small, and rather soft; micturition normal; urine of a pale colour, sp. gr. 1022°, with slight deposit, showing phosphates under the microscope. Catamenia now present. She does not sleep well, has slight headache and occasional vertigo, with muscæ volitantes, pain in lumbar region, but no tenderness over spine. Other functions normal. *To be left quiet in bed.* Appetite not improved; still much thirst. *Farinaceous diet in small quantities. To have the bowels gently opened.*—December 23. Bowels moved since last report; complains of much pain in the epigastrium; has had no vomiting of blood since admission. *Appl. hirudines* iv. *part. dolent. et postea foveatur. Omit. Alia.* ℞. *Lactis Recent.* ʒxii.; *Aq. Calcis* ʒvj. *M. To be taken as a drink when thirsty.*—December 30. Continues somewhat easier; complains still of occasional pain in stomach; appetite rather improved; less thirst. Bowels very costive.—January 4. Complains still of severe pain in epigastrium, with nausea, but no vomiting. Tongue rather furred. It has been found that the patient has been getting up and walking about after the visit, and has also been having some

beef-steak, contrary to orders. ℞. *Pulv. Scammon. Pulv. Jalap. ā ā* gr. x. *M. Hora somni sumend.*—January 6. Bowels well opened, tongue much cleaner, feels better, and slept well during the night. *To have rice diet.* Has been complaining again of pain in stomach; tongue furred, but moist; appetite rather better; slept well during the night; bowels open.—January 9. Has been rather sick to-day, and vomited a little during the night for the first time since admission. She still complains of pain in epigastrium. *Appl. Vesicat.* 3 + 2. *part dolent.*—January 12. Has been much relieved since the application of the blister, and expresses herself as feeling a great deal better. Tongue moist, and cleaner than before; less thirst; appetite improved; bowels still costive..—January 20th. Dismissed cured.

Commentary.—This case in all its essential features was very similar to the former one, with the exception that vomiting instead of occurring immediately after taking food, came on half an hour after eating. The same treatment was pursued, but was not so carefully followed, for it was ascertained that she was continually getting up and committing indiscretions which caused return of the symptoms.

CASE VII.—*Chronic Ulceration and Perforation of the Stomach; Peritonitis; Abdominal Abscess, simulating Pleurisy; Death.*

Evina Clark, æt. 29, single; housemaid. Admitted December 7, 1852. From the age of fifteen, she has had more or less derangement of the functions of the stomach, as exhibited by frequent vomiting of greenish matters, not preceded by any nausea. She attributes her complaint to a severe stomach disease at the age of fifteen, which confined her to bed for some months. Two months ago the vomitings became more frequent, and have continued worse than usual ever since. She has been in the habit of taking very large quantities of bi-carbonate of soda, sometimes even so much as 1 oz. per day. On the day before admission, she took a dose of castor-oil, and this morning (Dec. 7) she rose at 5 o'clock to stool, then returned to bed. At half-past 5, she again rose to see what o'clock it was, and again returned to bed and fell asleep without having experienced any pain. About 6 o'clock she awoke with severe pain in the epigastrium, and a feeling of faintness. Her mistress, on being summoned to her bed-side, administered to her half a glass of brandy in some hot water. Immediately after this was swallowed the former pain became excruciating; the abdomen was then fomented with hot water, and medical assistance sent for. The medical man ordered warm bran poultices to be applied, which somewhat relieved the symptoms; three hours afterwards the pain again becoming violent, one drachm of tincture of opium was administered, and she was sent to the Infirmary. The catamenia have always been regular.

On admission, she was in a state of extreme depression, the surface cold, face livid, pulse 108, almost imperceptible; and the house-clerk administered a draught, which was at hand, composed of *Sp. Ammon. Aromat. M.* x.; *Sp. Æther. Sulph. M.* xv.; *Sol. Mur. Morph. M.* xx.; *Aquæ* ℥ss. *Warm fomentations to the epigastrium were also ordered.* When first seen at the hour of visit, she complained of intense pain in the abdomen, especially in the epigastrium and left hypochondrium, which was increased by pressure. The tongue was slightly furred in the centre, but moist. She had great thirst, no nausea; bowels had been freely opened at five o'clock this morning. Heart sounds normal; pulse 136, the strength having much improved since the draught, which caused no increase of the pain. Is evidently under the influence of opium. All the other functions are normally performed. *To have immediately an enema of beef-tea, with an ounce of brandy. The warm fomentations to be continued.* In the evening the pain was diminished to a feeling of soreness; pulse 129, small; lividity of face and depression continue; surface cold; no rigors. Has had, at intervals of three hours, four enemata of beef-tea, and an egg, two containing an ounce of brandy, and two one drachm of laudanum. She has also been sucking ice

to relieve her thirst.—December 8. There has been profuse sweating during the night; face is still pale; pulse 126, weak and thready; acute pain continues on pressure below ensiform cartilage. The abdomen is tense and tympanitic, but the tenderness is slight; considerable flatulence in stomach; febrile symptoms well pronounced. ℞. *Bismuthi albi*, gr. xviii; *pulv. opii* gr. iii. *M. fiant pil.*, vi. *One to be taken every six hours. The nutritive and anodyne enemata to be continued.*—December 9. She vomited yesterday afternoon, 3 P.M., about a pint of green fluid, and at the same time passed a fluid feculent stool. During the night has slept somewhat. To-day she is somewhat refreshed, but the symptoms are the same as yesterday.—December 10. Yesterday evening, the epigastric pain having increased, and extended into left hypochondrium, six leeches were applied, followed by warm fomentations. To-day pain and tenderness continue; pulse 120, improved in strength; *six more leeches to be applied. The anodyne and nutritive enemata to be continued. To suck ice to relieve the thirst.*—December 11. Bowels were open shortly before the visit; pulse 128, full; tongue dry; thirst continues; but appetite is returning. Tenderness of epigastrium and abdomen has nearly disappeared. *To have beef-tea, by the mouth, in table-spoonfuls at a time, and occasionallg toast-and-water* to relieve the thirst.—December 12. The beef-tea produced a disagreeable but not painful sensation in the stomach, but there has been no vomiting. Pulse to-day 128, of good strength; feels much easier, and can turn herself more freely in bed. There have been two fæcal evacuations since yesterday.—December 13. She has no pain; pulse 128, of moderate strength. *To have a little toasted bread soaked in beef-tea.*—December 14. The toast and beef-tea occasioned uneasiness and tightness in the epigastrium and both hypochondria, followed by dyspnœa and general restlessness, but no pain. The bowels were opened twice during the afternoon and evening. To-day there is tenderness over the right hypochondrium; febrile symptoms have again returned; pulse 132, rather feeble. *Six leeches to be applied over the tender part. To have enemata of brandy and beef-tea every two hours. To suspend all food by the mouth.*—December 15. At the evening visit yesterday the febrile symptoms had greatly increased; thirst excessive; tongue dry and cracked; abdominal tenderness much relieved by the application of the leeches. To-day the febrile symptoms continue; face is flushed; and, on being interrogated, she states that she had a rigor and feeling of cold yesterday afternoon. On percussing the chest posteriorly, there is dulness over lower third of right lung, with double friction murmur and œgophonic vocal resonance; on the left side also slight dulness inferiorly, with crepitation during inspiration; pulse 132, feeble. The enemata, which have been continued at intervals, are no longer retained. *Intermittantur enemata. To have a little calf's-foot jelly by the mouth, alternated with a table-spoonful of clear brown soup every two hours.* ℞. *Solutionis tartratis antimonii*, ʒiij.; *Potassæ acetatis*, ʒij.; *Sp. ætheris nitrici*, ʒv.; *aquæ*, ʒj. *M. Fiat mistura. A tea-spoonful every four hours, in two or three table-spoonfuls of water.*—December 16. Yesterday evening there was great exhaustion and feebleness; *the mixture was suspended; and a table-spoonful of wine was ordered every two hours.* To-day no tenderness over abdomen, but the dyspnœa and the physical thoracic signs continue; febrile symptoms still strongly marked; pulse 140, soft and vibrating; there is much flatulence. At her own request, she was allowed *fifteen grains of the bi-carbonate of soda. To continue the jelly and brown soup, with half an ounce of sherry every hour.*—December 17. Feels better to-day; urine loaded with lithates; flatulence has been relieved by the bi-carbonate of soda.—December 18. No change. *To have milk and lime-water to drink.*—December 19. Complains of increased pain in inferior portion of right side of chest, where there is still dulness and loud friction. Some dyspnœa. *Six leeches to be applied.*—December 20. Pain was relieved by the leeches, but the dyspnœa and physical signs on both sides of chest continue. *Blister*, 4 + 3, *to be applied over lower portion of right lung posteriorly.*—December 21. Has had copious sweating during

the night; otherwise the same.—December 22. Much weaker; pulse 136, small and weak; lithates have disappeared from the urine; great dyspnœa. ℞. *Sp. æth. Nit.*, 3ss.; *Tinct. Colchici*, 3ij.; *aquæ*, 3v.; *a table-spoonful every four hours. To have an enema of beef-tea and egg; and rice, with beef-tea, by the mouth.*—Dec. 23. Is free from pain; general symptoms unchanged; pulse 124, weak; slight subsultus tendinum; appetite capricious; prefers arrow-root to rice.—December 25. Complains now of diarrhœa. *Habeat Enema c. Tr. opii, M.* xl.—December 26. Diarrhœa continues; early this morning took the following draught: ℞. *Sol. M. Morph. m.* xv.; *Tinct. Catechu*, 5ss.; *Syrup. Limonum*, 3j. *M. To be taken immediately in water.* No change in the febrile symptoms, dyspnœa, or the pulmonary physical signs; has no pain; pulse 124, weak; skin clammy; states that she felt very cold during the night.—December 27. Diarrhœa continues. *To have an enema of starch and opium.*—December 28. Diarrhœa has ceased; dyspnœa and febrile symptoms increased; no pain; face pale and anxious.—December 29. Evidently weaker; breathing laboured; pulse 140, weak and thready; countenance of a yellow waxy tinge. There was profuse sweating this morning; other symptoms unchanged. She continued to sink, and died at 3 A.M. on the 30th, death having been preceded by repeated vomiting of dirty brownish-green matter.

Sectio Cadaveris, thirty-three hours after death.

Head not examined.

Thorax.—Two drachms of clear serum in the pericardium; heart healthy; the right lung healthy, but its lower lobe and the diaphragm on that side were considerably pushed upwards by an abscess containing nearly a pint of pus, situated above the liver and below the diaphragm; the left lung also healthy, with the exception of a gangrenous ulcer, the size of a shilling, in the centre of the lower lobe inferiorly, where it rested on the diaphragm. This ulcer presented a brownish, broken up, sloughing surface, and was surrounded by red hepatization, occupying the pulmonary substance, to about the extent of a hen's egg.

Abdomen.—On reflecting the integuments, a considerable quantity of pus escaped from the abscess above alluded to on the right side, immediately below the diaphragm and above the liver. This abscess contained nearly a pint of pus, and was situated in a circumscribed pouch formed by the diaphragm above, the liver below, the peritoneum anteriorly and externally, and false lymph of considerable tenacity internally. Lymph also glued these parts and the small curvature of the stomach together. On reflecting the integuments, the anterior wall of the abscess was removed, and so the pus escaped. The stomach, transverse colon, and coils of intestine in the superior third of the abdomen, were all glued together by bands and flakes of lymph, which, though of tolerable tenacity, were gelatinous in consistence, and could readily be torn through by the fingers. In the left hypochondrium there was a layer of this lymph half an inch in thickness, softened, purulent, and gangrenous in the centre, situated above the spleen, and communicating, by a sloughened opening, with the ulcer and hepatization in the lung formerly described. On cutting open the stomach, in the line of its large curvature, there flowed out a dirty, greenish-brown, grumous liquid, containing coagulated masses, apparently of milk, tinged of a dark-red colour by port-wine. In the posterior part of the stomach, about its centre, was observed an oval ulcer, the size of a five-shilling piece, with smooth, thickened edges, and surrounded by puckered folds of the mucous membrane, which was otherwise healthy. The ulcer was adherent to the pancreas behind, which constituted its base; but the adhesions round its superior half were composed of the same gelatinous lymph as has been previously alluded to. On dissecting the ulcer from its attachments, it was seen to have completely perforated the coats of the stomach, although the opening behind, viewed on the serous surface, was not above the size of a shilling. On removing the intestines from the pelvis, flakes of purulent lymph

were observed between several of their coils and on the serous membrane of the pelvic cavity. All the other organs healthy.

Commentary.—All the facts connected with this case were obtained with great accuracy, and left us in little doubt from the commencement that we had to treat a chronic ulcer of the stomach, which on the morning of the day she was admitted had so perforated the stomach as to induce the violent pain she complained of. The peritonitis, which may have been induced by the perforation alone, was undoubtedly augmented by the brandy and water administered to rally her from the state of collapse into which she was thrown by the immediate effects of the accident. On entering the house also about five hours after she became ill, a stimulating and anodyne draught was administered by the clerk to rouse her from her depressed condition, but this was followed by no increase of local pain, but by improvement of the vital powers, and we may therefore fairly conclude that the practice, though highly questionable, was not productive of injury. Nothing, indeed, is more natural on the sudden occurrence of violent pain in the epigastric region with a feeling of syncope, than to have recourse to stimulants, for perforations of the stomach are rare occurrences, and it is not every one who at such a moment, even among the profession, has sufficient coolness and discrimination to detect the real nature of the disease. Hence, why so frequently these perforations are fatal, not from their own natural results, so much as from the stimulating remedies, which pass through the aperture into the abdominal cavity. Indeed, had not brandy and water been given in this instance, there is every reason to suppose that the perforation might not have occasioned much mischief, for it occurred early in the morning, before breakfast, and long after her evening meal, when the organ was consequently empty, and it is to be observed that such perforations have a great tendency to become re-closed by the rapid formation of fibrous lymph round their edges. In this case, however, extensive peritonitis was already occasioned when I first saw her, and the subsequent treatment was directed—1st. To prevent the introduction of further matters into the stomach ; 2d. To rally her from collapse by stimulating and nutritive enemata; and 3d. To combat the inflammation and conduct it to a favourable termination by local fomentations and opiates largely administered in the form of enemata, and subsequently in pills by the mouth. This treatment was attended with apparent success, so that on the fifth day nourishment was cautiously administered by the mouth, and also with tolerable benefit. On the eighth day, however, rigors appeared, followed by fever, which was attributed to a pleurisy on the right side, where increased thoracic dulness was discovered inferiorly, with loud friction and œgophony. Circumscribed pneumonia evidently also existed on the left side, as indicated by crepitation. This formidable complication was attempted to be combated by gentle salines, and topical bleeding by leeches, the condition of the patient not admitting of more active treatment. It was soon apparent, however, from the appearance of dyspnœa and other symptoms, that there was now little hope, and notwithstanding the liberal administration of stimulants, she sunk on the twenty-third day. Dissection exhibited exactly what was anticipated with regard to the stomach and peritoneum, but showed that the symptoms of the presumed pleurisy were owing to an abscess, which by pushing up the diaphragm and occupying the lower portion of the thoracic space on the right side, gave rise to all the physical signs of pleuritis. On the left side there was limited pneumonia as was expected, but it communicated by a gangrenous ulcer in the diaphragm, with the lymph exuded above and around the spleen. The edges of the ulcer were firmly united all round to the pancreas, so that the patient undoubtedly died of the extensive peritonitis occasioned by the brandy and water administered.

Various cases are on record, where evident perforations of the stomach have by judicious treatment been cured, and the following seems to me to be an instructive example of this favourable termination of the lesion.

CASE VIII.—*Chronic Ulceration of the Stomach—Perforation produced by a Fall (?)—Recovery.*

Barbara Ferguson, servant, aged 51, admitted January 6, 1853. States that she enjoyed excellent health till about eight years ago, when she first began to complain of her stomach—suffering from pain of a cutting or grinding character, always worse after taking food, and for which she was blistered about five years ago. Her appetite has all along continued good, but she often experienced considerable thirst; has never had nausea or vomiting. She believes that she has been getting worse lately, but has had no distinct exacerbation. On January the 4th instant, stepping upon a chair, her foot slipped, and she fell with the stomach across the back of it. She was immediately seized with intense pain in the epigastrium, rapidly extending over the whole abdomen. She did not faint, and was still able to speak, but had to be carried to bed. The accident occurred about 9 P.M., three hours after she had taken any food, which had consisted of some coffee, with a few mouthfuls only of bread. She was immediately ordered a one-grain pill of opium, which was to be repeated every four hours. On the next day, as the acute pain still continued, four leeches were applied to the epigastrium, followed by warm fomentations. She has had nothing by the mouth except the opium pills, up to date of admission. On admission she appears very weak and nervous, and in a state of partial collapse; the countenance is sallow; pulse 100; weak; heart sounds normal; no headache, but a feeling of vertigo on attempting to rise or change her position; tongue clean, moist; no nausea or vomiting; appetite gone; considerable thirst; pain in epigastrium, which, with the whole abdomen, is excessively tender on pressure; she has had great dysuria and pain on micturation ever since the accident. All the other functions are normal; *ordered to have no food by the mouth but an enema of beef tea, with the yolk of an egg immediately, to be followed in two hours by an opiate enema, with 40 minims of tincture of opium; to be kept quiet and not to get out of bed.*—January 7. Was almost free from pain yesterday evening, and felt altogether much better, having slept a good deal during the afternoon. *The beef tea enema, with yolk of egg, has been repeated at intervals of four hours, and she had another opiate at* 4 A.M., after which she slept well. To-day she feels easier; pain, or rather tenderness, in epigastrium somewhat diminished; and considerable pressure may now be exerted without causing uneasiness. She has still thirst; tongue dry; very little inclination for food; pulse 100, soft.—January 8. Was considerably easier last night, and expressed a desire for some food; the pulse was of better strength, 90. To-day is still improving; complains of no pain when lying quiet, but still pain on pressure in epigastrium; she expresses fear and pain when other parts are touched, but not to the same extent; pulse 95, of moderate strength. *She has had the beef tea, etc., enemata as before, with an opiate enema every* 10 *or* 12 *hours,—to have beef tea and milk by the mouth, in table-spoonfuls at a time, repeated every five minutes if the patient desires it.*—January 9. Felt rather uneasy after taking the beef tea and milk, which occasioned a sense of "working" in the stomach. An opiate enema was ordered in about two hours, and in the evening she expressed herself as free from uneasiness, and rather refreshed from the beef tea. To-day she feels not quite so well, and her general appearance is more depressed. She has continued the beef tea, but has had an egg and beef tea enema twice a day in addition; pulse 88, of good strength; bowels have not been open since admission; *to have a warm water enema, with an ounce of castor oil, followed, if necessary, by an opiate one.*—January 10. Felt rather weak and exhausted after bowels were opened; the opiate enema was administered two hours afterwards, and she has felt better since; had some tea, with a little toast, by the mouth this morning; takes beef tea for dinner, but cannot eat rice or any farinaceous food; no bad effects have followed taking food by the mouth; pulse 88, of good strength; little pain complained of, and she can now sustain considerable pressure on epigastrium without suffering. Slept less

last night than before ; urine clear, sp. gr. 1020, contains phosphates. *Omit the enemata.*—January 24. Since the last report she has been doing well, and gradually gaining strength.

Commentary.—This case is very like the preceding one, except that the symptoms were induced by a blow on the epigastrium, instead of coming on after straining at stool. Violent pain, tenderness on pressure, and collapse were the immediate symptoms. Fortunately, I saw the patient immediately after the accident, and with the case of Clark fresh in my remembrance, you may be sure I gave no brandy and water, or stimulating draughts. A grain of opium in the form of pill was administered every four hours, quietude enjoined, and complete abstinence insisted on. Next day the local pain continued, and on the following morning I sent her to the Infirmary. Nourishment was administered by enemata, but on the fourth day was cautiously given by the mouth, and no untoward symptom arose. Since then she has been slowly recovering. Of course we have no positive evidence that there was a perforation in this case, although for eight years she has been subject to severe attacks of pain in the stomach, increased on taking food. But there had been no vomiting. It is possible that the blow may have been sufficiently strong in itself to induce the pain and subsequent symptoms, although, from all the inquiries I could make, it did not appear to be so. One of her fellow-servants indeed maintained that it must have been trifling. Wherever anatomical evidence fails, there must be more or less uncertainty hanging over the history of those cases which recover ; but, taking all the circumstances into consideration, I cannot help thinking that had brandy and water been given in this as in the former instance, there is every chance that here also fatal peritonitis would have been occasioned.

(*To be continued.*)

Part Fourth.

PERISCOPE.

PHYSIOLOGY.

Experimental Researches applied to Physiology and Pathology.

By E. Brown-Sequard, M.D., of Paris.

(*Continued from p.* 66.)

VI.—ON A MEANS OF MEASURING DEGREES OF ANÆSTHESIA AND HYPERÆSTHESIA.

The curious facts discovered by E. H. Weber, on tactile sensibility, are well known. He found that if the two blunted points of a pair of compasses are applied simultaneously on the skin, there is, according to certain circumstances, either the sensation of one or of two points. When the points are both inside of certain boundaries, they are felt as one only ; when they are outside of these boundaries, both are felt. These boundaries vary exceedingly in different parts of the skin, but for a given part the differences between men are not extremely considerable. I have made use of the compasses for measuring the degrees of tactile sensibility in diseases. In a case of considerable anæsthesia of the lower extremities, the patient only felt a single impression on one leg, although the points of the compasses were ten, fifteen, or even twenty centime-

tres apart; whilst on the other leg he could distinguish them at a distance of twelve centimetres. The normal limit is generally, for that limb, from three to five centimetres. In another case where anæsthesia was slighter, the limit of the discriminating power was at from nine to sixteen centimetres. In two other cases, in which the diminution of sensibility had not been found by the other means of diagnosis, the compass indicated a very slight and beginning anæsthesia. The limit was at from six to seven centimetres.

These facts, and many others, have demonstrated to me that by the help of the compass, a physician can ascertain: 1st. Whether there is a slight anæsthesia or no. 2d. What is the degree of anæsthesia. 3d. What changes occur every day in the amount of anæsthesia.

The same is true for the cases of hyperæsthesia. In a case of paraplegia of the motor power, the patient felt the two points of the compasses, on his feet, even at the distance of five millimetres, whilst a healthy person feels the two points only when they are at a greater distance than twenty-five millimetres.

I shall add, that for succeeding in such experiments the two points must be blunted and applied simultaneously.[1]

VII.—ON THE CAUSES OF THE TORPIDITY OF THE TENREC (*Erinaceus ecaudatus*, Linn.)

The influence of cold as a cause of hybernation has not been considered essential, on account of an assumed fact, which is altogether incorrect. This fact is that a mammal called the Tenrec (*Erinaceus ecaudatus*, Linn.), which lives in the Islands of Mauritius, of Madagascar and Bourbon, is torpid under the joined influences of warmth and dryness. Cuvier says, about the Tenrec: "They are nocturnal animals, remaining in lethargy three months each year, although they inhabit the torrid zone. Moreover, Bruguiere asserts that it is during the time of the greatest heat that they are torpid." Physiologists have admitted this fact as true, and they have supposed that warmth and dryness could be, like cold, a cause of torpidity.

The following facts prove that the torpid sleep of the tenrec takes place from precisely the same cause as that of the hedge-hog (*Erinaceus Europeus*, Linn.), and of other hybernating mammals:—

1st. The tenrec and the hedge-hog belong to the same family, and they are both much alike.

2d. According to MM. Desjardins, Telfair, and Coquerel, and to my own observations, the tenrecs earth themselves, and are torpid from the month of June to the month of November, *i. e.*, during the winter season of the islands where they live.

3d. Hybernating animals, belonging to varied species, observed by Pallas, Mangili, Marshall Hall, Berthold, Barkow, and myself, have been found torpid at the temperature of 61 to 66° F. (16 to 19° Cs.) Moreover, I have found that dormice (*Mus. glis.*, Linn.) even at a temperature of 68 to 72° F. (20 to 22° Cs.) may become torpid, and I have observed some that were constantly sleeping, during a whole week, at a temperature varying from 59 to 68° F. (15 to 20° Cs.) I have stated that hedge-hogs may be torpid during the summer; in Paris, at a temperature of 68 to 72° F. (20 to 22° Cs.) and lately, in Philadelphia, I have seen a marmot (*Arctomys monax*, Buff.) torpid, in June, at a temperature of 71 to 73° F. (21.5 to 23° Cs.)

4th. During the time of its torpidity, the tenrec is under the influence of a temperature of 59 to 72 or 73° F. (15 to 22 or 23° Cs.) rarely more, and sometimes less. Therefore these animals are exposed to a temperature sufficiently low to render them torpid, for that temperature may produce that effect on the hybernating animals of cold countries.

From these facts I believe it is right to conclude that torpidity is induced in the tenrec, in the same way as in the other hybernating mammals, and therefore it is not necessary to suppose that warmth and dryness produce torpidity.

[1] See Gaz. Méd. de Paris, 1849, t. 4, p. 1012.

VIII.—ON THE INFLUENCE OF POISONS UPON ANIMAL HEAT AS A CAUSE OF DEATH.

Prévost and Chossat, and after them, M. Magendie, have ascertained that death occurs quickly in mammals when their temperature is notably diminished. My experiments confirm the correctness of that statement. The diminution of animal heat in mammals, is so dangerous that, in one case, I have seen death take place in a rabbit after a diminution of only 22° F. (12° Cs.) I have never observed any animal continuing to live when I had diminished its temperature more than 44° F. (24°.5 Cs.) I have found the law established by Chossat perfectly correct, according to which the diminution of animal heat necessary for killing is less and less, in proportion to the rapidity with which that diminution takes place.

It is very probable that in all the cases where, in consequence either of disease, or of a wound, or of poison, the temperature of man is diminished many degrees, his life is in danger from the very fact of that diminution. It is thus in cholera, in sclerema, in certain cases of palsy, in cases of great disturbance of the respiration, in fractures and luxations of the vertebral column, in the cervical, and even in the dorsal regions, in cases of profuse hemorrhage, and in many cases of poisoning when death is not rapidly produced.

It has been long known that temperature is diminished in poisoned persons; and there are but few cases of poisoning on record in which it is not said that the patient was cold. Chossat has found that a dog, into whose veins he had injected opium, had its temperature diminished from 105° to 62.6° F. (40°.3 to 17° Cs.), twenty-two hours after the injection. Brodie has found that many poisons act upon animal heat so as to diminish it considerably. Demarquay and Duméril, junior, and later, these two experimenters, joined with Lecointre, have found the same thing as Brodie in many toxic agents. I have made very numerous experiments on this subject, and some of their results have been published before the last papers of Demarquay, Duméril, and Lecointre.[1]

I have stated that many poisons, either injected in the veins or absorbed by the vessels of the skin or of the digestive canal, may diminish sufficiently the temperature of Guinea pigs and rabbits, to produce death. This occurs when the dose of the poison is not large enough to kill in less than four or five hours. These poisons may kill only by their action upon animal heat. It may be so with opium, cyanhydric acid, the cyanide of mercury, hyoscyamus, digitalis, belladonna, tobacco, euphorbia, camphor, alcohol, acetic, oxalic, sulphuric, azotic, chlorohydric acids much diluted, and some oxalates.

Of course the action of these poisons is the greater, the colder the atmosphere; but it is not always immediately so, and instead of diminishing the animal heat, many may increase it for a time, especially when the temperature of the atmosphere is elevated.

I have discovered that a dose of one of these poisons, sufficient to kill an animal, when there is no obstacle to the diminution of its temperature, may be unable to destroy life when the temperature of the animal is maintained by artificial means to its normal degree, or not far from it. My experiments have been conducted as follows:—

Equal doses of poison were given, simultaneously, to two animals, as much alike one another as possible. One of them was left in a room at a temperature of from 46 to 50° F. (8 to 10° Cs.), and the other was kept not far from a chimney, in a place where the air was at from 75 to 86° F. (24 to 30° Cs.) The first was dead after a certain number of hours, or sometimes one or two days, having its temperature much diminished. The other, on the contrary, had no perceptible diminution of its temperature, and was generally cured very soon. Therefore, when taken in certain doses, many poisons may kill only by their influence on animal heat, and physicians, in cases of poisoning, should try as

[1] See Gaz. Méd. de Paris, 1849, t. 4., p. 644.

much to prevent the diminution of temperature, as to expel the poison, or to act against it by an antidote, by pulmonary insufflation, or otherwise.

In experiments which I have made lately on the action of very pure digitaline, that had been prepared by M. Quévenne, the celebrated chemist, who has made such interesting and accurate researches on digitalis and the substances of which it is composed, I have found that this poison may also diminish temperature. I believe it is easy to explain the contradiction existing between Traube and Stannius, as regards the influence of digitalis on animal heat. When the atmosphere, in which the animal is, is cold, then its temperature may be diminished by digitalis or digitaline, but when it is warm the diminution does not take place, or it is very small. But, of course, if the dose is sufficient to kill very quickly, then it is indifferent whether the atmosphere is cold or not, because there may be not time enough for the diminution of the temperature of the animal.

I have to relate another fact which, I believe, ought to be considered as analogous to the preceding. It is that kind of poisoning which occurs when a layer of oil, of varnish or of gelatin, is put on the skin of a warm-blooded animal. Death, then, is very probably produced by a substance unknown until now, and which is secreted by the skin. The layer of oil, varnish, or gelatin preventing that secretion taking place, that unknown substance becomes accumulated in the blood, and then are produced the phenomena so well studied by MM. Fourcault, Becquerel, Breschet, and Magendie. I have found that in such a case the animals may live, if the atmosphere in which they are kept is at a temperature inferior to 79 or 80° F. (26 or 27° Cs.) In these circumstances their temperature is not sensibly diminished, while it diminishes much when the atmosphere is cold. Therefore it is especially by their loss of warmth that animals are killed, when their body has been entirely covered with oil, varnish, or gelatin.

Part Fifth.

MEDICAL NEWS.

EDINBURGH MEDICO-CHIRURGICAL SOCIETY.

SESSION XXXII.

MEETING II.—*Wednesday, December* 8, 1852.—DR BEGBIE, P., in the Chair.

DILATATION OF THE HEART.

1. *Dr W. T. Gairdner* read a communication "On the Causes of Dilatation of the Heart." The object of the paper was to review the different circumstances under which dilatation, and the concomitant lesion of hypertrophy, of the heart arise, and especially to determine, by the analysis of a considerable number of recorded observations, the relation of dilatation of the heart to diseases of the lung. The author observed, that obstruction of the circulation on the one hand, and debility or disorganisation of the heart's fibre on the other, were the causes to which most cases of cardiac hypertrophy and dilatation were referred by pathological writers, the residual cases being usually considered as inexplicable on mechanical principles. Among the causes of obstruction in the course of the circulation, leading to secondary disease of the heart, chronic pulmonary diseases, and especially emphysema, had been long recognised. Dr Gairdner stated, that his own experience fully corroborated the assertions of authors as to the connection of emphysema of the lungs with simple hypertrophy and dilatation of the heart; but he adduced numerous considerations tending to prove that obstruction in the pulmonic capillaries was not, as commonly supposed, the sole, or even the

principal, cause of this connection. From the examination of a variety of details, Dr G. concluded, that emphysema of the lung, and simple dilatation or hypertrophy of the heart, very frequently co-existed, and stood in the mutual relation, not so much of cause and effect, as of joint results of a common cause; and that dilatation of the heart arose in connection with pulmonary disease, under the same circumstances as the dilatation of the air-cells in emphysema; viz., when the capacity of the thorax was out of proportion with the volume of the lungs, these organs having suffered partial atrophy from some chronic disorganising disease. Under these conditions (as Dr G. had formerly attempted to demonstrate), emphysema arose, by the effect of inspiration upon a lung not prepared to dilate equably, and therefore dilating irregularly, and, in some parts, excessively. Under precisely similar circumstances, the heart yielded to the dilating force of the external parietes, became habitually gorged and distended with blood, and finally became the subject of organic permanent dilatation, with or without corresponding hypertrophy. Dr G. remarked that this theory of the connection of cardiac with pulmonary disease was not only consistent with the facts already known, but would tend to explain numerous cases of hypertrophy of the heart, hitherto inexplicable, in which disorders of the lung of more recent origin have masked the original atrophic lesions. Such cases could only be properly understood by means of careful and correct clinical study.

A discussion took place on Dr Gairdner's paper, in which

Dr Bennett said that a case had recently occurred to him, bearing on the theory suggested by Dr Gairdner. A young man had died in the infirmary after a protracted illness, and having had all the signs of a healing tubercular excavation in the left lung. On examination, this lung was reduced to little more than the size of a man's fist, and contained numerous small cavities, lined by perfectly firm and smooth membrane. The opposite lung was healthy, with the exception of a few traces of healed tubercle in the upper lobe. If atrophy of the lung were, as Dr G. supposed, a cause of hypertrophy and dilatation of the heart, the latter affections ought, as it appeared to him (Dr B.), to have been present to a marked extent in this case. The heart was, however, normal or nearly so, and he therefore appealed to Dr Gairdner to reconcile the case with his theory, he (Dr G.) having been present at the dissection.

Dr Alexander Wood thought it necessary to enter a *caveat* against the views of Dr Gairdner, although from the mode in which he had treated the subject, and the number of details involved in the argument, he (Dr W.), was not prepared to state all his specific objections. He thought that the older mechanical theories, and particularly the obstruction theory of cardiac disease, had been undervalued by Dr Gairdner; and that the effect on the heart of the obstruction to the current of blood, could not be set aside, considering the illustration which it derived from valvular disease. He considered, too, that loss of tone from inflammation and other disease of the heart, was a very unquestionable cause of dilatation. The class of cases referred to by Dr Bennett was a large and important one; and he thought that the theory promulgated by Dr G. failed altogether as applied to phthisical cases.

Mr Glover said a few words in objection to the inspiration theory of emphysema.

Dr W. T. Gairdner said, in reference to Dr Bennett's case, that he thought the absence of cardiac disease admitted of a very clear explanation. The man was reduced to the last stage of emaciation and exhaustion long before death, and although the remains of extensive lung disease existed, there was practically no serious impediment to respiration, and no necessity for preternaturally strong inspirations. The evidence of this was not only the absence of heart disease, but the absence of dyspnœa during life, and of emphysema of the remaining lung after death. The discharge from a large number of tubercular abscesses in the kidneys, had in fact operated as a drain upon the system, reducing the amount of blood to a conformity with the diminished breathing capacity. Accordingly, the body was emaciated and pallid, the chest lank and narrow on both sides, the whole of the organs smaller than usual, except perhaps the heart, which was not

at all remarkable. Hypertrophy and dilatation of the heart could not reasonably be looked for as a result of atrophied lung in a case like this; but he (Dr G.) was perfectly willing to test the theory he had advanced by phthisical cases, and indeed had referred to them in his paper, as illustrating it. He admitted that in ordinary cases of phthisis there was no tendency to cardiac dilatation; but in ordinary phthisis, besides the existence of a constitutional condition unfavourable to over-distension of the heart, the local circumstances were altogether the opposite of those under which emphysema and dilatation of the heart occurred. Atrophy of the lung only occurred in very chronic and partially healing cases of tubercle; and accordingly wherever the cure of an extensive tubercular affection had advanced to a considerable extent without the counteracting circumstances in Dr Bennett's case, Dr G. had found that dilatation and hypertrophy of the heart almost invariably occurred to a greater or less extent. This proposition was entirely borne out by an analysis of his cases; and so far from tubercular cases being, as supposed by Dr Wood, at variance with the law he had endeavoured to maintain, they had always been regarded by him (Dr G.) as one of its principal illustrations. In regard to Dr Wood's other objections to his paper, Dr Gairdner would only say, that he had no thought of either overthrowing or of undervaluing the older theories for the sake of setting up his own. He had in fact strongly urged the evidence of both the theories favoured by Dr Wood, but claimed a place in pathology for the new law, on the ground chiefly of facts hitherto unexplained, or which he at least thought he had shown by ample evidence not to be satisfactorily explained. With these remarks, he left the subject to the mature consideration and renewed observations of the members of the Society.

The *President* read a valedictory address, alluding shortly to most of the papers and communications read to the Society during his term of office, and giving a general historical retrospect of the condition of the Society during the last two sessions.—(See Monthly Journal for January 1853).

ELECTION OF OFFICE-BEARERS.

The following gentlemen were elected office-bearers for the present session,—viz.,

President.—Dr J. Y. Simpson.

Vice-Presidents.—Dr Douglas Maclagan, Dr Combe, and Dr Scott.

Councillors.—Dr Charles Bell, James Miller, Esq., Dr David Wilson, Dr William Robertson, Dr John Gairdner, Dr John Taylor, Benjamin Bell, Esq., Dr James Struthers.

Treasurer.—Dr Robert Omond.

Secretaries.—Drs W. T. Gairdner and J. Warburton Begbie.

MEETING III.—*Wednesday, 5th January.*—Dr SIMPSON, President, in the Chair.

Dr Simpson, on taking the chair for the first time, delivered an address, pointing out the particulars in which the science of medicine might be said to have advanced during late years, and showing how false were the statements of some, that of all the sciences medicine was the only that had during that period made no progress.

1. *Dr R. J. Mackenzie* exhibited a limb amputated below the knee, on account of gangrene of the entire foot, occurring during typhus. Dr Mackenzie pointed out how very nearly, in this instance, a natural amputation had been completed.

2. *Dr Alexander Wood* read a communication, entitled "Practical Deductions from Physiological Laws, in regard to Diseases where the Properties of the Moving Fibre are implicated."—(See Original Communication in present Number of Journal.)

Dr Matthews Duncan thought that Dr Wood had omitted to bring prominently forward what were now-a-days universally considered to be the most important and most common causes of convulsive seizures. These were of two classes, either,—1st, Purely nervous in their origin, including irritations of the

peripheral nerves on the various surfaces of the body, or irritations of the nervous centres; or, 2d, Where there existed a poison in the blood, introduced artificially, or the result of a morbid process in the economy, as was for instance now generally believed to be the case in some forms of puerperal convulsions, where albuminuria afforded evidence of the morbid state of the blood. The former of these classes offered a clear indication of treatment,—namely, the removal of the irritating cause,—the convulsions arising from dentition was a familiar example. He differed from Dr Wood in considering the liberation of the tooth by incision the chief object of treatment. He thought the condition to which Dr Wood attached most importance as causes of spasmodic affections exerted no such direct influence, but should be regarded as secondary influences modifying the violence and frequency of the attacks.

Dr Maclagan had seen many cases of tetanus, chiefly among soldiers, and occurring after lacerated and incised wounds. In all such he had found the most effectual plan of treatment free purgation, to which he attributed much importance, and the exhibition of opium.

Professor Miller looked upon the efficient treatment of tetanus as, in the first instance, clearing away any cause of irritation in the bowels, for always in tetanus noxious matter lodged there. This was to be done by active purging, and then the bowels to be kept open by gentle aperients; enemata were often sufficient. Purgation he regarded in all cases as essential, because the noxious accumulation in the bowels was often the cause of origin, and in all the cause of maintaining the disease. In traumatic tetanus the origin usually lay in the injured part, the nerves there being irritated by inflammatory or other change. This was to be met by enervation as by letter V incision, or by amputation. The latter was only expedient at the early stage. Besides limiting amputation to the early stage, Professor Miller felt inclined to adopt it only in the cases of minor operations. Large amputations both occasioned great shock, and involved a large part of the nervous structure in irritation by the inflaming wound. Antispasmodics should also be administered. He preferred Indian hemp. Tobacco in many cases has the power of mitigating the spasms, but not in all; while it has the effect, if employed freely, of lowering the patient too much. Had published successful cases; was not prepared to say what share amputation of the finger, which had been performed in one, or attention to the bowels, or Indian hemp, had in the cure; all did something along with general treatment.

Dr Mackenzie believed that free and repeated evacuation of the bowels was an essential point in the treatment of tetanus. He could not believe that opium, by its sedative action, had any effect in this way. However well Dr Wood's method of treatment might appear in theory, it did not seem to answer in practice. The obstinate constipation, which was one of the most frequent features of tetanus, only yielded to the more active purgatives. Many surgeons placed their chief reliance on croton oil. Dr Newbigging had published a case successfully treated in this way. The only case which had recovered under Dr Mackenzie's own care had been treated by free purgation, and by the exhibition of morphia, so as to produce sleep.

Dr Bennett, reverting to the subject-matter of Dr Wood's communication, differed from its author, as to the manner of advancing medical knowledge. He considered that careful observation by experiment, chemical analysis, and the examination of minute structure by means of the microscope, in conjunction with the study of disease at the bedside, and morbid anatomy, were the only rational modes for determining obscure points in our science. Whenever speculation upon unknown vital causes, as practised by Dr Wood, was adopted, it was sure to lead to nothing definite. Dr Bennett made some further observations on the treatment of tetanus, especially on the employment of tobacco; on amputation of the injured part, and division of the nerves leading to it.

Dr Mackenzie stated that recovery had undoubtedly occasionally followed amputation of the wounded part, but very rarely, and little confidence was now placed in amputation as a means of cure. With regard to the division of the

nerve or nerves leading to the wounded part in traumatic tetanus, he believed that the case referred to by Dr Bennett was the only one on record after which benefit had accrued.

Dr Andrew Wood, while he did not agree in all that Dr Wood had propounded in his paper, thought that there existed much in it calculated to suggest useful reflection, and to put practitioners on their guard in the treatment of convulsive disorders. In these diseases, as affecting children, there could be no doubt that many cases were associated with a sthenic state, which called for the employment of antiphlogistic treatment; while, in other cases, these convulsions were the result of an asthenic condition however produced. To treat the latter antiphlogistically would be manifestly irrational; and experience showed that the best effects were produced by generous diet, diffusible stimulants, opiates, etc. There existed then a necessity for nice discrimination. Allusion had been made to laryngismus stridulus, and the efficacy of scarification of the gums in checking it. That such a case might sometimes exist he believed, but was it not the case that the disease often existed independently of teething altogether; and that repeated scarification, even in cases in which the gums were swollen, was productive of no benefit? Practitioners who had seen much of this disease were agreed that its most effectual treatment consisted in adding strength to the system, by good diet, warm clothing, and mild sedatives, and especially by change of air. Dr Wood farther alluded to the treatment of puerperal convulsions.

Dr Alexander Wood explained that he had no intention of undervaluing the microscope and the chemical re-agents, as Dr Bennett said; but he did consider that by some they were too exclusively pursued; they make us acquainted with the results, but never with the process of diseases. To the facts with which they have furnished us, available theories must be superadded. In reference to what had been said by other speakers, Dr Wood explained that he did not object to the action of the bowels being secured in tetanus, or to the scarification of the gums in infantile convulsions, but he did object to the employment of these means in cases where the irritation of the purgative, or of the gum lanced, was likely to prove greater than the condition it was intended to remove.

EDINBURGH PHYSIOLOGICAL SOCIETY.

SESSION III. (*Winter* 1852–53.)—MEETING III.—18*th December* 1852.

Dr GAIRDNER, Vice-President, in the Chair.

1. *Dr Gairdner* showed some cancer cells, to which he had alluded at last meeting as exhibiting the usual characteristic of these structures.

2. *Dr Cobbold* made a lengthened verbal communication "On the Structure and Functions of Actiniæ." He commenced by stating, that, notwithstanding the ease with which these animals were obtained, and all that had been written about their organisation, reproduction, and mode of development, he believed, from a few recent original observations, that our knowledge of their true structure and functions was still very imperfect. He was convinced—

In the first place, that the stomach was not a simple sac closed at the base, but (as the specimens of *A. gemmacea* on the table showed) there existed at the bottom of that organ a considerable opening communicating with the ovarian chambers.[1]

Secondly. Dr C. had watched and witnessed the progress of evolution of the young of Actinia *from the mouth*. This observation coincided with the statements of Spix, Dalyell, and others, and confirms their opinion that Actiniæ are viviparous; and this fact, together with the circumstance of the opening at the base of the stomach, entirely clears up the mystery as to how the young polyps are set free. He proceeded further to show—

[1] This opening is not represented in the *figures* given by Drs Sharpey, Quoy, Gaimard, and others. Professors Carpenter and Rymer Jones state that there is no communication; Owen and Grant recognise it; and Mr Goodsir informs me, that he has repeatedly pointed it out in his lectures.

In the third place, The mode in which the young *were developed*, by which it was also demonstrated that the stomachal cavity must not only possess a superior, but also an inferior (or pyloric?), aperture, communicating with the interseptal chambers, and these again with the tentacular (or morphologically true anal) orifices with the surrounding medium. He had noticed, in common with others, that after the digestible matters of any shell-fish had been absorbed into the economy, the shell or indigestible portions of ingesta were expelled at the oral aperture; but having watched for hours and seen the young Actiniæ expelled by the mouth, he felt certain that some opening must exist in connection with the ovarian chambers, and this was fully borne out by tracing their *mode of development*.

Fourthly. When the young escaped, they invariably sank to the bottom of the vessel, resting on their outspread tentacula, with their *base* or portion corresponding to the sucking-disc uppermost; they then moved this latter part of the body sideways after the manner of Hydræ, and if the mother Actinia or any projecting body lay in the vicinity, they immediately attached themselves to it, and there remained. Some of them, becoming adherent to the adult Actinias, gave the idea of a gemmiferous mode of reproduction, which curious circumstance Dr Cobbold believed to explain fully the notion of reproduction by gemmation hitherto ascribed to the whole group of these animals by so many able and distinguished writers. Their multiplication by fissiparous division was well illustrated by the specimens then before the Society.

Fifthly. Dr Cobbold believed the so-called tubercles which exist in *A. mesembyanthemum* to be functionally *eyes*. In one species in which he had examined them microscopically, he had found them to contain in their interior innumerable small fusiform crystals. The blue colour of these *eyes* (?) was entirely owing to the presence of these mineral bodies, and not to the existence of true pigmentary matters, as some had supposed. The mode in which these crystals are formed, and the fact of their occurring also (in an imperfect state) round the edge of the sucking-disc, was also pointed out. Some remarks in conclusion were made on the histological characters of Actiniæ and of polyps generally, but details on this head were reserved for a future meeting.

EPIDEMIOLOGICAL SOCIETY.

A Committee of this Society is at present engaged in investigations connected with small-pox and vaccination; and at the request of the secretary, Dr Seaton, we beg to submit to our readers the following list of questions, which has been already pretty extensively circulated among members of the medical profession in England, and to which it is desired that equal publicity should be given in Scotland.

Answers to any or all of these queries should be directed to Dr Seaton, 33, Sloane Street, London.

1. Have you any record of the cases of small-pox, which have fallen under your observation, whether,—*a*, Natural, *b*, After a previous attack of small-pox, or *c*, After vaccination,—and if so would you state the number of each kind and any particulars you may be acquainted with, especially with regard to the ages of the patients and the character of the attack in each class of cases?

2. What has been the mortality in each class that you have specified, and in how many cases, *especially in those after vaccination*, could the death be fairly ascribed to small-pox, or to previous, coincident, or superadded disease?

3. In each case of small-pox after vaccination, how long a period had elapsed since the performance of the vaccination?

4. In cases of small-pox, presumed to be after vaccination, what was the evidence of the vaccination having been effective?

5. Have you any facts to show that the protective or modifying power of vaccination bears any relation to the *number* of genuine cicatrices left by the operation?

6. Have you *seen* the vaccine disease, and subsequently small-pox, occur in the same individual, and if so, in how many cases?

7. Have you ever had an opportunity of watching an epidemic of small-pox from its outbreak to its termination, and would you favour the society with the result of your observations? The committee are desirous of being informed whether the disease first appeared among the resident inhabitants of the district, or whether it was imported, or produced *by inoculation?*

8. Have you any evidence of the spread of small-pox from the improper exposure of persons labouring under the disease?

9. In what manner are you in the habit of performing the operation of vaccination? How many punctures or incisions do you make? In a person whom you have vaccinated, what are the tests which satisfy you that the vaccination has been effective?

10. Are you in the habit of omitting to vaccinate in the winter, and, if so, for what reasons?

11. Are you able to maintain a supply of lymph? If not, how are you supplied? Have you (at any time) experienced difficulty in obtaining lymph when required?

12. Have you ever seen any anomalous effects follow vaccination, or anything to support the notion that other diseases may be introduced into the system through vaccine lymph?

13. Have you ever vaccinated directly from the cow, or known any one so vaccinated, and with what results?

14. In how many cases have you re-vaccinated? what were the results of the operation? what evidence had you in each case of the former vaccination having been effective? Have you any facts in proof of re-vaccination affording additional protection from small-pox?

15. By any inquiries you have had occasion to make on the outbreak of small-pox, have you ever been able to ascertain, or could you now ascertain, the number of unvaccinated persons in your town, parish, or district?

16. Have you known instances in which vaccination of all the inhabitants of a village or district has prevented the epidemic visitation of small-pox, although it prevailed in adjoining places; and if so, will you state the exact particulars?

17. What have you found to be the principal obstacles to vaccination being universally practised, and can you suggest any plan for insuring the vaccination of every person throughout the kingdom?

18. Have you *yourself* been vaccinated, and at what age? Are there any cicatrices visible, and, if so, what is their number and character? Have you been re-vaccinated, and with what effect? Have you been frequently exposed to small-pox? Have you taken small-pox—and, if so, what period had elapsed since the vaccination, and since the re-vaccination?

19. What is your opinion of the efficacy of vaccination as a protection against small-pox?

PUBLICATIONS RECEIVED.

A Dictionary of Domestic Medicine. By Spencer Thomson, M.D. London: Groombridge. (Completed.)

On Some Affections of Bone. By Langston Parker, Esq. London: Churchill. 1853.

On Moral Sanitary Economy. By Dr M'Cormack. Belfast.

Syphilitic Diseases: their Pathology, Diagnosis, and Treatment. By John C. Egan, M.D. London: Churchill. 1853.

Anatomy and Physiology of the Vegetable Cell. By Hugo v. Mohl. Translated from the German by Arthur Henfrey, F.R.S. London: Van Voorst. 1853.

Braithwaite's Retrospect. Vol. II., for 1852.

The Medical Intelligencer. London: Churchill. 1853. No. I.

Contributions to Obstetric Pathology and Practice. By J. Y. Simpson, M.D. Part I. Edinburgh: Sutherland & Knox. 1853.

New York Medical Times. Vol. II. Nos. 1, 2, 3. October, November, December, 1852. New York: S. S. & W. Wood. 1852.

On Perforating Ulcer of the Stomach. By Edward Crisp, M.D. London: Teape and Son. 1852.

Appendix to Treatise on Diseases of the Blood-Vessels. By Edward Crisp, M.D. London: Teape & Son. 1851.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Pathology of the Bronchio-Pulmonary Mucous Membrane.* By C. BLACK, M.D., Chesterfield, Bachelor of Medicine, and formerly Medical Scholar in Physiology and Comparative Anatomy in the University of London; Fellow of the Royal College of Surgeons of England, etc. etc.—(*Continued from page* 97.)

First Pathological Condition of Bronchitis involving the Submucous Tissue.

All the severe forms of simple bronchitis are referable to this variety, which, in many pathological points, resembles the epithelial variety of the disease, but which nevertheless differs from the latter in several very important particulars. These points of difference refer to,—

1. Respective duration.
2. Type.
3. The tissues involved.
4. Nature and composition of the discharge from the affected membrane.
5. Effects, both primary and secondary.

The differences thus specified may be briefly described. Epithelial bronchitis is always acute, whereas the above variety may be either acute or chronic. The former is always sthenic in type; the latter is either sthenic or asthenic. In epithelial bronchitis, the epithelium and basement membrane only are affected; in the above variety, the submucous tissue is likewise involved. In the former, the discharges from the affected membrane consist of epithelial scales or patches, and an excess of the natural secretion; in the latter, certain organic and inorganic bodies are added to the above.

The effects of epithelial bronchitis are epithelial desquamation or ulceration, and epithelial and basement hypertrophy. Of bronchitis involving the submucous tissue, ulceration, hypertrophy, bronchial abscess, and bronchial obstruction, as primary; and emphysema, bronchial dilatation, and collapse and atrophy of the pulmonary tissue, as secondary.

Now, in reference to simple acute bronchitis involving the submucous tissue, the first pathological condition of the bronchial mem-

brane is similar to the corresponding stage of epithelial bronchitis, except in these particulars; that the congestion is more intense, and that the phenomena arising out of it bear a corresponding aggravation. But this aggravation may either be accompanied by sympathetic fever, denoting power, or by an excessive action, coupled with vital prostration, showing a marked deficiency of constitutional power. With the former, or sthenic condition, there is a disposition in the bronchial membrane to prolong the duration of its first pathological stage; with the latter, or asthenic condition, the tendency is to pass rapidly into the second pathological stage. The explanation of these facts is readily found in the great amount of vital tonicity of the bronchial capillaries in the sthenic form of the disease, which, in the manner before described, successfully opposes exudation for a time; and of the small amount of vital tonicity in the asthenic form, which very quickly allows the pressure of the blood to overcome the contractility of the bronchial capillaries, and thus to permit an early exudation to take place. Under both the sthenic and asthenic forms of the disease, however, the pathological condition of the bronchial membrane is similar, not only in reference to each of these forms, but also to the first stage of epithelial bronchitis; therefore the phenomena immediately dependent on this condition must be essentially similar in all of them. But the expression of these phenomena will certainly be modified by the extent and intensity of the disease, and the amount of constitutional power which exists. Hence, in the variety of bronchitis now being considered, in which the bronchial engorgement is greater than in the epithelial variety, the sense of oppression and of straitened breathing is greater than in the latter disease. This aggravated feeling is partly due to the relative increase of blood in the bronchial capillaries, as compared with the epithelial variety, and partly to spasm of the muscular fibres of the bronchi, induced by the irritation resulting from the pressure of engorged capillaries, which two conditions diminish, in a proportionate ratio, the natural diameter of the tubes, and thus shut out a greater quantity of air than in the epithelial form of the disease.

For the same reason the sibilant rhonchi are more intensified, and partake more or less of a musical character; whilst the deep, sonorous rhonchi of the epithelial variety are, in this form of the disease, represented by rhonchi inclining to the sibilant character.

The percussion note is said not to undergo any alteration as a general rule; nevertheless, that it is occasionally rendered somewhat clearer than natural.[1] In the first pathological condition of the variety of bronchitis now under consideration, there is certainly never any increase of the percussion note; but, on the contrary, there is, if the disease is at all extensive, and more particularly if it affects the capillary bronchi, a slight, yet manifest, deadening of the percussion sound.

[1] *Vide* Walshe on Diseases of the Lungs and Heart, page 258.

The pathological condition of the membrane, the mechanism of respiration, and the relative effects of oxygen and carbonic acid gas on the contractility of the bronchial fibre, confirm the truth of the above statement, but in what way the following explanation will show.

The bronchial membrane is deeply congested, owing to which the natural diameter of the bronchi is diminished, the sentient nerves irritated, and a reflex motor impulse produced in the bronchial fibre, which, by its contraction, still further narrows the caliber of the tubes. The result of this is a partial admission only of the normal quantity of air into the pulmonary cells. The desire for more air, or rather the sensation of want of air, is excited in proportion to the deficiency, and the mechanism of respiration is taxed to overcome the obstacle which opposes the introduction of the usual quantity of air into the lungs. Hence arises a correspondent action of the supplementary aids of respiration; but notwithstanding the increased muscular effort thus excited, but little more air is admitted into the pulmonary cells, the walls of which are consequently not so tense as in the healthy inflation of the lungs. With a deficiency in the gross amount of air in the lungs, there must of necessity be a corresponding flaccidity of the cell-walls, and, as a consequence of the latter condition, a proportionate deadening of the percussion note. But it may be said, that the proper distension of the cells is maintained by the retention and rarefaction of the included air, owing to the impediment which is offered to expiration by the narrowing of the bronchial tubes. Such impediment does not in reality exist beyond the degree of narrowing produced by the mere turgescence of the bronchial membrane; because the carbonic acid gas, with which the expired air is surcharged, quickly destroys, for a time at least, the contractility of the bronchial fibre, thereby relaxing spasm, and favouring the egress of air from the lungs. But, admitting that carbonic acid gas exerts no such influence over the contractility of the bronchi, that consequently the diameter of these tubes is the same in expiration as in inspiration, and that the natural expiratory force is, under the above circumstances, insufficient to expel a proper quantity of air from the pulmonary cells, it is evident that the distressing sensation of the want of breath thus occasioned, would excite a forced expiratory act, which being one-third more powerful than the extreme of forced inspiration,[1] would more than counterbalance the latter effort, and thus cause the greatest amount of vesicular distension to depend, not on rarefaction of included air, but upon the inspiratory act. Again, on the other hand, oxygen excites the contractility of the bronchial fibre, and thus the impediment offered by spasm is added to that occasioned by bronchial turgescence, during the inspiratory movement. It is hence certain, that the impediments to inspiration are greater than those to expiration; that consequently

[1] See Reid's article on Respiration, Cyclop. of Anat. and Physiol., Part 32.

there is no inordinate retention of air in the ultimate cells of the lungs, and that no unusual amount of rarefaction takes place. With these conditions for free expiration, relatively considered, and with a deficient supply of air during inspiration, there must arise a defective inflation of the lungs, which must and does produce a corresponding diminution of the healthy percussion sound of the chest. It may, therefore, be truly asserted,—

That the pathological condition of the bronchial membrane, in the first stage of inflammation, has an invariable tendency to diminish the healthy percussion note of the chest.

But the amount of this diminution, and consequently its appreciability or otherwise, will depend on the extent and type of the disease, and on the particular bronchi affected.

In all cases in which the capillary bronchi are the chief seat of disease, the percussion note is lower than in bronchitis affecting the larger tubes; and, in like manner, the asthenic exceeds the sthenic type in this tendency to interfere with the natural resonance of the chest.

The indications of treatment deduced from the pathological details above given, are in the main similar to those of the first stage of the epithelial variety of the disease. Here, however, a full bleeding will, in the sthenic form of the disease, produce a decided effect in relieving the engorged condition of the bronchial membrane. But this condition will only be temporarily relieved, unless *cold air* be constantly applied to the affected membrane, so as to maintain the vital tonicity of the bronchial capillaries, and thereby to prevent their re-engorgement with blood, which would otherwise inevitably take place on re-action following the temporary depression produced by the loss of blood. Bleeding carried to the extent of producing a decided impression on the system, by lowering the heart's action, diminishes the supply of blood to the bronchial membrane, the vessels of which, by virtue of their inherent contractile power, force forward their contents into the corresponding capillary veins, and thus inordinate congestion is at once relieved. But the same loss of blood which thus lowers the contractile power of the heart, exerts a similar effect on the contractility of the capillaries, and thus the power of both is temporarily weakened in a corresponding ratio. In the bronchial capillaries, too, previous inordinate distension has contributed to exhaust the vital tonicity of the vessels, from which it is evident that, on the full resumption of the heart's action, their yet remaining tonicity would be unable to withstand the pressure of the blood on their walls, which would consequently yield to the distending force, and thus the previous condition of congestion would again take place. Hence, then, the absolute necessity of exciting and maintaining, by direct application of cold air to the affected vessels themselves, the vital tonicity of the bronchial capillaries, whilst the action of the heart is reduced to the lowest degree compatible with the safety and well-being of the patient. Hence, too, in the latter

point of view, the value of antimony, ipecacuanha, digitalis, and of other depressants during this stage. With a different object, yet tending to the same general result, purgatives, diaphoretics, and diuretics are indicated, amongst which mercury, salines, jalap, scammony, antimony, Dover's powder, Jacob's powder, acetate of ammonia, the warm bath, and the spirit of nitric æther, may be specified. In the use of these different remedies, it is necessary so to administer them, that their action shall be manifested in one and the same direction,—that is, that when a purgative effect is intended, all the remedies then employed shall have that tendency; and otherwise, as the necessity of the case seems to dictate.

In the treatment of the first pathological condition of asthenic bronchitis, the depressant, evacuant, and other remedies of the sthenic variety, cannot be employed with any degree of success. The vital tonicity of the bronchial capillaries is, in a great measure, already exhausted, and therefore unable to respond immediately to the direct application of any agent calculated to excite and maintain it. Cold air, therefore, could not produce that amount of contractility in the bronchial capillaries which would enable them, after their congestion had been relieved by bleeding, evacuants, and other remedies, to withstand the force of the current of blood propelled into them, and to prevent their again being inordinately distended. Hence it does not, in this variety of bronchitis, exert that immediate and purposive effect which I have repeatedly observed it produce in the sthenic type of the disease. As such is the case, and as bleeding, evacuants, and depressing remedies generally, would only further lower the vital tonicity of the capillary walls, and of the system at large, their use cannot be indicated in the treatment of this type of bronchitis. There are, however, different shades or degrees of debility accompanying and characterising it, in some of which a partial adoption of the remedies proper for the sthenic variety may be beneficially employed. But where such is the case, where local depletion and moderate purgation are ventured upon, great care should be taken to allow a corresponding reduction of temperature in the air breathed, and to maintain, at the same time, considerable warmth of the skin. Of all the means employed, the temperature of the air breathed, and of the skin, should be the last to receive but common attention.

In cases not admitting even a partial adoption of the sthenic remedies, the indications are,—

1. To relieve the congested condition of the bronchial membrane, by derivation of blood to the skin, and occasionally to other organs, for the purpose of increasing their function.

2. To maintain the due aëration of the blood, and to give to the latter sufficient nutritive materials to support the general system, and to restore its deficient tonicity.

3. To gradually excite the vital tonicity of the capillary walls, by the constant inhalation of a cool air.

To fulfil the first of these indications, dry cupping, mustard poultices, or a terebinthinate epithem, should be applied to the chest. The extent of surface irritated should, however, be great; otherwise, the derivation of blood thereby produced would not be sufficient to relieve the pathological condition of the affected membrane. The maintenance of considerable warmth of the skin generally has already been noticed as necessary to this end. When purgatives are necessary, they should consist of those which are considered warm and somewhat stimulating, in order that the power of the system may be maintained, and the biliary and intestinal secretions excited, whilst the volume of blood is but scantily reduced. The temporary erythema thus produced in the intestinal mucous membrane, and the corresponding excitement of the kidneys under the action of diuretics, indicate the negative effect which they may be made to exert on the bronchial membrane in disease of the latter.

In carrying out the second of the above indications, a sufficient quantity of oxygen must, if possible, be admitted into the lungs, to effect the due aëration of the blood, and thereby to render it sufficiently stimulating to the different organs whose function is, to eliminate effete and morbid elements from the system, and thus to preserve or restore the integrity of the latter. But another important point indicated under this head is, to support the general system by proper food, in order that the inordinate debility which exists may be overcome, and the general power and tonicity restored.

With these means of treatment we must couple the third indication, and gradually excite the vital tonicity of the capillary walls, by the breathing of a cool air, which will now restore the proper diameter of the vessels, and thereby destroy the pathological condition of disease.

Second Pathological Condition of Bronchitis involving the Submucous Tissue.

When the bronchial membrane has assumed the second pathological condition, the particular and general phenomena arising out of it are essentially similar to those which characterise the corresponding condition of the membrane in the epithelial variety of the disease. These, therefore, do not require to be again noticed in this place; but they permit me at once to consider the pathological phenomena which are peculiar to this, as contrasted with the epithelial, variety of bronchitis.

In the latter, I have shown, from the absence of inflammatory exudation in the sputa, that there is sufficient presumptive evidence that the submucous tissue is not the seat of exudation, and that the disease consists in an excessive mucous cell-growth, coupled with epithelial desquamation; and that it is, therefore, confined to the bronchial epithelium and basement membrane. In the former variety of bronchitis, not only are these last-mentioned structures

affected, but there is invariably exudation into the submucous tissue, as an essential part of its pathological condition. This exudation takes place contemporaneously with that into the basement membrane, and in both instances it consists of the same elements of the blood,—namely, water, fibrine, albumen, and various salts. The ultimate destination, however, of the two exudations is not exactly the same, they being disposed of in the following manner.

The exudation into the submucous tissue coagulates by virtue of the fibrine which it contains; whilst the watery portion, holding in solution the different salts, is, in the generality of cases, re-absorbed into the vessels. The coagulated portion of the exudation now forms a nutritive matrix similar to the basement membrane in the healthy condition and action of that structure; and in it germinal centres arise from molecular aggregation of the fibrine, which possesses an inherent capability of being developed into cells. The necessary conditions for germination and growth being present in the heat and moisture of the part, these germinal centres begin to assimilate to themselves the intervening substance, and thereby to assume the shape and endowments of cells. As the disposition of the fibrine, during the process of coagulation of the exudation plasma, is to accumulate more or less in masses, and as each particle or molecule of such fibrine is endowed with the same inherent vitality, each of these masses must of necessity contain many germinal centres, all of which may assume an active development, and in this way produce masses of cells, as represented in the adjoining figure.

Fig. 8.

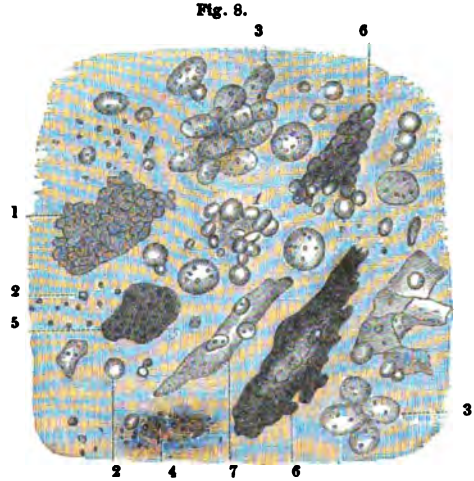

Fig. 8.—1. Exudation masses. 2. Exudation cells and molecules of fibrine. 3. Epithelial patch. 4. Superficial layer of basement membrane. 5. Entire thickness of basement membrane. 6. Mass of coagulated exudation, showing traces of cell development. 7. Bronchial cast.

Note.—The exudation-cells here figured are not of full growth; neither are many of them represented so round as they are observed in sthenic exudation.

Such structures are *invariably* present in the exudation during its early existence. They are, therefore, diagnostic of the existence of inflammatory exudation, and afford evidence, to a certain extent, of the degree of vitality which exists in the system, of which I shall hereafter speak. As these masses of coherent cells continue to grow, they assimilate and apply to themselves the nutritive matrix which binds them together; and after having thus exhausted it, they fall apart, and become distinct and isolated cells. Very many of such cells, however, do not originate in mass, but have, from the first, an isolated existence in separate and distinct molecules of fibrine. (Fig. 8, 2.)

During their growth, that portion of the nutritive matrix which cannot enter into the composition of the cell-wall passes, in a state of solution, into the cavity of the cells, which are thus kept in a state of moderate distension. When growth has reached its utmost limit, the endosmotic property of each cell permits more fluid to permeate it, and ultimately to distend it to such an extent, that the molecular attraction of its cell-wall is overcome, and disruption of the cell takes place. But this disruption does not consist in a mere rent, but rather of a molecular disintegration of the cell-wall, owing to the inordinate distension of the latter at every point of its surface, after it has ceased to be a living structure. In this way the exudation plasma undergoes a process of cell development and of cell decay in its removal from the system.

But sometimes other structures than these, to which the term pus-cell has been given, form a part of the exudation. They occur when the quantity of exudation is great, and the inflammatory action continues severe; or when a scanty exudation takes place, and continues to be influenced by a slow, persistent inflammation. They originate, like the exudation cells, in the fibrinous portion of the exuded plasma, and are in fact no other than such cells modified by the degree and persistence of the inflammation and the quantity of nutriment which is offered to them. The physical differences which they exhibit, consist in a slight difference of colour, to be afterwards explained, and in the presence of a number of granules, which are scattered over the inner surface of their walls, and which have the same chemical composition as the walls themselves. These cells generally measure from $\frac{1}{2100}$th to $\frac{1}{2600}$th of an inch in their diameter, contain from three to nine granules or nuclei, are rendered more transparent by the action of acetic acid, and are entirely destroyed by prolonged contact with liquor potassæ. Thus, in their chemical re-action, they manifest the same disposition as fibrine and albumen subjected to the same re-agents. Hence the identity of composition with these substances, in the distribution, as it were, of which the fibrine represents the cell-walls and nuclei, and the albumen the liquid contents. The conditions which determine their formation, may be thus explained. A severe inflammation affects the bronchial membrane, exudation takes place into the submucous tissue, but still the capillary engorge-

ment is maintained in all its intensity. None of the fluid exudation is therefore re-absorbed, neither is the temperature of the part reduced. The necessary stimuli of growth abundantly exist, the result of which is, that the inherent vitality of the exuded fibrine germinates, and rapidly develops cells. Owing, however, to the persistence of capillary engorgement, the nutritive elements offered to the cells are in excess of the demand which is necessary for the complete development of their walls. The overplus of fibrine, still in a state of solution, consequently passes by endosmosis into the cells, in which, from the force of homogeneous attraction between it and the cell-walls, it is precipitated in minute granules or nuclei upon the inner surface of the latter, thus constituting the plastic-corpuscles of Bennett. Such cells have now reached the utmost limit of development of which they, as isolated structures, are capable; and in this condition they either admit of being assimilated to the surrounding tissues, under the influence of the vital force which pervades the living organism, or they undergo a process of partial decay, during which they assume the particular character of pus-cells.

Now, this conversion into pus-cells is due to the action of oxygen on the structures of the plastic corpuscles, by which they undergo an *adipocerous* degeneration, similar to the well known effect produced on dead muscular tissue when exposed to moisture, and to a very partial contact with air. Hence the origin of the fat which invariably forms a chemical constituent of pus; and hence also the colour of the latter fluid as a consequence of the saponification of such fat by the alkalies present in the exudation. The oxygen necessary for this purpose is, in this particular instance, derived, at least in part, from the air taken into the lungs during respiration; but when maturation occurs at some distance from a mucous surface, or from the surface of the body, it (the oxygen) is supplied by the blood as it passes through the capillaries in the immediate neighbourhood of the affected part.

When a scanty exudation maturates, in consequence of a slow, persistent inflammation, the process is exactly similar to the one just described, except that in this instance the exudation first coagulates before cells are formed from it; whereas in copious exudation, accompanied by a continuance of capillary engorgement, cell structures, which ultimately assume the pus character, at once begin to form, without previous coagulation of the fibrinous portion of the exuded plasma. When, by exudation, the excessive engorgement of the capillaries is effectually relieved and does not return, the watery part of the exudation is re-absorbed into the vessels, whilst its fibrinous portion coagulates. This re-absorption of the fluid, and coagulation of the fibrinous, portion of the exudation, limit, to a certain extent, the conditions of germination and growth; but still, by the persistence of the capillary engorgement in a minor degree, sufficient fluid, impregnated with an excess of nutritive elements, is

supplied to the coagulated plasma to effect its gradual development into pus-cells.

It is, then, on the persistence of capillary engorgement, in a greater or less degree, after exudation has occurred, that the formation of pus-cells depends; but the actual conversion of plastic into pus-cells is, as I have before said, due to the destructive action of oxygen on the former bodies.

In the application of the above facts to the exudation of asthenic bronchitis, we should naturally expect that, as the exudation in such cases is always copious, and as capillary engorgement subsequently exists in its original intensity, thereby preventing the re-absorption of the more fluid part, and retarding or preventing complete coagulation of the exuded plasma, there would be a strong disposition in the latter to develop pus-cells. Such, indeed, we find, from actual observation, to be the case; in proof of which I may add, that in every instance of well-marked asthenic bronchitis which I have had the opportunity of examining after death, pus-cells formed a part of the exudation into the submucous tissue. Their formation in such cases depends, not only on the conditions before named,—*i. e.*, persistence of capillary engorgement after exudation has already occurred, and the action of oxygen on the plastic corpuscles, which constitute the ultimate limit of organisation of which such exudation is capable, —but also, in part, on the deficient vitality of the exuded plasma, which neither permits plastic corpuscles of full growth to arise, nor enables them, when they have attained a diminutive growth, to resist the destructive action of oxygen, and consequently their conversion into pus-cells so effectually as similar structures in a healthy exudation. Hence, therefore, the inherent disposition of imperfectly vitalised exudation to assume the distinctive character of pus-cells. Such structures generally measure from $\frac{1}{2500}$th to $\frac{1}{4000}$th of an inch in their diameter; are not so uniformly round as healthy cells, but present more or less of an irregular or angular figure; and are mingled with numerous flakes and masses of undeveloped fibrine.

Fig. 9.

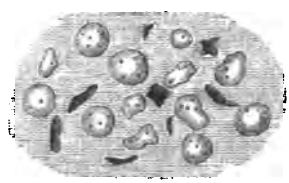

Fig. 9.—Pus-cells from imperfectly vitalised exudation.

Whenever, therefore, the characteristic structures of pus present the physical appearance just described, we acquire indisputable evidence that the fibrine, out of which they spring, was imperfectly vitalised, and that consequently the constitutional powers are defi-

cient—the deficiency being in proportion to the dimensional and figurative aberration of the cells from the standard of healthy pus.

As to the proofs that these so-called pus-cells are nothing else than plastic corpuscles in a state of adipocerous degeneration, and that the latter bodies are but the exudation-corpuscles in an advanced stage of growth, it may be observed, that the characteristic cell of inflammatory exudation, in its very first stage of development, is the exudation-corpuscle only; that subsequently, under a persistence of the inflammation, the plastic-corpuscle appears at the expense of a comparatively numerical diminution of the exudation-corpuscle; and that the plastic-corpuscle is superseded by the pus-cell, with the all but total disappearance of the exudation-corpuscle. This conversion of the one cell into the other may be watched in the fluid exudation of an abscess during its different stages of development. Again, the walls of all, and also the nuclei of both the plastic and pus cells, are similarly affected by acetic acid and liquor potassæ. Thus, by acetic acid, they are rendered more transparent; whilst by prolonged contact with liquor potassæ they are destroyed. These results quite accord with the previously described effect of such re-agents on fibrine; and they not only serve to show the chemical composition of the walls and nuclei of such cells, but also their common origin, and the ready convertibility of one cell into the other. Whatever difference is manifested between themselves, on the application of chemical re-agents, is due to the different nature of their contents, and not to the cell walls themselves. Thus the contents of the exudation-corpuscle consist of water, albumen, and the alkaline salts of the blood; of the plastic-corpuscle, the same, with fibrine in addition; of the pus-corpuscle, of all the last-mentioned substances, together with the addition of fat, the origin of which is due to the action of oxygen on the fibrine of the plastic-corpuscle, as before described. Hence the solution of the different effects of various re-agents on the above structures.

Having now traced the pathological changes which take place in the exudation poured out into the submucous tissue during the second stage of bronchitis, it is proper to consider, in the next place, the corresponding pathological condition of the epithelial surface of the bronchial membrane, and the modification produced in its secretion.

Contemporaneously with exudation into the submucous tissue, a similar exudation takes place into the basement membrane, which thus becomes surcharged with the elements of cell-growth, and which, by virtue of the excess of fluid present, furnishes a powerful condition for germination and development. The epithelium, which till now had been deprived of its due amount of nutritive fluid, in consequence of the engorged condition of the capillaries preventing the passage of even the necessary quantity, is now flooded, as it were, with such fluid, the result of which is a rapid development of mucus-corpuscles. Hence these bodies in excess appear in the first

portions of the sputa expelled; but with them we also find a great number of epithelial scales or patches, which, having been blighted during the total suspension of all natural exudation in the first pathological condition of the membrane, are now unable to assimilate the entire portion of basement membrane allotted to them, and are consequently cast off, together with such portions of membrane, in the process of decay and repair. There are, therefore, one portion of the affected membrane denuded of its epithelium, and another portion of it with epithelium undergoing a rapid development into mucus-cells. Hence a difference in the vascular shading of the affected membrane, which is even detectable by the unaided sight, but which is rendered very apparent by the microscope. Amongst the epithelial scales and patches thus shed, there are others, which consist of the most superficial layer of the basement membrane enclosing nuclei, and sometimes of the whole thickness of that structure itself.

They measure, in general, from $\frac{1}{800}$th to $\frac{1}{1700}$th of an inch in their longest diameter, and are distinguished from each other by their relative degree of transparency; those patches of basement membrane which consist of its superficial layer only being much lighter and more transparent than those composed of its whole thickness. (Fig. 8, 4.)

Those portions of the membrane which have lost their epithelium in patches, and also those parts of it in which the basement membrane is almost or entirely cast off, afford sufficient outlets for the fluid exudation beneath, which now transudes through the basement membrane, fills the vacuities occasioned by its superficial or entire destruction, coagulates upon the denuded surfaces left by the shedding of the epithelium, and thus acquires a *locus standi*, and the necessary conditions for its germination and growth. Cell-growth, therefore, immediately commences, and results in the partial development of masses of exudation-corpuscles, which, however, are frequently detached by the effort of coughing, and consequently do not attain mature development. (Fig. 8, 1.)

These structures supersede the epithelial and basement patches, which disappear from the sputa shortly after expectoration has been established. If they are not detached by cough, or any other accidental circumstance at this stage of growth, they reach their full development as exudation-corpuscles; after which, if the elements of nutrition continue to be supplied to them, they (the elements) are precipitated as granules on the inner surface of their walls, and thus the modified exudation-corpuscles become plastic-cells. They have now reached their utmost limit of growth as living structures; and, in obedience to the universal law of change which presides over every living structure, they must either enter into structural relation with the containing tissue, or perish, become subject to chemical laws which govern inanimate matter, and be cast off as dead and waste material. In the last-mentioned condition they

assume, in the way before described, the character of pus cells, and as such they now appear in the sputa.

Fig. 10.

Fig. 10—1. Exudation-corpuscle of full growth.
2. Exudation mass, showing the development of one of its cells into a plastic-corpuscle.
3. Plastic-corpuscle of full growth.
4. Pus-corpuscle of full growth.

It has been observed, that before either exudation, plastic, or pus cell appears in the sputa, the latter have contained an unusual quantity of epithelial and basement patches, which leave the membrane denuded at certain points. If, in this condition, the membrane be carefully dissected away and examined by the microscope, it will be found that these denuded parts are covered by coagulated exudation, whilst the surrounding portions of the epithelium are quite free. It is, therefore, evident that these points are the only outlet for the exudation, and that epithelial desquamation is an essentially preparatory step to the formation of either exudation, plastic, or pus, cell, on a mucous surface. Hence the doctrine, that pus can be formed on the free surfaces of mucous membranes, *without there being any breach of structure*, is opposed to microscopic observation, which teaches—

That every case of pus from mucous surfaces is invariably preceded by epithelial desquamation, and not unfrequently by ulceration of the basement membrane itself.

In addition to the structures already described as products of inflammation appearing on the free surface of the bronchial membrane, other bodies, which present neither definite figure nor structure, are found in the discharged sputa. They are, in general, too opaque to transmit the light; but occasionally, here and there, are spots, through which light can pass, and from which we discover that such bodies contain cell-germs, as shown in Fig. 8, 6.

By prolonged contact with liquor potassæ, the whole of these bodies is destroyed. Their chemical composition, therefore, seems to indicate their fibro-albuminous nature; whilst the explanation of their origin is, that after epithelial desquamation has taken place, exudation occurs upon the denuded surface of the basement membrane, that it coagulates, and that it is subsequently expelled by cough before it has had time to undergo cell-development. Occasionally, in bronchitis affecting tubes from the third gradation in size downwards, the exudation poured out is so copious that, on coagulating, it forms a perfect cast of the bronchus, in the same way as renal

casts are formed in the uriniferous tubes of the kidney.[1] I have, however, observed, that such bronchial casts are more frequent in subacute and semichronic, than in acute attacks of bronchitis.

They often occupy their position for some time, owing to their glutinous nature, to the inability of muscular effort to dislodge them by cough, and, in some instances, to a probably too deficient sensibility in the affected membrane to take cognisance of their presence. They resemble dirty-white or yellowish-white gelatinous, worm-like bands, which measure from $\frac{1}{200}$th to $\frac{1}{2000}$th of an inch in their longest diameter, and which include granules, exudation and plastic corpuscles, together with an occasional admixture of pus-cells. (Fig. 8, 7.)

By so obstructing the bronchi that the air cannot enter the pulmonary cells to which the former lead, they give rise to pulmonary collapse, so ably pointed out by Dr William Gairdner, to whom pathological science is greatly indebted for several important advances connected with this and other subjects. Whilst, therefore, I refer all those who are interested in the subject of pulmonary collapse to his published work, "On the Pathological Anatomy of Bronchitis," I have pleasure in confirming the majority of his positions, by reference, not only to the human subject, but also to the inferior animals, and particularly to the ruminantia, rodentia, and digitigrade carnivora. During the present year (1852), I have examined the diseased lungs of eighteen animals, belonging to the above-mentioned classes, in fourteen of which I found certain bronchi obstructed with inflammatory exudation, together with collapse of the corresponding portions of lung. In eleven of these cases the collapsed portions of lung corresponded to the posterior and inferior lobes of the human lung, and were associated with different degrees of emphysema in the unobstructed portions of pulmonary tissue. In most of these cases the tissue of the collapsed lung was of a deep lake-red

[1] I may here remark, in reference to uriniferous casts in acute and chronic desquamative nephritis, that, according to my observations and investigations, hereafter to be published, the conditions of their formation are exactly similar to those which determine the formation of bronchial casts,—namely, 1. Vascular engorgement of the capillaries surrounding the uriniferous tubes, arresting for a time the natural transudation of fluid into the basement membrane, with which they lie in contact; 2. Exhaustion of the vital tonicity of these vessels, exudation as a consequence, and desquamation of those portions of epithelium which have perished during the stage of congestion, and the consequent suspension of all natural transudation; 3. The escape from the denuded surfaces of the basement membrane of an exudation, which, when copious, fills the corresponding uriniferous tubes, coagulates, and thus forms a cast, but which, when scanty, remains for some time attached to the basement membrane, on which it coagulates, germinates, and undergoes the same process of cell-development as characterises the products of bronchial inflammation. I therefore agree with Dr Christison, that the presence of uriniferous casts in the urine is pathognomonic of renal inflammation; whilst, with respect to their origin, I am compelled to differ from Dr Johnson, who refers them to transudation of fibrine through the walls of the congested Malpighian capillaries.

colour; and in all it manifested the firmness and consistence of liver. When incised, the cut surface presented a smooth, non-granular appearance, thus showing that the consolidation was not owing to inflammatory deposit. A fine horizontal section presented semi-dark linear shadings, from the falling together of opposite cell walls, whilst the fibrous tissue of the cells had evidently undergone a degree of atrophy, and the capillaries a certain amount of thickening, probably as the result of an unusual degree of contraction of their walls, induced by the pressure of surrounding collapsed pulmonary tissue.

The mean of sixteen analyses of the sputa of acute sthenic bronchitis gave, of—

	In 100 parts.
Water, ...	96·75
Organic matter, ...	2·15
Alkaline sulphates, „ phosphates, Chlorides of sodium and potassium, Sulphate and phosphate of lime, ...	1·10
	100·00

On comparing the above analysis with that of the healthy secretion of the bronchial membrane, it will be found that there is a considerable increase of solids in the sputa of sthenic bronchitis. In the asthenic type of the disease the difference is not so great, the average increase of solid constituents being, according to the mean of six analyses, from 1 to 1½ grains in 100.

To determine how far the organic matter agreed with fibrine and albumen under the action of acetic acid and liquor potassæ, as before detailed, a quantity of mucus was evaporated to dryness over a sand-bath, after which the solid residue was triturated in a mortar, and then digested in warm distilled water for some time. The whole was afterwards poured on a filter, and the fluid portion, holding in solution the soluble salts, passed through. The residue left on the filter was collected, dried, and divided into three portions. One portion was digested in cold acetic acid, a second in liquor potassæ, whilst a third was boiled with the acid. The first portion increased somewhat in bulk, became slightly more transparent, but was not in the least degree dissolved at the end of seventy-two hours; the second dissolved at first rather rapidly, afterwards more slowly, and imparted to the liquor potassæ the well-marked tinge of a weak solution of burnt sugar; whilst the third portion, in by far its greater part, underwent solution in forty minutes.

The result of these experiments seems to leave no doubt as to the chemical composition of the organic matter, which in acute sthenic bronchitis bears the proportion of upwards of 3 to 1 as compared with the organic matter of healthy mucus; whilst the salts of the latter are as 1 to 10 of the former. If, again, we compare the relative proportion of salts to organic matter in both healthy mucus

and the sputa of acute sthenic bronchitis, we find that in the former the salts are to the organic matter as 1 to 5·66; in the latter as 1·10 to 2·15. It is, therefore, evident that inflammation of the bronchial membrane produces a greater relative increase of salts than of organic matter in the sputa, the reason of which appears to be the following:—

After the coagulable portion of the exuded plasma has solidified on the denuded surfaces of the basement membrane, a continual supply of alkaline fluid from the blood is required to effect its resolution, before it can be assimilated in the growth and development of cells.

This supply of alkaline fluid regularly taking place for some time after all inflammatory exudation has ceased, must necessarily lead to a relative increase of salts, as compared with the organic matter of the sputa, which, according to the analyses before given, is actually the case.

If we take into consideration the absolute increase of organic matter and salts, as well as the greatly increased quantity of sputa, in acute sthenic bronchitis, we can appreciate the drain which is continually being made upon the blood by this cause alone; and if we further regard the waste produced by all the other secretions, and occasionally by the effects of medicines, as well as the negative result of almost total abstinence from food, we can readily understand how and why the bulk and weight of the body rapidly decrease in disease.

Looking at the pathological condition of the bronchial membrane in the second stage of bronchitis involving the submucous tissue, it may now be asked,—What are the indications of treatment which this condition affords? The vital tonicity of the capillaries is exhausted; exudation has occurred into the submucous tissue, as well as into and upon the surface of the basement membrane; and the free surface of the latter structure is in part denuded of its epithelium.

The condition of the membrane itself is very similar to that which accompanies the corresponding stage of the epithelial variety of the disease; therefore the treatment must be based upon the same principles. But there are those differences in the pathology of the two varieties of bronchitis already described, which must necessarily modify, to a certain extent, the treatment. In the more severe variety there is exudation into the submucous tissue, as well as upon the denuded epithelial surface of the basement membrane; in the epithelial variety neither of these conditions occurs. Again, bronchial obstruction, from inflammatory exudation into the tubes, is apt to occur in the more severe kinds of bronchitis; but in the epithelial variety such is never, or very rarely indeed, the case. These differences must necessarily modify the indications of treatment; but, in the main, they are the same as in epithelial bronchitis.

To procure the removal of the exudation from the submucous tissue, its tendency to cell-development must be favoured, and its subsequent disintegration procured. Heat and moisture—the natural stimuli to cell-growth—are already present in the affected part; but the liquefaction of the coagulated portion of the exudation, and the subsequent molecular disintegration of the resulting cells, will be greatly facilitated by the exhibition of the alkalies. Were it possible to exhibit these in large quantities, without injury to the red globules of the blood, and to the fibro-albuminous structures, by doing so the exudation might readily be disintegrated, and its absorption procured, without its undergoing the process of cell-development. For the removal of the exudation from the epithelial surface of the basement membrane, for the liquefaction of inspissated mucus, and for mucus generally, and also for the disintegration of bronchial casts, the alkalies are by far the most efficient. But in the case of inspissated mucus and of bronchial casts, so great may be the extent of bronchial obstruction and of pulmonary collapse produced by them, that the consequent insufficient aëration of the blood may require that the breathing power of the lungs shall be immediately improved. Sufficient time, therefore, cannot be given to the alkalies to effect the gradual disintegration of the causes of obstruction through the route of the circulation, in which case the use of emetics is indicated, for the purpose of effecting a mechanical dislodgement. But it must be borne in mind that some emetics, as antimony, are much more prostrating in their effects then others; and that the condition of the system induced by their exhibition would be accompanied by a corresponding diminution in the vital tonicity of the bronchial capillaries, which would again favour congestion and exudation, and thereby render the beneficial effects of such emetics very temporary indeed. Hence the importance of exhibiting emetics which produce the least depressing effects on the system, amongst which the sulphate of zinc, a warm solution of common salt, and mustard, deserve to be specified.

Sometimes, however, expectoration fails in consequence of sheer debility; and it is in these asthenic cases that expectorants, strictly so called, are indicated. All real expectorants (amongst which I do not include antimony, mercury, ipecacuanha, and the alkalies) exert more or less of a stimulating effect on the bronchial fibre and capillary circulation, which degree of stimulation is unnecessary and even injurious in sthenic bronchitis, or in cases in which sufficient power remains to dislodge the mucus by an ordinary degree of cough. If, by such an effort, the sputa are not dislodged, it is certain that the cause is owing to the tenacity of the sputa themselves, for which the appropriate remedy is not an expectorant, but a liquefacient, as potash, soda, or a mercurial preparation. But when manifest debility is present, and when expectoration fails as a consequence of such debility, expectorants are clearly indicated, and of

these senega, squill, ammoniacum, and the different balsams and oleo-resins are in vogue. The pathological condition of the membrane, however, indicates, in addition, the necessity for the exhibition of a generous diet, in order that the vital force of the system may be increased, the power of expectoration maintained, and a reparative process instituted and sustained by the affected membrane.

Other important indications, alike applicable to this and epithelial bronchitis, are expressed in the treatment of the second pathological stage of the latter variety, to which the reader is referred. Other, yet minor, indications will in most cases arise out of the urgency of individual symptoms, as cough, all of which should receive the judicious attention of the physician.

(To be continued.)

ARTICLE II.—*Report of an Attack of Colic, which occurred as an Epidemic, among the troops at Newera-Ellia, Ceylon, in the commencement of* 1852. Communicated to the Superintendent of the Army Medical Department, by Assistant-Surgeon ALEXANDER SMITH, M.D., Her Majesty's 37th Regiment.

SIR,—In reply to your letter of the 16th November 1852, I beg to forward, for your information, the following notice of an epidemic of colic, which prevailed among the troops at Newera-Ellia, in the island of Ceylon, between the 12th February and the 20th June 1852.

The period, during which I was in medical charge of the troops at the above station, extended only from the 5th May to the 31st July of the present year, but included the period in which the epidemic prevailed with greatest severity; and, previously to the 5th May, I had an opportunity, at Kandy, of witnessing several of the more severe cases sent from Newera-Ellia to the Head-Quarter Hospital. From the observations I was then able to make, as well as from subsequent examination of the Medical Records, I have every reason to believe that the epidemic maintained the same character throughout; so that, without impropriety, the following may be considered a description of it in its whole course.

Newera-Ellia is situated in the interior of the island, within about six and a half degrees of the equator, at an elevation of 6000 feet; and, from the latter circumstance, it has a mean annual temperature under 60°. It consists of a small native village, the barrack, and European residences—all widely scattered over a space of ground several miles in extent, and which may be described as an irregular table-land, intersected by water-courses. The

more elevated parts are wooded, whilst the lower are covered with a coarse prairie-grass, and an occasional tree or bush of the Rhododendron species. The soil, where free from jungle, is of a very dark colour, and bears much the character of an inferior variety of peat, and, excepting in the hollows and by the streams, is rendered exceedingly dry by resting immediately on a layer of fine gravel, a few inches in depth, which acts the part of a universal drain.

From April to October, during the prevalence of the south-west monsoon, there are high winds and almost constant rain; but during the remainder of the year, the weather is dry and very enjoyable, and Newera-Ellia becomes the resort of all the European inhabitants who can possibly find means to leave the lower and more warm parts of the island.

During the time the epidemic lasted, the weather did not appear to present any character differing from that of other years at the same season, which included parts of both the wet and dry monsoons.

From the 12th February to the 5th June—at which latter date their numbers were reduced by thirty-two of the sick being sent to Kandy—the troops at the station consisted of a company of H. M. 37th Regiment, together with a few sickly men from head-quarters, and numbered, in all, about 100 men. Their duties were light, and consisted of the usual barrack guard and parades, a small night guard on the treasury, and an occasional escort. A few of the men were also employed as artificers in the engineer department, for which duty there was an extra allowance of pay. Their leisure hours were chiefly employed in the cultivation of vegetable gardens, in which they raised large quantities of cabbages and potatoes, and in rearing pigs and poultry.

The barracks are built on a dry bank of considerable elevation, round the base of which a rapid stream of very pure water runs over a pebbly bottom. They are commodious, clean, and well drained, and are not provided with cistern or other means for collecting rain-water. The whole of the water used for drink, cooking, etc., is drawn from the stream already mentioned, in unpainted wooden buckets or tubs, and the cooking utensils are of untinned iron.

The rations of the troops consist of a coarse variety of brown bread, and, for that country, remarkably good beef; for the supply of both which articles the commissariat department receives tenders from the trades-people of the place. A ration of English salted pork is also occasionally issued from the stores of the same department; from which also each man daily receives an allowance of arrack. In lieu of the latter, when not drawn, there is made a small allowance in money; a privilege often claimed at other stations in the island; but in Newera-Ellia, probably from the mild temperature, the arrack allowance is in almost every instance drawn

in kind. The other articles of diet, consisting of coffee, sugar, potatoes, potherbs, and salt, are daily purchased by a "marketing party," at whichever of the village stores the corporal in charge may consider he can best do so with advantage to the company; and I have ascertained, that during the whole of the period embraced in this report, the sugar was supplied by one store, and that it was throughout part of the same stock.

Until the commencement of the present year, Newera-Ellia was considered the most healthy spot in Ceylon; the troops returning from a residence there having always presented a striking contrast in complexion and general condition to those stationed at any other part of the island; and the duties of its medical officer had always been exceedingly light.

On my assuming charge of the detachment, the cases of colic under treatment in hospital—which was calculated to contain only twenty patients—and in barrack, where the remainder lay, numbered twenty-six; and, on medical inspection of the remainder of the detachment, I found the general appearance of the men very unhealthy, and that, with scarcely an exception, they presented on the gums, at their junction with the teeth, a distinct line of a grayish-black colour, the mark varying from the slightest trace on the gums of the second molar tooth of the upper jaw, where it could invariably be first detected, to a fringe of nearly one-eight of an inch in depth, extending completely round the gums of both jaws, and seen, although less marked, as well on the inner as on the outer surfaces of the teeth. When seen in the highest degree, it was very often accompanied by dark-gray coloured patches of irregular form, in the mucous membrane of the lips, at the points in contact with the discoloured part of the gum; and that appearance was not in any way connected with either the number of blood-vessels, or the quantity of blood in the part. In the most marked cases, the gums were spongy, and appeared, where they touched the teeth, as if ulcerated; and, in such cases, a disagreeable taste was complained of, and likened to that of "rotten wood;" and this in cases where mercury had not been administered.

The symptoms of the complaint, and their order of occurrence, were in general nearly as follows:—About a month before admission into hospital, the appetite became impaired, and there were frequent headaches, with pains referred to between the shoulders, loins, and lower limbs, but especially to the insides of the thighs, and to the knees. The bowels then became irregular, generally tending to constipation, although in a few cases there was diarrhœa. There were occasional twisting pains in the abdomen, and the evacuations, when observed, were always of a very dark colour. At length the bowels became completely constipated,—an occurrence always accompanied by excruciating abdominal pain of a twisting character, and chiefly referred to the regions of the umbilicus and hypogastrium, and more rarely to the epigastrium. The patient

was compelled to seek relief from his agony in constant change of posture; but the positions which seemed to bring most ease were, either lying on the face, or sitting up in bed, and clasping the knees with both arms, so as to press the abdomen against them. The pain was increased by sudden and partial, but much relieved by steady and uniform pressure, and was in the majority of cases attended with great retraction of the abdominal parietes, the muscles of which were contracted and felt very hard. In a few cases, however, the abdomen was much distended, and in these the escape of flatus afforded marked relief. There was, at the outset, almost constantly more or less vomiting, generally of matters tinged with bile, but never of stercoraceous matter; and there was much anxiety of countenance, with great irritability of manner. The face was generally flushed, and the skin hot and dry, and emitting a peculiar earthy odour. The tongue, towards its base, was much loaded with a creamy fur; but there was not much thirst. The urine was scanty and high-coloured. The pulse, during the violence of the abdominal pain, was reduced in frequency—in general to about 60—and was rather hard; but, as the pain abated, it rose to between 80 and 90, and became more soft; and in some cases it attained a considerably greater frequency. The nights were invariably sleepless, and, even after the abdominal pain had nearly ceased, there was an almost constant complaint of watchfulness from the intolerable gnawing pains experienced in the limbs, and especially in the knees, which, however, showed no signs of local affection. Under treatment, the abdominal pain generally began to abate on about the third day; but it was not until the fifth or sixth, and in many cases not until considerably later, that there was an evacuation from the bowels. The evacuations were always of a very dark colour, and were at first much indurated. On the bowels once acting freely, the abdominal pain, as a general rule, ceased, but the appetite remained impaired, and the strength much reduced,—the patient continuing to suffer from pains in the back and limbs for a considerable time after all other symptoms, with the exception of the mark in the gums, had disappeared, and he had returned to duty, after being about a fortnight in the hospital.

The disease showed a great tendency to relapse in the course of a few weeks, and a return of its symptoms was generally observed to follow either exposure to wet, or a night guard. In a few cases, there were as many as four attacks in less than as many months; and in all these the emaciation and reduction of strength became extreme. In four men who had been thus frequently attacked, and who had been among the first to suffer, there was an almost complete paralysis of the arm, but especially of the muscles of the forearm, which made its appearance gradually, and first attracted notice by the growing inability to button the clothes,—a loss of power which, at length, advanced so far as to render it impossible to raise

the arm from the side, or, unassisted, to perform the most trifling office. When, with assistance, the arm was raised and held out, the wrist "dropped;" but, although the extensor muscles seemed most affected, so little power remained in the flexors, that, on requesting the patients to grasp the hand, scarcely any pressure could be felt. In all the cases under treatment, the dark mark in the gums was to a greater or less extent visible. From the time of admission it did not appear to increase; and although it never entirely disappeared, yet in some cases, under treatment, it seemed to undergo a marked diminution.

Between the 5th May and the close of the epidemic, five cases proved fatal. In the first death was sudden, from the bursting of a vomica, the disease having supervened in the last stage of phthisis pulmonalis. The symptoms of colic had been well marked, and the gums presented the dark markings; but before death the bowels, which had been much constipated, acted regularly. On examination, however, the ileum presented, in two portions, well marked intussusceptions; and in one or two other positions, for the space of three or four inches, it was contracted to nearly the size of a large goose quill, the contracted portions being of a deeper colour than that presented by the rest of the intestine. There was no ulceration of the bowel, and beyond evidence in the lungs of advanced phthisis, there was no other unusual appearance. In two of the fatal cases there had been before death paralysis of the fore-arms, and in all, with the exception of the first already mentioned, the mode of termination was the same—death by coma supervening in a sudden attack of violent convulsions, which bore much the character of epilepsy, and which continued to recur, at short intervals, until the patients sank into profound coma, and at length expired. In all these cases, there was considerable effusion of serum into the sub-arachnoid spaces and ventricles of the brain; and the inner layer of the arachnoid membrane, on the upper surface of the hemispheres, to a considerable distance on either side the longitudinal fissure, and as far forward as the anterior lobes, was covered by what appeared to be a very remarkable development of the glandulæ pacchioni, which, in many positions, were placed so closely to each other as to resemble a continuous granular false membrane. The gums, in all, presented a broad dark line, and in one the mucous membrane of the lips was stained. The colour, which in life had been a grayish black, had after death, from the contrasting blanched surrounding tissues, a bluish tint. On removing a portion of the gum, the seat of colour was found to be close to the surface of the tooth, where the dark matter appeared to be deposited in the gum in a fringe of minute radiating lines, in their direction corresponding, or nearly so, to the long axis of the tooth.

In all the cases, the mucous membrane of the stomach, at its large extremity, presented a diffused patch of a dark gray colour,

and close inspection showed it to depend on a deposit of innumerable small dark points, which, in colour, resembled the lines seen in the gums; and in two of the men, who had been during their lives addicted to hard drinking, and who, before death, had suffered considerably from epigastric tenderness, there was, near the pyloric orifice, a small oval ulcer, the surface of which was in both cases of an intensely black colour. The portions of the liver and spleen in contact with the stomach, presented surfaces the colour of which was much darker than usual, and contrasted strongly with the other portions of these organs. In all, there was more or less contraction at different points in the ileum, and in one there was a slight intussusception—so slight, however, that it became unfolded in the course of careful handling. Similar appearances could with the greatest ease be produced by gently pushing the natural portion of bowel forward upon the contraction, or by pressing the latter into the more natural portion beyond. In one instance, where there was no intussusception, the contraction was so complete as to require slight pressure to be made on the upper portion of bowel, in order to cause the contained gas to pass the narrow part, which it did with a slight explosion. At all the points of contraction, the bowel was of a slightly pink hue, and contrasted strongly with the more natural contiguous tissues. The corresponding mucous surfaces presented no unusual appearance, beyond an increase of colour corresponding to that seen on the peritoneal surface.

The colon contained a considerable quantity of very dark-coloured fæces, and in nearly all, presented several points near the sigmoid flexure, where the caliber seemed much smaller than is usually seen in examinations after death.

In all the cases, the mucous membrane of the caput cœcum was found, after careful washing, to be nearly black—the colour gradually disappearing in the mucous membrane of the ascending portion, into which it did not extend beyond a few inches. The colour arose from a deposit, in the mucous membrane, of small dark gray points, of a character similar to those seen in the stomach, but differing in degree, being more marked than in the latter situation. With the exception of a partially adherent pericardium, which one of the cases presented, and which was not the result of recent disease, no other unusual appearance could be detected.

As the symptoms above enumerated appeared to bear an exact resemblance to those described as proceeding from poisoning by some salt of lead, and did not approach in character to those of any other disease of which I had ever read, the treatment adopted was in accordance with the belief that they arose from that cause. On the supposition, therefore, that the epidemic arose from some article of the food or drink of the troops containing a salt of lead, sulphate of magne-

sia, acidulated with sulphuric acid, was daily administered to each individual in barracks, and, although the adoption of that plan did not arrest the disease, yet it was evident that soon after it was commenced the attacks became much less violent in character.

In the treatment of an attack in hospital, various plans were at first adopted; but experience soon indicated a few remedies, to which all others gave place. Of these, the following were among the most efficacious:—

Large enemata of water, as hot as could be borne, were introduced by means of the œsophagus tube of the stomach-pump passed as far as possible, and forced up until the patient could no longer bear the distention,—which was, in general, when a washhand basinful had been injected. Within the first few days, no approach to an evacuation was ever produced by these means, but the relief following its use was often very marked, and in cases where there was much distention from flatus, the temporary ease it afforded was striking.

Alum in half-drachm doses, combined with opium in large quantity, and given three times a-day, proved a valuable remedy, and together with hot water enemata and the early application to the abdomen of a large blister, generally within a couple of days rendered the pain endurable. Both blood-letting and leeching were tried at the commencement, and certainly afforded much relief, but it was found that whilst these proceedings much reduced the strength, the same good effects followed the application of a blister, without the resulting depression, an occurrence if possible to be avoided, when the liability to frequent relapses was taken into consideration.

When the above course had been followed for a few days, the abdominal pain very much abated; and at length, in from five to eight days, the bowels acted slightly—a result soon followed by comparative freedom from pain, so far as the abdomen was concerned; but an occurrence which seemed to be rather retarded by the administration of purgatives in any form. When there had once been a slight evacuation, castor oil, guarded by a small quantity of tincture of opium, generally caused the bowels to be several times moved, and from that time recovery commenced. More active purging seemed to do harm.

When the bowels had again acted and the pain abated, the opium was omitted; but the alum, given in infusion of calumba, and where the patient had previously suffered from remittent fever, with the addition of sulphate of quinine, was continued for a few more days.

In order to procure sleep and allay the intense aching pains of the back and limbs, which, after the abdominal pain had ceased, continued for some time to cause much suffering, large doses of morphia were found necessary.

Calomel and opium were found to be remedies of much value in

removing pain; but the system appeared to be peculiarly liable to the occurrence of severe mercurial action; and it was remarked, that patients so treated, although the first amendment was rapid, ultimately recovered slowly, and long remained sallow and emaciated.

In order to ascertain if any article of food or drink supplied to the troops contained traces of lead, I examined specimens of everything used by them; but only in arrack and sugar could I detect any appearance calculated to excite a suspicion of the presence of that metal.

On passing a stream of sulphuretted hydrogen through arrack contained in a tube about one inch in diameter by about four in length, a slight brownish black discoloration was instantly produced, and was very apparent when the liquid was examined through the long axis of the tube. On one occasion, after a few minutes, the precipitate was deposited on the sides of the tube in the form of a dark waving line, and on another I succeeded in collecting a small quantity on a filter; but, from want of the necessary apparatus, its examination could not be pushed to the necessary extent. On dropping a small crystal of iodide of potassium into a wine glassful of arrack, or adding a strong solution of the same salt, and allowing it to stand for a few minutes, a decidedly yellow colour was produced; and the same result followed after the slightly acid re-action of the arrack had been removed.

As it only occurred to me to treat the sugar in simple solution in water, which remained unchanged when submitted to sulphuretted hydrogen and hydro-sulphate of ammonia, and as filtering did not render the solution colourless, neither the effects of sulphuretted hydrogen, nor of iodide of potassium, could be judged of; but on adding hydro-sulphate of ammonia, and gently agitating the tube, a dark brown cloud formed at the junction of the test with the solution, and gradually increased as the hydro-sulphate descended the tube. On submitting to similar treatment a specimen of English loaf sugar, and a finer variety of brown sugar used in the family of one of the European residents which escaped the epidemic, I failed to detect any appearance of a precipitate.

From want of sufficient apparatus, I was unable to attempt carrying the examination of the suspected articles to that extent which would have removed all doubt; but feeling satisfied that the appearances seen indicated the presence of one or more of a few metals among which lead is classed, and the symptoms and history of the epidemic having indicated poisoning by lead, I felt justified in believing, that there existed more than a probability that the precipitates arose from the presence of lead in either the arrack or sugar, or in both.

From my statements on the subject, the issue of arrack was dis-

continued, and a native sugar called jaggery, obtained from the juice of the cocoa-nut tree, was substituted for that previously used by the troops. The issue of arrack ceased on the 28th May, and the suspected sugar was discontinued two days later. On the 10th June the last case of colic was admitted into hospital, and on the 20th of the same month there was no longer any case of that disease under treatment. Within a week of the date on which sugar and arrack ceased to be used, the appearance of the troops began to improve; and from the 10th June until the 31st July, when I left the station, there had been no return of the complaint, neither had there been any admissions into hospital for other diseases, with the exception of one or two cases of chronic ophthalmia, sent from head-quarters for the benefit of change. And when, on the day previous to my leaving the station, I inspected the detachment, I found that the men had regained much of their former robust and healthy appearance. The dark line, however, still remained visible in the gums—faintly in many, but in all sufficiently well marked to be easily recognised.

Whilst the epidemic prevailed among the men in barracks, the women and children suffered in a like degree, and two officers, whose residences were distant about a quarter of a mile from barracks, were also attacked; whilst, at a distance of more than a mile from the same place, individuals in two families were similarly affected. In all these cases I ascertained, that sugar from the same stock which supplied that used by the troops, had been used in some part of the food; but I could not discover that, among the natives who use jaggery alone, any case of colic had ever occurred.

I was also able to learn that, in the other resident families who entirely escaped the complaint, the sugar used had been procured from quite a different source, and had in appearance nothing to identify it with that on which my suspicions rested.

It may be interesting to mention, as perhaps tending to throw some light on the origin of the dark line in the gums, that, in the only three cases under my notice, in which the tooth-brush was regularly used, there was no trace of a dark mark in the gums, although the symptoms of colic could not be mistaken; and that I fancied I could trace a distinct relation between the quantity of "tartar" on the teeth, and the intensity of the marking in the gums.

The accompanying return is intended to show the numbers of admissions and discharges for colic, during the prevalence of the epidemic; but the column of deaths is inaccurate, as five cases terminated fatally between the 5th May and 20th June, and there was one death in April.—I have the honour to be, etc. etc.

Return of admissions and discharges for Colic in the Detachment Hospital, H. M. 37th Regiment at Newera-Ellia, from the 12th February to the 20th June 1852.

Strength.	Date.	Admitted.	Discharged.	Died.	Remained in Hospital	Remarks.
87	February 12	1	...	...	1	
...	... 13	...	...	...	1	
...	... 14	1	...	...	2	
...	... 15	...	...	...	2	
...	... 16	...	1	...	1	
...	... 17	...	...	...	1	
92	... 18	...	...	...	1	
...	... 19	...	1	...	...	
...	... 20	...	...	...	...	
...	... 21	1	...	...	1	
...	... 22	...	...	...	1	
...	... 23	1	...	...	2	
97	... 24	...	1	...	1	
...	... 25	...	...	...	1	
...	... 26	1	...	...	2	
...	... 27	1	...	...	3	
...	... 28	1	...	...	4	
...	... 29	...	1	...	3	
...	March 1	...	...	...	3	
...	... 2	1	...	...	4	
...	... 3	...	...	...	4	
...	... 4	...	2	...	2	
...	... 5	2	...	...	4	
...	... 6	...	...	...	4	
...	... 7	...	...	...	4	
...	... 8	1	...	...	5	
...	... 9	...	...	...	5	
...	... 10	...	5	...	...	
...	... 11	...	...	...	...	
...	... 12	...	...	...	...	
...	... 13	...	...	...	...	
...	... 14	1	...	...	1	
92	... 15	...	...	...	1	
...	... 16	1	...	...	2	
...	... 17	1	1	...	2	
...	... 18	...	...	...	2	
...	... 19	...	...	...	2	
...	... 20	1	...	...	3	
...	... 21	...	...	...	3	
...	... 22	1	...	...	4	
...	... 23	1	...	...	5	
...	... 24	1	...	...	6	
...	... 25	...	...	...	6	
...	... 26	...	...	...	6	
...	... 27	...	2	...	4	
...	... 28	1	1	...	4	
...	... 29	1	...	...	5	
...	... 30	2	...	...	7	

Return of admission, etc.—continued.

Strength.	Date.		Admitted.	Discharged.	Died.	Remained in Hospital.	Remarks.
92	March	31	...	1	...	6	
...	April	1	1	...	...	7	
...	...	2	...	...	...	7	
...	...	3	...	2	...	5	
...	...	4	3	...	...	8	
...	...	5	1	3	...	6	
..	...	6	...	...	...	6	
...	...	7	...	...	...	6	
...	...	8	...	...	...	6	
...	...	9	1	2	...	5	
...	...	10	1	2	...	4	
...	...	11	...	...	...	4	
...	...	12	1	...	...	5	
...	...	13	1	1	...	5	
...	...	14	1	...	...	6	
...	...	15	...	1	...	5	
...	...	16	3	...	...	8	
...	...	17	5	...	...	13	
...	...	18	2	...	...	15	
...	...	19	2	3	...	14	
...	...	20	1	1	...	14	
...	...	21	4	5	1	12	
...	...	22	4	...	...	16	
...	...	23	2	4	...	14	
96	...	24	...	3	...	11	
...	...	25	...	2	...	9	
...	...	26	...	3	...	6	
...	...	27	...	...	...	6	
...	...	28	3	...	...	9	
...	...	29	...	3	...	6	
95	...	30	...	...	...	6	
...	May	1	4	...	...	10	
...	...	2	6	...	...	16	
...	...	3	4	...	...	20	
...	...	4	4	1	...	23	
...	...	5	3	...	...	26	
...	...	6	1	1	...	26	
...	...	7	1	...	...	27	
...	...	8	...	...	...	27	
...	...	9	2	...	...		
...	...	10	...	...	...	29	
...	...	11	3	2	...	30	
...	...	12	3	2	...	31	
...	...	13	1	1	...	31	
...	...	14	...	5	...	26	
...	...	15	3	3	...	26	
92	...	16	1	3	...	24	
...	...	17	...	2	...	22	
...	...	18	3	2	...	23	
...	...	19	1	...	...	24	
...	...	20	2	2	1	23	

Return of admission, etc.—continued.

Strength.	Date.		Admitted.	Discharged.	Died.	Remained in Hospital.	Remarks.
92	May	21	...	1	...	22	
...	...	22	2	...	1	23	
88	...	23	3	...	...	26	
...	...	24	...	...	...	26	
...	...	25	1	1	...	26	
...	...	26	9	...	...	35	
...	...	27	12	...	...	47	
82	...	28	1	...	1	47	
...	...	29	4	...	...	51	
...	...	30	4	3	...	52	
...	...	31	2	...	...	54	
...	June	1	...	8	...	46	
...	...	2	...	...	...	46	
...	...	3	...	3	...	43	
...	...	4	...	...	...	43	
...	...	5	...	32	...	11	32 sick sent to head-quarters at Kandy.
56	...	6	...	8	...	3	
...	...	7	...	...	...	3	
...	...	8	1	...	...	4	
...	...	9	...	...	...	4	
...	...	10	1	...	...	5	
...	...	11	...	...	...	5	
...	...	12	...	...	...	5	
...	...	13	...	...	...	5	
...	...	14	...	...	...	5	
...	...	15	...	...	...	5	
...	...	16	...	1	...	4	
...	...	17	...	1	...	3	
...	...	18	...	1	...	2	
...	...	19	...	...	...	2	
...	...	20	...	2	...	...	

The actual number of deaths was six; but the mistake cannot be corrected without a communication from Ceylon.

ARTICLE III.—*Analysis of the Cases of Scarlatina that occurred in James Donaldson's Hospital during the Autumn of* 1852; *with some Remarks on the present state of our Knowledge of that Disease.* By J. D. GILLESPIE, M.D., F.R.S.E.[1]

(*Read before the Medico-Chirurgical Society, 19th January* 1853.)

SCARLET FEVER has prevailed so extensively in Edinburgh during the last few years, and has occasionally committed such ravages among the younger branches of the community, that no opportunity should be neglected of adding to our knowledge of this very preva-

[1] These observations were made when acting for Dr Gillespie, senior, who has the medical charge of the hospital.—J. D. G.

lent and often most dangerous disease. I trust, therefore, that the Society will bear with me while I endeavour to communicate the result of my experience during the late epidemic at Donaldson's Hospital.

No city in Great Britain, I believe, offers such peculiar advantages as Edinburgh, for the investigation of diseases incidental to adolescence. However questionable the utility may be, of having so many charitable institutions endowed and fitted up like palaces for the education of the poorer classes, it cannot be denied, that much benefit has accrued to medical science from the opportunities afforded for watching the progress and effects of treatment in the various epidemics that have attacked these large assemblages of children. Valuable information has thus been obtained, regarding the different epidemics of scarlatina that have prevailed in Edinburgh and its vicinity; and this, combined with the recorded experience of others engaged in extensive practice, must form a very important adjuvant to the proper appreciation of the disease. It appears to me, however, that some writers, from an analysis merely of the cases which came under their notice during one epidemic, have proceeded somewhat hastily to draw general conclusions, with regard to the pathology of scarlatina, which the observations of others, even during the same epidemic, go far to disprove.

It is not to be wondered at that the older authors, who first recognised and described with such fidelity the disease as it appeared to them, and who had not the ample details afforded to modern inquirers, should fall into such an error; but in the present day, when the literature of the subject is so extensive, too much caution cannot be exercised in drawing inferences with regard to the pathology and treatment.

No disease has presented a greater variety of features, and more fully illustrated the observations of Dr Graves on the changes which from time to time occur in the type of fevers; and no disease has, I suspect, required more decided modification of treatment, in accordance with the leading characteristics of the prevailing epidemic.

Donaldson's Hospital, a charitable institution opened about two years ago, for the education of children belonging to the most deserving of the poorer classes, is situated amid extensive grounds on an eminence about half a mile from the western suburbs of Edinburgh. It possesses accommodation for about 300, but at present contains only 123 children, and 26 adults. The children are between the ages of eight and twelve, and are of both sexes, a portion of them consisting of deaf mutes. There are 61 girls and 62 boys; of the former 21, and of the latter 26, are deaf mutes.

Previous to the admission of each child, a schedule had been sent to the ordinary medical attendant of the family, to be filled up, in answer to queries regarding the various diseases to which youth is subject; and from this source I have gathered, rejecting all the instances where the details were not quite satisfactory, that of the 123

children, 23 are expressly stated to have had scarlatina, viz. 9 girls and 14 boys, 9 of them being hearing children, and 14 deaf mutes. It will be subsequently shown that some of these had a recurrence of the disease.

Of 33 adults, who were resident in the hospital either during part of the time or the whole period of the epidemic, only 5 can be ascertained to have had the disease previously, and none of them had a second attack.

The first cases of scarlatina appeared on the 17th of September 1852, about a fortnight after the majority of the children had returned from their annual vacation. The disease did not develop itself gradually, but seemed to have suddenly fallen like mildew on the institution, parties occupying very various portions of the building being first attacked.

During the first week no less than 40—36 children and 4 adults—were sent to the sick-rooms, showing evidence of the disease; and though in some it never went further than the premonitory symptoms, there was still, I think, sufficient proof of these effects being produced by the same morbid poison.

The last case of scarlatina occurred on the 19th of December, and between the 17th of September and that date, 70 cases—62 children and 8 adults—had come under observation, besides a number of milder cases, probably caused by the same contagion, but where the symptoms were not sufficiently marked to warrant their being ranked among any of the ordinarily received classes of the disease.

It has been very generally asserted, that scarlet fever is so diversified in its action, that it is not unfrequent to observe its most deadly variety, in conjunction with its mildest form, seizing on the same family.

During the late epidemic at Donaldson's Hospital, I was therefore led to expect that among so many children differing in constitution, a corresponding difference would be apparent in the character of the disease; but I was surprised to find a wonderful persistence in the same type throughout the epidemic.

The most prominent features of the disease which I noticed were the following:—

I. The almost invariable sickness and vomiting at the commencement, most frequently of greenish biliary matter.

II. The invariably injected appearance of the fauces prior to, or ushering in, the febrile symptoms.

III. The sthenic rather than asthenic type of the fever.

IV. The little tendency to severe head symptoms.

V. The total absence of the malignant form of the disease.

VI. The mildness of the sequelæ, which for the most part affected the glands and cellular tissue of the neck.

VII. The non-existence of dropsy or of albuminous urine, whether as a concomitant or sequela.

Such being the characteristics of the fever, will sufficiently account

for the absence of any fatal case, though indeed the complications, which were superadded in two instances, for a considerable period occasioned much dubiety as to their ultimate recovery.

The varieties noticed may be arranged under the five classes enumerated by Dr Copland; premising that, as in every case more or less sore throat existed, those only are noted as coming under the appellation of scarlatina simplex, where the affection of the throat was comparatively mild.

I. Scarlatina simplex.
II. Scarlatina anginosa.
III. Scarlatina maligna.
IV. Scarlatina faucium.
V. Scarlatina latens.

Every epidemic hitherto observed has had one or other of the first three for its prevailing type. These have been so frequently and accurately described, that it would be superfluous here to enter on their consideration. The remaining two are exceptions; but a sufficient weight of evidence has now, I think, been adduced to constitute them distinct varieties.

Scarlatina sine exanthemate—an affection of the throat, accompanied by fever, without the scarlet eruption on the skin, and followed, in many instances, by some of the sequelæ incidental to the more ordinary forms—has been noted as occasionally occurring even by the earliest authorities. At Donaldson's Hospital several well-marked cases came under observation.

Scarlatina latens—a form where the usual primary fever and its concomitants are wanting, or at least undeveloped—has been constituted a new variety by Dr Copland, who claims for himself the credit of first pointing out this form of the disease.

At the time (1848) when he was first led to the conclusion, that this was a true variety, dropsy appears to have been a most frequent sequela of the epidemic in London, and it was the discovery of this affection seizing on persons exposed to contagion, but who had not shown any outward evidence of the ordinary symptoms that justified his establishing the latent variety of the disease. Dropsy, with its accompanying dangers, has therefore been chiefly adverted to in his description; but if it be admitted that the presence of one occasional sequela be taken as an indication of scarlatina having existed in a latent form, we cannot deny the same acceptation to other and more constant, though perhaps milder, sequelæ.

Accordingly, I have placed under the fifth class several cases where swelling of the parotids, not preceded by sore throat or eruption, was the only manifestation of the disease.

As scarlatina latens has not yet, so far as I am aware, been very generally recognised by the profession, I may here state the evidence which I have been able to collect regarding its occurrence.

1. Dr Copland saw several cases of dropsy, without any other symptoms, in individuals exposed to contagion.

2. Dr Graves and Dr Alexander Wood have each recorded one case of anasarca, where the primary symptoms were wanting.

3. Mr Miller, physician to the London Western Dispensary, during the epidemic of 1848, saw ten cases of anasarca occurring in infected habitations, without any previous fever or eruption.

4. Dr Rowland, at the Bloomsbury Dispensary, saw several cases of dropsy without the scarlatinal eruptions. Care was taken to examine into the previous history. More than two-thirds of all the cases of scarlatina under his care had subsequent dropsy.

5. At the New Town Dispensary I lately saw an infant of eighteen months, labouring under extensive anasarca; and the mother declared, that though several of the other children were convalescing from the ordinary form of scarlatina, the infant had not shown any indications of the disease.

6. Three cases of swelling and tenderness of the parotids, in one of them also suppuration of the sublingual gland, were noted at Donaldson's Hospital, where none of the previous symptoms had been observed to be developed, though the individuals were subjected to a daily scrutiny.

These are the data on which I have grounded my opinion, that scarlatina latens should be considered a distinct variety. They may appear inconclusive to some, but they are thrown out at present, in order that ulterior observation may be directed to determining the question.

I now proceed to enumerate the cases under the various classes indicated.

I.—*Scarlatina Simplex.*

Of this variety there were 27 cases, which may be thus arranged:—

			Hearing.	Deaf-Mutes.
27 Children,	Boys,	7	4	3
	Girls,	20	10	10
Total,		27	14	13

No important peculiarity was noticed in this variety. The eruption exhibited very different degrees of intensity, in some being of a bright florid colour, and very profuse; in others, of a livid brownish colour, and scanty. Sequelæ occurred more frequently among the latter than the former.

II.—*Scarlatina Anginosa.*

Eighteen cases—fourteen children and four adults.

			Hearing.	Deaf-Mutes.
14 Children,	Boys,	6	3	3
	Girls,	8	5	3
Total,		14	8	6

4 Adults,.....	Male,.....	1
	Females,.....	3
Total,.....		4

In several of these cases the tongue was more severely affected than the throat, being swollen, covered with patches of fetid lymph, and deeply ulcerated in some places, with frequent abrasions of the mucous membrane in others.

In one boy the eruption presented an unusual appearance, consisting of innumerable small vesicles on a slightly florid ground. These vesicles, at first clear, became filled with a milky fluid, flattened, and dried into small pearly scales.

III.—*Scarlatina Maligna.*

No illustration.

IV.—*Scarlatina Faucium.*

Twenty-two cases—nineteen children and three adults.

			Hearing.	Deaf-Mutes.
19 Children,.....	Boys, ...	12 ...	7 ...	5
	Girls, ...	7 ...	4 ...	3
Total,.....		19 ...	11 ...	8

3 Adults, ... Females, 3.

The fever ran very high in some of these instances, but was not of so long continuance as in the eruptive forms.

Some doubts have been entertained, whether cases of scarlatina faucium can propagate the disease. My own belief is, that they have not that power, or at least possess it in a very inferior degree.

Mr Sidey, in the "Edinburgh Medical and Surgical Journal" for 1837, mentions twelve cases, in which he observed that when the sore throat was unaccompanied by eruption, it did not extend to other members of their families, though they consisted of many susceptible younger individuals.

Another *questio vexata* is, whether, having had scarlatina faucium, renders a person less amenable to the eruptive form. In the first place, I think it will be found that the majority of cases of this affection have had the regular disease previously; and, secondly, where such a state of matters does not exist, that the having undergone scarlatina faucium confers merely a slight degree of immunity, and that chiefly from the then prevalent epidemic.

In Dr Alexander Wood's interesting paper on the connection between scarlatina and erysipelas, I find it stated that every case of scarlatina faucium that came under his observation had previously passed through the eruptive disease.

V.—*Scarlatina Latens.*

Three cases—two boys, both deaf-mutes, and one adult female.

One boy had swelling of the parotids, more especially on the left side, with no affection of the throat whatever; the other had inflammation and suppuration of the sublingual gland, with no sore throat. The adult was one of the teachers, and had considerable fever accompanying the swellings, which affected both parotids.

This analysis shows, that of the 70 cases enumerated there were 35 girls and 27 boys, with 8 adults. It has been very generally asserted, that females are more susceptible of the contagion of scarlatina than males; the present instance to some extent supports the observation. This becomes more apparent, when we examine the two regular varieties—scarlatina simplex and anginosa; for we find under these classes 28 girls and only 13 boys.

It may be supposed also that deaf mutes are more liable to the disorder than other children; for, though they averaged not much more than a third of all the children, nearly the same number were affected with the disease as there were cases among the hearing children.

It would be rash, however, to place implicit reliance on the results in this one example merely, but subsequent observation may possibly show, that deaf-mutes have a stronger predisposition to be affected by the peculiar poison of scarlatina.

Such an inference may also be drawn from the fact, that ten out of the eleven cases, where scarlatina, or some of its symptoms, attacked the individual a second time, were deaf-mutes.

As it is not easy to get well-authenticated cases of second attacks of scarlatina, and as I think there are several points of interest connected with them, I shall enumerate very shortly the leading features in each.

The first nine cases have been satisfactorily ascertained, from the schedules to which reference was formerly made, to have had scarlatina previously, being certified by medical men, who had been long in attendance on their families. The tenth case has been specially investigated since; and the eleventh had two distinct attacks while resident in the Hospital.

Case I.—E. S., a deaf-mute girl, admitted September 18th, with sore throat and slight fever.

September 21st. Dismissed convalescent. *25th.* Re-admitted with sore throat; no fever. *28th.* Throat still injected; no fever; had warm baths repeatedly, and now a mottled papular appearance presents itself on the nates and shoulders.

October 4th. Dismissed convalescent. *9th.* Re-admitted with swelling of the right side of the neck; little fever. *18th.* Dismissed cured.

CASE II.—R. C., a deaf-mute boy, admitted October 15th. Slight fever; pulse 110; skin hot; throat slightly inflamed; small vesicular eruption on forehead.

October 19*th.* Dismissed cured. 28*th.* Re-admitted with swelling, tenderness, and pain of both parotids; tongue foul, covered with a whitish fur; pulse 126; eruption again visible on forehead; small vesicles scattered over a red patch. 29*th.* Pulse 112; skin over the parotid pink.

November 6*th.* Dismissed cured.

CASE III.—M. R., a deaf-mute girl, admitted October 1st. Sick and vomiting; eyes heavy; tongue white, with enlarged papillæ; pulse 100.

October 3*d.* Fever not abated; pulse 108; tongue foul, browner than yesterday; throat much swollen; no eruption. 4*th.* Fever abated; tongue still foul; throat easier. 8*th.* Pulse 112; tongue still very foul; she looks heavy and dull. 11*th.* Allowed to be up.

November 18*th.* For some weeks this child has been sickly, but is now well; dismissed cured.

CASE IV.—R. G., a deaf-mute boy, admitted October 9th, with sickness and slight fever; no sore throat; through the night the nose bled freely.

October 11*th.* Dismissed convalescent. 15*th.* Re-admitted with slight fever and sore throat; small vesicular eruption on the back of the neck and shoulders. 18*th.* Dismissed convalescent.

November 12*th.* Re-admitted with profuse suppuration of the left ear. 15*th.* Both parotids considerably swollen. 25*th.* Dismissed cured.

CASE V.—E. N., a deaf-mute girl, admitted October 17th. Slight sore throat; foul tongue; no fever; no eruption.

October 21*st.* Slight appearance of eruption below the skin; tongue foul; face slightly swollen and flushed. 24*th.* Dismissed convalescent.

CASE VI.—P. M., a deaf-mute boy, admitted September 29th, with rigors; throat slightly inflamed; tongue very foul, brown and furred; pulse 92.

September 30*th.* Tongue still foul; pulse 92.

October 1*st.* Tongue cleaning at the tip; pulse 94. 4*th.* Dismissed convalescent.

CASE VII.—J. A., a deaf-mute boy, admitted November 17th, with a large swelling over the left parotid; no sore throat; no fever.

November 25*th.* Convalescent.

CASE VIII.—P. D., a deaf-mute boy, admitted November 13th, with very slight sore throat.

November 15*th*. Dismissed well. 18*th*. Re-admitted with inflammation of the sublingual gland, which went on to suppuration; 25*th*. Convalescent.

CASE IX.—J. A., a hearing boy, admitted September 29th; vomiting, headache, and swollen tonsils.

September 30*th*. Tongue foul, covered with fetid lymph.

October 1*st*. Tongue cleaning, raw at various places; tonsils injected. 4*th*. Convalescent.

CASE X.—W. M., a deaf-mute boy, admitted October 2d; sick and vomiting; sore throat; after the bath, a vivid eruption came well out.

October 3*d*. Pulse 110; fever smart. 4*th*. Pulse 112; skin of a deep red colour, with more vivid papulæ; tongue foul, covered with a white fur, and enlarged follicles; throat not severely inflamed; restless and delirious last night. 7*th*. Fever gone; tongue clean; skin desquamating. 10*th*. Convalescent.

November 2*d*. Dismissed cured.

CASE XI.—E. A., a deaf-mute girl, admitted October 14th, with severe sore throat, fever, but no eruption; pulse 118.

October 15*th*.—Pulse 110, full; no eruption; face flushed. 16*th*. Pulse 102, full impulse. 19*th*. Convalescent.

November 2*d*. Dismissed. 29*th*. Re-admitted with sore throat; fever high; face flushed; eyes suffused; pulse 126; inflammation of throat severe, especially of left tonsil. 30*th*. After the warm bath, a copious vivid miliary eruption.

December 1*st*. Tonsils covered with flaky lymph; pulse 102. 3*d*. Fever and eruption quite gone. 7*th*. Convalescent.

The details of these cases may be considered as tending to elucidate two important points:—

First, What I alluded to previously: the apparently greater susceptibility of the deaf and dumb to the poison of scarlatina, as evidenced by ten out of the eleven belonging to that class, who have twice had the disease. Whether such a liability depends merely on a scrofulous diathesis is very doubtful; for it remains yet to be shown, that the scrofulous cachexy predisposes to scarlatinal influence.

Observation of the individuals on whom the epidemic fell does not bear out that supposition; for it was not remarked that the disease preponderated among children possessing a scrofulous taint; and many well-marked strumous cases escaped altogether from the malady.

In the remarks made by Dr Graves on the various epidemics of scarlatina which he witnessed, he states that he did not observe any peculiar liability to scarlatinal contagion evinced by strumous children.

This does not invalidate the opinion, that the poison of scarlatina may exercise a more than usually severe influence on such children, when the disease has once become established.

Secondly, In the majority of cases the absence of the specific eruption, or, when it was present, its scanty nature, is deserving of comment.

Only one case in the eleven—the boy W. M.—had the scarlet eruption well defined, and it is somewhat strange that his is the only case to which doubt appertains as to whether he really had the disease formerly; that point resting on the authority of his parish minister, who, however, has since given me to understand that, from further investigation, it amounts almost to certainty that the boy had the disease previous to admission.

His mother says he was the youngest of a large family; that the disease was very prevalent at one period in the village where she resides; that all the members of her family took it; and that she distinctly remembers the scarlet rash appearing on his chest.

Case XI.—the deaf and dumb girl, E. A., who was twice in the sick ward, first with scarlatina faucium, and latterly with the eruptive form—supports the conclusion formerly arrived at by others, that having undergone an erratic variety of scarlatina does not confer a great degree of immunity from the more regular forms. There were many other instances, though in a less marked degree, of children having the congested sore throat, accompanied by ephemeral fever, in whom, some weeks afterwards, scarlatina simplex or anginosa was developed.

The converse of this statement also appears to hold good,—that persons who have had one or other of the regular forms of the exanthema, are more or less liable to be affected with some of the milder symptoms, though they escape from those which may be termed of a specific nature.

Concerning the other cases of scarlatina little need be said, as they did not differ materially from the ordinarily-described varieties of the disease. In some the fever ran very high, especially in those where the eruption did not appear copiously at first. There were not above seven or eight children in whom the angina was very severe; two of the adult females, however, suffered most acutely from this affection. In another of the domestics the virulence of the attack passed at once to the internal ears, causing excruciating pain and suppuration from both external orifices, with almost complete deafness for a time; but hearing was subsequently in a great measure restored.

One little girl had the severe complication of pleuro-pneumonia, which supervened on the second day of the appearance of the scarlatinal febrile symptoms, and which seemed to delay the development of the eruption to the fourth day. While convalescing from this chest affection, she had a severe attack of peritonitis, apparently chiefly involving the serous covering of the cœcum, which again

caused serious apprehensions to be entertained for her safety; however, she ultimately got well.

Shortly after the accession of the premonitory symptoms of scarlatina, a boy was seized with hooping-cough, which held its sway for some weeks without any further manifestation of the exanthema, but on its declension he was again affected by the ordinary symptoms, and experienced a mild form of scarlet fever.

Fifteen children had sequelæ, but some of these were of so trivial a nature that they are hardly worthy the appellation, and are only enumerated in order to show in what organs there was a tendency to a secondary affection in this epidemic, as far, at least, as regards Donaldson's Hospital.

In ten cases there was more or less tenderness and swelling of the parotids or submaxillary glands. Some of these had smart fever accompanying the disorder; but the majority were mild, and entirely subsided in a few days.

In one girl—E. M.—the left submaxillary gland suppurated. This child had been imprudently permitted to go out into the open air with some friends who had come to visit her.

One boy had inflammation and suppuration of the sublingual gland.

Two children had suppuration of the ears.

Two of the female servants had painful affections of the neck; one of the glands, the other of the muscles in that region.

One of the little girls had a severe neuralgic or spasmodic affection of the bowels, which occurred periodically, coming on chiefly through the night, and causing much uneasiness and depression. I think it likely to have been dependent on some affection of the mesenteric glands, the child being of a strongly-marked scrofulous habit.

Two had mild bronchitic attacks. In several of the children, who during the previous winter had laboured under eczema, impetigo, or psoriasis of the scalp, a slight re-appearance of the disease took place while convalescing from scarlatina.

Such were the whole of the sequelæ of the seventy cases of scarlatina, and I cannot help ascribing their comparative rarity and mildness in some degree to the measures adopted for the elimination of the poison during the primary disease, and the careful guarding against exposure to cold during convalescence.

The treatment may be summed up in a few words:—

1. The free and repeated use of the warm bath.

2. Mercurial laxatives on first admission, the indication for these being (I would suggest) the loaded state of the tongue, and the almost invariable occurrence of bilious vomiting at the commencement of the disease.

3. Afterwards small, frequently-repeated doses of a diaphoretic, containing antimonial wine, sweet spirits of nitre, and aqua acetatis ammoniæ.

4. The application of a strong solution of nitrate of silver to the throat, with external fomentations, poultices, or sinapisms.

5. Warm flannel dressing-gowns whenever the fever abated, and the children were likely to expose themselves incautiously to cold.

Making the children wear these flannel gowns was, I am confident, of essential service in warding off the sequelæ; for every one who has had much to do with the treatment of children, must be aware how difficult or impossible it is to prevent their imprudently exposing themselves to cold when convalescing from disease.

This was all the treatment required in the great majority of cases, but of course some of the complications or unusual symptoms indicated a modification in the remedial measures employed.

The advantage of the warm bath was exemplified in a very marked degree; indeed, I have no hesitation in saying, that in the non-malignant forms of scarlatina, at all events, the frequent use of this remedy, especially in the earlier stages, is the most powerful agent we possess.

From what is now most generally received as the true pathology of scarlatina,—viz., that its symptoms are induced by a morbid poison, circulating and reproducing itself in the blood; that, as in all the exanthemata, an attempt is first made by nature to throw out that poison by transpiration from the skin; that, when that object is not thoroughly attained by such a method, the effects of the poison are apt to fall on other organs, and more especially on the kidneys—the recognised emunctories of the effete matters circulating in the blood,—the beneficial results expected from, and realised by, the warm bath can be satisfactorily understood.

Dr Golding Bird, who had extensive experience in the treatment of scarlatina, asserts that he scarcely ever saw dropsy supervene, when the warm bath had been judiciously had recourse to.

In a clinical lecture by Professor Bennett on eruptive fevers, published in the last October Number of the "Monthly Journal of Medical Science," alluding to the treatment of scarlatina by endeavouring to cause determination towards the skin (a procedure, by the way, which has long been adopted—and not very recently, as the tenor of Dr Bennett's remarks would lead us to suppose), that gentleman makes no mention of the warm bath, but suggests, in addition to diaphoretics, the trial of a species of vapour bath, invented and extensively employed by Dr Andrew Wood.

With all due deference to both these distinguished members of the Society, I would submit that, so far from seeing any peculiar advantage which this method possesses over the ordinary plunge bath, I consider it liable to sundry objections, the most important of these being, the great danger of care not being taken to remove from the patient subsequently the bed-clothes, which must be pervaded with a not very wholesome moisture. It is, perhaps, scarcely fair to ascribe the rheumatism, experienced by the only case in which Dr

Bennett tried the method, to such a source, though it is certainly a popular belief, that lying in damp sheets is as legitimate an excitant of rheumatism as could well be imagined.

It is possible that some instances might arise, in which the warm bath, failing to effect the object in view, a longer application of heat and moisture, by means of the hot bottles and wet stockings, might succeed; but, except in such cases, the danger I have hinted at should prevent it from being extensively employed, except under vigilant observation or personal superintendence.

Among the poorer classes it is certainly not available, for changes of bed-clothes, after each application of the bottles, cannot be expected. But, even in the poorest habitation, a large tub may be found, which may be made as powerful an auxiliary as the most costly bath.

The vaunted powers of belladonna as a prophylactic were not tested, for two reasons:—1st, Because no faith was put in its efficacy, the late experiments of Dr Benjamin Bell at Watson's Hospital being deemed conclusive; and 2dly, Because, had belladonna been administered, the experiment would not have been decisive without allowing the healthy children to mingle freely with the infected; but as great facilities were afforded for keeping them entirely separate, such a procedure would not have been warrantable.

The accommodation for the sick in Donaldson's Hospital cannot be too highly commended. It reflects much credit on the governors who suggested, and the architect who planned, the arrangements.

The whole of the upper story of the back of the building is appropriated to that purpose, being farthest removed from the various dormitories and class-rooms. All access to it is completely prevented, save by two staircases, one for the boys, the other for the girls, the doors at the foot of which can be kept locked, if necessary.

The accommodation for invalids of each sex respectively is as follows:—

1. Three lofty rooms, of equal dimensions, communicating with each other by a spacious doorway, minus the door. A large closet, or rather side-room, opening off one of these, contains a plunge and shower bath, with other conveniences. The centre room is occupied by the nurse; the other two by the patients, and they contain eight beds each. The only connection of these rooms with the passage is by a door entering from the nurse's room.

2. Two other separate rooms, of about the same size, with a bath-room attached.

3. A small private room, and two small rooms, containing two beds each, in the story above, intended for servants.

4. A kitchen and scullery, common to the two sets of sick-wards.

A uniform temperature is kept up in these apartments, by means of heated air.

The accommodation for the sick being so extensive, we were enabled to set apart a room for doubtful cases, and another for convalescents, besides the one ordinarily used as a play-room for the latter class of cases.

These conveniences also admitted of the patients who had laboured under scarlatina being kept apart for some weeks, in order that there might be little danger of communicating the contagion to others.

Previous to dismissal, they each got a warm bath and a complete change of clothes. Notwithstanding these precautions, about half of the entire establishment took the fever, which might be urged as an argument in favour of atmospheric influences being sometimes sufficient to generate the disease.

I hoped to have heen able to elucidate some deductions with regard to the theories of contagion, from the manner in which this epidemic showed itself in the hospital, but the fact of so many children having been a short time previously allowed to visit their friends, many of whom, it is more than probable, were resident in infected districts, would greatly invalidate any conclusions which I might come to on the subject.

There can be no doubt that accurate observations of the manner in which contagious diseases seize upon large, secluded, densely populated habitations, would greatly tend (if, indeed, we are ever likely to arrive at the ultimate truth in such matters) to clear away the dark veil of mystery which at present enwraps our knowledge of the mode in which epidemics are propagated.

In conclusion, I now proceed to notice what has of late years attracted the attention of all observers of the phenomena of scarlatina,—the occurrence of albumen in the urine, with or without dropsy as a sequela.

This subject has become invested with so much interest, and has given rise to so many conflicting statements, that I fear an undue importance has been attached to the existence or non-existence of albumen in the urine, and that conclusions have been hastily arrived at, by over-zealous theorists, from too limited a field of observation. I do not for a moment question the accuracy of their details, as regards the particular variety of disease which comes under observation, but what I object to is, that, reasoning on these too limited data, absolute pathological inferences are drawn, which the experience of the very next writer may at once disprove. For instance, in the course of my reading, I have repeatedly seen it remarked, that a scanty flow of urine in the progress of scarlatina is a sure indication of the supervention of dropsy; but in the cases of scarlatina which I have lately attended at Donaldson's Hospital, and in the practice of the New Town Dispensary, I have occasionally observed, during the first days of desquamation, the urine reduced to very small amount, without subsequent appearance of dropsy or any trace of albuminuria.

I believe a marked diminution in the diurnal secretion of urine has been frequently observed during the critical period of many other diseases.

Dr James W. Begbie, in the "Monthly Journal of Medical Science" for October, has written a valuable paper on "temporary albuminuria," and has in it intimated his opinion, that albumen will be very generally found in the urine of persons labouring under scarlatina during desquamation of the cuticle. I consider Dr J. W. Begbie's communication of much value, as being calculated to draw the attention of the profession to the critical evacuations, which often occur in the course of many diseases; but I cannot subscribe to the conclusions which he there comes to concerning the temporary albuminuria of scarlatina, either as regards the frequency of its occurrence, or the theory of its elimination.

Knowing, as I do, the amount of laborious investigation which Dr J. W. Begbie has bestowed on the subject, I feel much diffidence in making statements opposed to his high authority; but though the discrepancy in our observations may appear very considerable, it can be easily accounted for by the fact, that our labours have been bestowed on different epidemics.

If Dr J. W. Begbie had so qualified his remarks, by making them applicable merely to the epidemic prevailing during the period of his inquiry, I would not have deemed it necessary to bring evidence in disproof; but in both the communications which he has read to this Society he has made no such qualifying statement, but has laid down as almost an invariable, what will probably hereafter be shown to have been merely an accidental, or at least typical, occurrence.

Prepared to realise Dr J. W. Begbie's observations on the urine, I instituted a careful and repeated examination of that fluid during the period of desquamation in all the cases (and they were numerous) in which that process was distinctly established, but I could not discover in one single instance the slightest trace of albumen by the usual tests of heat and nitric acid.

So far, then, as my experience goes, what has been described as an almost invariable rule has proved a complete *non sequitur*.

In the years 1847, 1848, and 1849, when, I apprehend, most of Dr J. W. Begbie's investigations were instituted, a remarkable tendency to dropsy has been shown by many authors to have existed. From this we may legitimately infer, that a peculiar liability was evinced by the kidneys to be acted on by the morbid poison, and it can be easily understood how a very slight affection of these organs may have frequently given rise to the temporary albuminuria described by Dr Begbie, without producing the more formidable general anasarca.

But even though there be in some epidemics a remarkable persistence in type, we occasionally find that in some localities, whether modified by difference of location, a peculiarity in the specific con-

tagion, or other undetermined causes, a diversity of symptoms or sequelæ may arise.

A good instance of this may be taken from Mr Wood's excellent paper on the epidemic of scarlatina, as it occurred in various charitable institutions in this city during the year 1836.

In Heriot's Hospital, of 45 boys who were attacked by scarlatina 8 had dropsy, with albuminous urine; while at the Merchant Maidens' Hospital, which is closely adjacent, and during the very same period, 21 girls had the fever, but not one of them dropsy or albuminous urine!

Such a disparity of results occurring in the practice of one medical man, about the same period, and in hospitals not far removed from one another, must surely prove, or at least suggest, admitting, as is most likely, that the treatment and sanitary arrangements were the same, that a different type of the fever existed in one series of cases than was established in the other; that in the former there was a tendency in the morbid poison circulating in the blood to affect the kidneys, while in the latter no such disposition was evinced.

With regard to the theory propounded by Dr J. W. Begbie of the *rationale* of the production of albuminuria, I must say a few words, though I am not prepared to enter at length on the changes which must be effected on the kidneys, ere this unusual deposit can make its appearance.

Dr J. W. Begbie remarks:—"What, then, is the cause of albumen in the urine in simple scarlatina, and what its pathological import? I conceive it to be as essential a symptom of the disease as is desquamation of the cuticle,—to be associated to a certain extent with that desquamation,—to be, in fact, the result of a desquamative process, which the mucous membranes in this disease, equally with the skin, are subject to.

"Granted, then, that this desquamation occurs when such a change is taking place in the epithelial membrane, lining the minute tubes of the kidney, the office of the cells composing which is to eliminate from the blood the matters, solid or fluid, which in the normal exercise of the renal function compose the urine, it surely is not surprising that the albumen from the former should to a slight amount enter into the latter. Such I believe to be the cause of its occurrence; nor can I regard its presence as indicating any pathological condition farther than the separation of epithelial cells, and their passage in the current of urine."[1]

This, then, is the theory offered for our acceptance, and it doubtless contains much that is probably correct; but if we are to admit the accuracy of the main proposition, that the separation of epithelial cells in considerable quantity from the minute tubes of the kidney, is all that is necessary to give rise to albuminuria, then we must surely expect to find that in all cases where an unusual amount of epithelial cells is discovered in the urine, albumen also must be

[1] Monthly Journal of Medical Science, October 1852.

present,—an observation which, so far as I am aware, has not yet been accredited by modern pathologists.

Bearing antagonistically on this doctrine, I may quote from Mr Benjamin Bell's account of the scarlatina at George Watson's Institution, a paper which is so far corroborative of my opinion, that albumen is not always to be considered as frequently occurring during the period of desquamation; Mr Bell having repeatedly made experiments for its discovery, without success.

Mr Bell observes:—"In one of them, while this process (desquamation) was at its height, I found the urine loaded with epithelial cells, but not rendered opalescent by either nitric acid or the application of heat."

Dr Bennett, in the clinical lecture published in the "Monthly Journal of Medical Science" for October last, also relates a case where casts of the tubes with epithelial cells were found in the urine, though it was not coagulable by heat or nitric acid.

Something more than the mere desquamation of epithelial cells from the tubuli, must therefore be in operation to produce the phenomenon of albuminuria, and its presence must indicate "a pathological condition further than the separation of epithelial scales and their passage in the current of urine."

If the presence of albumen in the urine is as essential a symptom as desquamation of the cuticle, how comes it that in the seventy cases treated at Donaldson's Hospital, this essential symptom did not become manifest?

The conclusions at which I have arrived from a consideration of the phenomena of scarlatina, witnessed by myself or described by others, may be finally enumerated:—

I. Epidemic scarlatina, considered on an extensive scale, has a characteristic type, which changes at different periods, though all the varieties may possibly have illustrations during the same time, and in the same locality.

II. In some epidemics the morbid poison, instead of being thrown off by the ordinary outlet (transpiration through the skin), is apt to fall on the internal organs, and more especially the kidneys, causing congestion in these excreting bodies, and impairment of function, which, in some instances, may give rise to temporary albuminuria, either with or without dropsy.

III. Although it be admitted that a peculiarity of type may dispose to albuminuria and dropsy, the frequency of their occurrence may often be materially modified, or in those epidemics where no great liability to such affections exists, be even prevented, by careful endeavours to eliminate the poison in the first instance, and latterly by protection from exposures to cold or moisture.

IV. The primary and most important indication for treatment must therefore be, resorting to such measures as will most efficiently favour the natural efforts of the system to throw off the morbid poison by transpiration from the skin.

These, Mr President, are the conclusions at which I have arrived. They may be crude and imperfect, but I bring them before you, satisfied that the ample experience of many members of this Society will correct the errors into which I may have fallen, or still further elucidate the interesting and important phenomena of scarlatina.

JANUARY 5, 1853.

ARTICLE IV.—*Entire Absence of Vagina, with Rudimentary State of Uterus, and Remarkable Displacement of Rudimentary Ovaries and their Appendages, in a Married Female, Seventy-Four Years of Age.* By JAMES BRAID, M.R.C.S. Edin., M.W.S., etc.

MRS M——, aged seventy-four years, died on the 25th December 1852. She had been a patient of mine for thirteen years previous to that event. Mrs M—— had never menstruated, but she had been married, and lived comfortably with her husband for many years; the husband being still alive, at the age of sixty-seven. When first attended by me, in 1839, I found Mrs M—— suffering severe pain in the sacral and lumbar regions, and also in the right hip, with frequent desire to go to stool, attended with much pain and difficulty in passing her stools. She also experienced considerable pain in the urinary bladder occasionally, with frequent desire to void urine, together with dyspepsia and flatulence of the stomach and bowels. She had, moreover, been for many years unable to extend the right leg fully, every attempt to do so causing an aggravation of her pain, in consequence of which she had always stooped and walked lame for many years before my attendance, which symptoms she attributed to cold and damp, to which she had been exposed previous to said seizure. She had undergone much treatment before my attendance, without adequate benefit; and, notwithstanding I had recourse to every variety of treatment, local and general, internal and external, which I thought the symptoms indicated, nothing beyond partial and temporary relief could be afforded to her. I also gave her the benefit of a consultation with a physician of great experience; but, having exhausted the whole of our resources to so little purpose, at last I recommended a suspension of medical treatment, and to rely on the effect of regimen, with fresh air and moderate exercise. In this way she went on, so that I saw very little of my patient for several years. However, for the last twelve months of her life, my attendance had been required for dyspeptic and renal symptoms, accompanied with anasarca of the lower extremities, which were also affected for a few days with erysipelas. Three days before death, paralysis of the left side ensued, which was followed by coma, and with great difficulty of deglutition, in which

comatose state she died without a struggle. The urinary secretion, although scanty, was never entirely suspended, for the urinary bladder contained about four ounces of highly coloured urine when the body was inspected.

From the physical investigations which I made in this case about a dozen years ago, and which were also made with equal care by the physician who saw Mrs M—— in consultation with me, it was ascertained that, notwithstanding she was a married woman, and had lived comfortably with her husband for eight-and-twenty years, she had *no vagina*, beyond a depression sufficient to admit the point of the finger to the depth of something less than half an inch, where it terminated by a complete closure, firm and unyielding, having not the smallest aperture, excepting the meatus urinarius, of the usual size and in the usual position. The nymphæ and clitoris were undeveloped, beyond what appears in a girl of a few years old, and there was very little hair on the pubes.

On examining per rectum there was no tumour, no symptom of diseased structure in the rectum, nor could any semblance of a uterus be detected by the finger, either by myself or by the physician who also examined her for the purpose of determining these points. The patient's aspect did not indicate malignant disease, and we therefore thought it probable that some displacement of the uterus, by which it was pressing on the rectum and sacral nerves, beyond the reach of the finger, might be the cause of the more painful and distressing symptoms of which she had so long complained, and for the permanent relief of which treatment had proved of so little avail.

On the death of Mrs M——, who had suffered considerably, almost to the last, from the symptoms enumerated as existing when I was first consulted by her in 1839, in addition to the dyspeptic and renal symptoms, and the paralysis and coma, which had immediately preceded her death, I was most anxious to have an inspection of the body, with the view of ascertaining the state of the *internal organs of generation* in a patient so particularly undeveloped and malformed as I knew her to be in the external parts, as already described, and about which there could be no mistake, as also the malposition of the uterus, which had been *inferred* when no vestige of it could be detected by the finger per rectum.

On the morning of the 27th of December 1852, fifty-four hours after death, I inspected the body of Mrs M—— with great care and attention, with the kind assistance of Dr Henry Browne and Mr Walter Andrew. The body was four feet ten inches high, very thin, excepting the lower extremities, which were slightly œdematous. There was no œdema, however, elsewhere, nor was there any dropsical effusion within the abdominal cavity. The chief object of the inspection was to ascertain the state of the organs of generation, and of the kidneys and other contents of the abdomen and pelvis. The extreme distance between the crests of the ilia was nine inches

and a half. We omitted to take the exact measurements of the pelvis, but it seemed to be of the average size and form for a female of Mrs M——'s stature.

On careful examination of the external organs of generation, the following facts were observed:—Very little hair on the pubes, and none on the labia; the clitoris very small, the nymphæ also very small, and the labia majora and vagina undeveloped beyond what appears in a girl of a few years of age. On still more minute examination, it was ascertained that there was *no vagina* beyond what could admit the point of the fore-finger to the depth of nearly half an inch, where it terminated, the parts being entirely closed by a firm solid structure, excepting on the upper edge, in the usual situation of the meatus urinarius, where the urethra readily permitted a small tube to pass into the bladder, as proved by the discharge of a few drops of urine. This termination of the vagina had every appearance of having been an original formation, and not of being a morbid adhesion of parts subsequent to birth.

Having laid the abdomen open by the usual incisions through the parietes, the stomach and bowels were found almost entirely empty, with the exception of a little flatus. Having drawn the bowels aside, in order to ascertain the state of the contents of the pelvis, the bladder was seen distended with about four ounces of highly coloured urine, as ascertained by evacuating its contents; the colon was almost entirely empty, and the whole of the sigmoid flexure and rectum were empty and contracted, excepting about two inches of the lower part of the rectum, which contained a little pultaceous fæces; but *there was no appearance of the slightest vestige of a uterus or its appendages within the cavity of the pelvis.* We therefore instituted a careful search in the abdomen, and there, on the outside of the origin of the right psoas magnus muscle, between that and the right kidney, and in a line with the lower part of that organ, we discovered a small ovary, about the size of a small field-bean, with its Fallopian tube and fimbriated appendage adherent to and growing from the peritoneum at the point indicated. On the left side, in nearly a similar position, but somewhat lower, there was another ovary and Fallopian tube, and fimbriated extremity, only this ovary did not exceed the size of a large pea, and the fimbriæ were short but numerous. The right Fallopian tube was about three inches long, the left about two inches long.

On examining the state of the walls of the abdomen and pelvis, both inguinal rings were found open, so as readily to admit the point of the finger to pass down for about an inch. The canal was conical, with the apex downward, so that there could have been no permanent lodgment of the bowel or omentum within the sac, so as to have caused pain from strangulation of its contents.

Having now pressed up the fæcal matter from the lower part of the rectum, which readily passed upwards and distended the bowel, thereby proving that no permanent contraction or stricture of the

rectum had existed, a ligature was passed round the upper part of the rectum and secured, and the bones of the anterior part of the pelvis having been sawn through, we removed the whole of the contents of the pelvis, the rectum, bladder, pubes, and external organs of generation, including also the perineum and anus, so as to afford better opportunity of examining the whole, and of making a preparation with all the parts in their relative connections.

Upon careful examination under the peritoneum, where it was reflected from the posterior part of the urinary bladder to the rectum, and consequently in the position in which the uterus is usually situated, we found two small, flat, gland-like bodies, more than half an inch apart. On careful dissection, we ascertained that these bodies were the original rudimentary bi-cornute points of development of the uterus, with a rudimentary fallopian tube, and round ligament passing off from the external angle of each. Each body was triangular in form, nearly half an inch in length, three-eighths of an inch broad, and three-sixteenths of an inch thick at the centre. We examined with great care, but could not detect the smallest semblance of an attempt at the formation of a vagina.

Both kidneys were small, but that on the left side was also very soft and white, and contained many small cysts. On examining this kidney by the microscope, the white deposit was found to be fatty. Many of the convoluted tubes were so crowded by granules, globules, and granular cells, as to present a dark outline, within which no epithelium could be recognised.

The state of the kidneys was sufficient to account for the paralysis and coma immediately preceding death; but we could discover no structural cause to account for the acute sufferings the patient had been afflicted with for the last five-and-twenty years of her life, and which so completely baffled the efforts of all who undertook the treatment of said symptoms. The only conclusion, therefore, is this, that said symptoms were entirely neuralgic in their character, depending upon some condition of the nerves, too minute to be detected after death, by the eye only; and we neither had time, nor would have had permission, for minute microscopical investigation of the state of all the nerves likely to have been implicated.

P.S.—I shall forward casts of the external and internal organs of generation above described to the Royal Colleges of Physicians and Surgeons of Edinburgh and London.—J. B.

Burlington House, Manchester,
January 31, 1853.

ARTICLE V.—*An Account of Two Cases of Poisoning with Carbonic Acid in remarkable circumstances.* Communicated by JOSEPH LAW, Esq., Surgeon, lately House-Surgeon of the Sheffield Infirmary.

ON Sunday, January 4th, 1852, Mr Wright Wilson of Sheffield was summoned to the house of George Hall, in Park Hill Lane, called also, from its elevated position, "Sky Edge." Mrs Hall was extended on her right side on the chamber-floor, close to the bed-side; while her husband was in an easy bent position, also on his right side, on the opposite side of the bed, his head and chest being under the bed. They were both dead, and quite cold. The woman's countenance was mild and placid; the man's bore a slightly uneasy expression. They were in their night dresses, and there was a plaster on the chest of each. With the exception of an abrasion (less than the nail of the little finger, and surrounded neither by redness nor swelling) on the crown of the man's head, no external trace of violence could, on a very careful examination, be discovered. There were slight suggillations on the man's back; but elsewhere the skin of every part of the two bodies was perfectly natural in colour and appearance. The mucous membranes of their mouths and pharynges were healthy, and free from every kind of odour. The chamber was orderly, every thing appearing to be in its place. There was a singular and intolerable smell in the house, strongest in the chamber. The woman had been in the habit of taking laudanum; of this drug two or three drachms were found in the house.

On Monday, January 5th, probably thirty hours after death, Mr Wilson, by order of the coroner, examined the bodies.

There were no unnatural external appearances.

There was congestion of the membranes and sinuses of the brain. Each lateral ventricle contained about ʒss. of clear serum. The lungs were gorged with dark blood; the bronchial mucous membrane was slightly inflamed; and there were extensive pleuritic adhesions. The left sides of the hearts were nearly empty; the right contained a quantity of dark half coagulated blood. The stomachs were healthy; the woman's contained about ʒiv. of thick gruel-like fluid. This fluid, the stomachs, duodenums, portions of the jejunums, and portions of the colons, were given, at the request of the coroner, to Mr Haywood, professional chemist, to be analysed.

On Tuesday, January 6th, Mr Wilson, suspecting the drainage, again examined Hall's house, but again without finding any explanation of the catastrophe. In the adjoining yard, however, he perceived a small quantity of smoke issuing from a cesspool, filled with cinders and fæcal matter, in a state of smouldering combustion. When the burning mass was disturbed, even slightly, with a

spade, it yielded dense volumes of smoke, and a most disgusting smell, resembling that in the house. Knowing that the ground was loose, stony, and permeable, Mr Wilson communicated his suspicion to the coroner and to Mr Haywood, that the gaseous products of this combustion had found their way into Hall's house.

It was proved on the inquest, that, on the night of the 3d January, Mrs Hall perceived a very disagreeable smell in her house, and that she, assisted by a neighbour, carefully searched for the cause, but found nothing. At this time the cinders in the cesspool were red hot; but it was not at all suspected that the fire in the cesspool was in any way connected with the impure state of the atmosphere in the house. A straw mattress had been burnt in the cesspool a few days previously. In this way the cinders had been set on fire; but Mr Wilson could see no trace of the mattress.

Mr Haywood's Statement.

On Wednesday, January 7th, I examined Hall's house in Park Hill Lane, one of the highest and most airy situations in Sheffield. The house stands on a heap of stones, the remains of an extensive quarry; it consists of one sitting-room, with a small pantry on the ground floor, and a bed-room over them; it is about two feet below the level of the ground on the south-western side, on a level with the ground on the east, and somewhat above it on the north-west. In consequence of the loose nature of the ground, the south-west wall has deviated about one foot from the perpendicular, and, in order to make this wall look straight in the inside, boards, extending from the floor to the ceiling, have been placed near it, there being a space of some inches between the two. At a distance of twelve feet from this wall, and in the bed of loose stones on which the house is built, is excavated a cesspool for ashes and night-soil; this is six feet long, five feet broad, and four feet deep; its walls are lower than the surface of the loose stones, and have little or no mortar; any gases, therefore, generated within the walls of the cesspool, would easily find their way into the bed of stones, and thence through the broken foundation into Hall's house. Mr Haywood found the cesspool nearly full of *burning* cinders, mixed with fæcal matter. The fire had not made much progress previously to the evening of the accident, when, as proved before the coroner, the contents of the cesspool were "one mass of fire." Ashes were thrown upon it in the hope of extinguishing it; but under a heavy western gale, which sprang up towards the morning of the 4th January, the contents of the cesspool again became a mass of fire. The wind, pent up in the corner where the ashpit is placed, forced the gases through the loose stones and broken foundation, into the space between the boards and wall of the lower room, thence into the space between the ceiling and bed-room floor, and thence through large holes and cracks into the bed-room itself. In consequence of the chimney and windows being on the eastern side of the house, the strong west wind produced

a partial vacuum in the bed-room, and this vacuum drew, while the wind pushed, the gases from the ash-pit into the bed-room. The current of air through the bed-room floor blew out a candle; and, although the combustion had nearly ceased when the examination was made, yet the atmosphere of the room was intolerably offensive and oppressive.

By means of an aspirator, Mr Haywood passed four cubic feet of air, as it entered by one of the openings in the floor, through a solution of acetate of lead; no discolouration being produced, it was inferred that neither sulphuretted hydrogen nor hydrosulphate of ammonia was present. Another portion was then passed through a solution of nitrate of silver, without producing anything but a brown tint, arising from a small quantity of organic matter; no cyanogen compounds were consequently present. The air in the aspirator contained 1·4 per-cent of carbonic acid gas, with a trace of sulphurous acid, pure air having only a twenty-fourth of this quantity of carbonic acid.

It is usually believed that an atmosphere containing five per cent. of carbonic acid may prove fatal to man. The bed-room and staircase of Hall's house contain 1283 cubic feet of space. As sixty-two cubic feet of carbonic acid, enough to have poisoned Hall and his wife, are produced by the burning of little more than two pounds of cinders, it may be readily understood that more than sufficient to explain the accident, must have resulted from the burning of a heap of cinders measuring sixty or seventy cubic feet.

The communication between the burning cinder heap and the bed-room was more complete than between the former and the sitting-room, where a canary, favourably placed opposite the north-west window, was found uninjured. The space between the boards and wall acted as a flue; and, on the night in question, the larger part of the air, which entered the bed-room, passed through the burning cinder heap. The sitting-room had an open fire place, and a window to the north-west, through which, on that boisterous night, a current of fresh air was driven. The boards were covered with paper, and the communication between the bed-room and sitting-room was cut off by a door at the bottom of the staircase. A little carbonic acid, however, was detected in the sitting-room, coming from behind a corner cupboard placed against the boards, where the paper did not extend.

Three causes prevented the descent of the carbonic acid from the bed-room to the sitting-room:—The door at the foot of the staircase; the rarefied state of the air in the bed-room produced by the west wind; and the condensed state of the air in the sitting-room produced by the wind blowing through the north-west window. If air passed from either of the rooms to the other, it would have passed from the lower to the upper; but this was probably prevented by the open chimney in the former.

A chimney-board in the bed-room was considered to have very much increased the evil; but as the chamber was part of the flue

through which the poisonous gases discharged themselves, their current through the chamber, had the chimney been open, would have been more rapid; but there was space enough between the chimney-piece and chimney-board, and through the imperfectly-fitting windows, for the passage of air; and probably the less impure air of the room passed up the chimney to give place to the poisonous products of the cinder heap.

To trace the communication between the ash-pit and the house, the loose stones, with their covering of earth, were removed to a considerable depth. Mr Haywood found the warm air from the burning cinder heap, passing through and below the broken foundation; and finding also that the oxide of iron on the stones was converted into a sulphuret, he concluded that the hydrosulphate of ammonia had, at the commencement of the burning, been driven in that direction, although he could not detect the presence of any of this substance, or any trace of its action on the metals in the house.

There was not the slightest indication of any vegetable or mineral poison in the parts which were analysed.

From the foregoing facts Mr Haywood concluded, that George Hall and his wife died from inhaling carbonic acid, and other products of the combustion of the burning cinder heap above described.

Part Second.

REVIEWS.

Traité de Chimie, Anatomique et Physiologique. Par MM. ROBIN et VERDEIL. Paris: 1853. 3 Tom. avec Atlas des Planches.

THE science of life, or biology, like that of physics, is obviously divisible into two great sections, the limits of which it is always important to keep in mind, although they are intimately connected with, and mutually dependent on, each other. The former of these comprises the study of living beings in their inactive condition (*vital statics*), while the latter treats of the functions which organisms perform, or of their active or dynamical condition (vital dynamics). The first is considered, in general terms, to be equivalent to the science of anatomy, while the second corresponds to that of physiology.

The essential idea of organisation is that of the mutual depend-

ence, each upon each, of the various parts in relation to one another and to the whole. Hence it is impossible to form an adequate conception of an organised being by studying it in the statical condition only; for in doing so we, by the very act, exclude this fundamental idea from consideration, and look upon each part or organ as independent of, and out of relation with, the rest, and only connected with them on account of certain characters which they possess in common, and which constitute them organic. Although, however, these two aspects of the science of biology do not correspond to actual divisions which are capable of being comprehended, even in the slightest degree, independently of each other, the mode which we have alluded to of viewing the subject is useful as much facilitating its study. By so doing, we commence our investigation by making ourselves acquainted with elementary parts, confining ourselves to their intrinsic characters, and, by means of the combination of these, forming an exact notion of the general characters of the whole organism, which they themselves combine to form. This process constitutes the study of anatomy. It is capable of being continued in a similar manner into the limits of physiology. Arrived at this point of our progress, we investigate, first, the functions which elements perform taken separately; and secondly, their functions when taken in relation to each other and to the whole organism,—this last constituting the study of the special endowments of the tissues and humours, and of organs and systems of organs.

It being admitted that the outline which we have given above is in correspondence with the most natural method of viewing the science of biology, it is evident that the question which presents itself on the threshold of our acquaintance with vital statics must necessarily be that of the determination of the meaning to be attached to the term element. If a distinct definition in reference to this question be wanting, it is clear that further progress is impossible.

In the first place, an *element* in biology is obviously a body which either does live actually, or has lived at some former period, inasmuch as it forms an integrant of an organism, of every atom of which this may be asserted.

By observation it is found, that the two characters of organic beings which are most universally distributed throughout their substance are those of the possession, on the one hand, of a peculiar form, which we call organic, and, on the other, a peculiar chemical constitution. It is by keeping in mind these two characters, both of which are essential to the idea of organisation, that we arrive at an acquaintance with the two corresponding orders of biological elements. The first order consists of formal or anatomical elements, these being the last bodies, in respect of simplicity of *form*, to be arrived at by anatomical means, and which, at the same time, possess morphological characters which are not to be found in

nature, otherwise than in organised beings. The second order consists of what are called the *immediate principles*, being the last bodies to which we can arrive, without the employment of means capable of producing chemical decomposition, and the mode of existence of which is without analogue in the mineral kingdom, inasmuch as they form integrant parts of living structures.

In correspondence with this method of viewing the relation of the science of biology to the immediate principles, it must be admitted that the study of these last, both as respects their chemical characters and much more directly as respects the part they play in vital phenomena, belongs exclusively to the biologist. There exists no more immediate connection between the study of the "immediate principles" and the science of chemistry than between physics and the study of elementary organic forms, although no one will deny for a moment that the knowledge of chemistry is as essential to the biologist as that of physics. Of this doctrine, which has been alike overlooked, much to the prejudice of science, by chemists and physiologists, it is the object of the work before us to assert the importance. The title of the book is objectionable, as it fails to indicate with precision either its origin or its object. It is not a treatise on chemistry, it is a treatise on general anatomy and physiology. The "immediate principles" form the entire subject of the work, being described in the conditions under which they actually exist in the organism, and in relation to the functions which they actually perform. We should have much preferred that the work should have been entitled, according to the suggestion of a talented writer in the "Union Médicale," a treatise "On the Physical, Chemical, and Dynamical Properties of the Normal and Morbid Immediate Principles which constitute the Human Body and that of the Mammalia." For, in point of fact, as we shall find in pursuing our examination, it is one of the avowed objects of M. Verdeil to show that the subjects treated of are not included in the science of chemistry. He has especially endeavoured to avoid falling into the errors of that so-called "vital chemistry" which is now so prevalent, —a surreptitious science, which would endeavour by algebraic formulæ to represent the mode in which the most important functions of the economy are accomplished, and to submit to calculation the most inscrutable and intimate of the changes which take place in living beings.

In commencing the sketch we propose to attempt of the general arrangement of the work, there are one or two points worthy of being noticed in a preliminary manner. In the first place, it is evidently undertaken by its two authors with somewhat different objects. M. Verdeil is a physiologist as well as a philosopher, an original observer as well as an original thinker, and it is from him that all the facts newly acquired to science which are contained in the work are derived. M. Robin, who is well known to many readers of this Journal, is a disciple of the positive school, and is rather

occupied with the relation of biology to "la haute philosophie" than with actual investigation.

The first volume, of no less than 723 pages, is altogether devoted to the consideration of the immediate principles in general. It is for the most part contributed by M. Robin. In the first place, 100 pages, under the head of *Prolegomena*, are taken up with the demonstration that the study of the immediate principles is included in biology, and the discussion of the relation of that science to the other sciences of observation. With reference to this part the only remark which we can make is, that, as it appears to us, it might with ease have been condensed to one-tenth of its bulk, without losing in value. In a similar manner we feel obliged to pass over the first chapter, which contains 190 pages, and treats of the *character* of the proximate principles in general, as prefatory to the discussion of classification. These characters are discussed in their variations according to sex, age, race, different species of animals, and abnormal conditions; and are themselves sub-divided into five orders,—namely, mathematical, physical, chemical, organoleptic, and organic. It seems to us that this multiplication of sub-divisions is decidedly objectionable; and that two out of the five orders—the first and fourth—might have been dispensed with. By an anomaly not very easily explained, one of the most important questions, that of the formation of the immediate principles, is discussed incidentally in this chapter, under the head of "variations according to age"! The third chapter is devoted to the development of the classification itself. In this respect the authors have succeeded perfectly. Setting out on the admitted principle, that every classification must be founded on the comparison of the *ensemble* of the characters presented by the bodies to be classified, MM. Robin and Verdeil have indicated three fundamental and very natural groups. Of these the first comprises all the immediate principles which are either crystallisable or volatile without decomposition,—a character which corresponds to their possessing a definite chemical composition, and being capable of combining with similar chemical compounds or elements in definite proportions; the second includes bodies which are neither crystallisable nor volatile without decomposition, and which possess, correspondingly, an indefinite chemical constitution. This group is formed of the albuminous and gelatiniferous compounds, which are extremely numerous, and, as respects their chemical composition and characters, pass into each other by insensible transitions.

The first group very naturally separates itself into two sub-divisions, of which the one includes the principles of mineral origin, and the other those of organic. The former of these enter the organism from without, in the same chemical form that they possess while constituting a part of it. The others occur only in organised structures, and their formation is a phenomenon of life. By an anomaly of arrangement, which has been already mentioned, we have to refer

for the justification of these divisions to the preceding chapter. There, in fact, we find discussed one of the most important questions in the history of the immediate principles—that of their formation and mode of termination. Let us endeavour to penetrate the views of our authors on this point. We shall find that the divisions which we have indicated above correspond as completely to distinctions in the dynamical as in the physical or chemical characters of the immediate principles.

With respect to the principles of the first group, it is to be observed that they generally remain in the condition in which they existed exteriorly. Of these some, such as phosphate, carbonate of lime, silica, etc., do not, however, remain in the body in the liquid form, but, in the condition of what has been termed incrustation, combine with the substance of various tissues. In this respect they are not exceptions to the general facts mentioned above, inasmuch as their chemical, though not their organic, condition is the same in the mineral kingdom. All these substances, which are insoluble in water, must obviously enter the organism in a state of liquefaction or solution. The method in which this is effected has been illustrated by M. Verdeil, in researches which were communicated to the Academy of Sciences only last summer. M. Verdeil's investigations, which were conducted, in conjunction with M. Risler, at the Government School of Agriculture, were directed to the solution of the question of the mode in which silica, carbonate of lime, and other insoluble substances, enter into the tissues of vegetables. M. Verdeil has discovered that in all fertile soils there exists a substance, previously undescribed, soluble in water, possessed of very peculiar characters. This substance is easily obtained from the soil by percolation with distilled water, and slow evaporation of the filtered solution. It contains, as immediate principles, first, a compound analogous to sugar in properties and composition, which forms in general from 40 per cent. to 50 per cent. of the dried extract; and secondly, various mineral principles, several of which are perfectly insoluble in water,—as, for example, silica, which in some soils is found in the proportion of more than 16 per cent. of the dried extract, and carbonate of lime in that of more than 20 per cent. There could be no difficulty in referring the solubility of the substances in question to the presence of the organic principle; and M. Verdeil has since discovered, that it is possible to produce artificially a similar result by employing a solution either of grape sugar, cane sugar, or dextrine, which is capable of dissolving silica, phosphate and carbonate of lime, etc. The experiment is rendered even more conclusive by the observation, that no sooner is the sugar of the solutions in question allowed to undergo putrefaction, than the silica, or other insoluble principle, is at once precipitated. It appears to us that, not only with respect to the nutrition of plants, but in their bearings on the physiology of absorption in animals, and

their practical applications in dietetics and therapeutics, the facts demonstrated by M. Verdeil may probably be the first of a very important series.

The immediate principles of the second group, to which the name of organic substances has been given, are represented in animals by the so-called albuminous or protein compounds. The process of their formation, to which the term assimilation has been assigned, is one the analogue of which is not to be found otherwise than in connection with organisation. This process is divisible into two stages. Of these the first corresponds to the development of the compounds in question, at the expense of the mineral principles or of their elements. This development occurs exclusively in vegetables. The second stage corresponds to the transformation of the nitrogenous principles for the condition in which they exist in the food of animals, by a series of isomeric metamorphoses, to that in which they combine to form the living tissues. Of the nature of the first stage it is impossible, in the present state of science, to form an adequate conception, inasmuch as we are unable to compare it with any other process in nature with which we are acquainted, and, for want of observations made upon plants at different stages of their development, we are altogether without information as to its concurrent phenomena. The only undoubted fact which we can boast of is, that the organic substances cannot be formed otherwise than in living beings. The grounds seem, to us, at present insufficient for the admission of the doctrine which was first indicated by M. Robin, in his "Tableaux d'Anatomie," three years ago, and which has received its further development in the present work, of the analogy between the process in question and certain forms of chemical change dependent on catalysis. In supporting this doctrine, we think that it may be shown that our authors have neglected the warnings, so often repeated by themselves, of the danger of deriving explanations of organic phenomena from supposed correspondences with molecular acts occurring in the inorganic world. M. Robin observes :—

"The act of the formation of elements which gives rise to the non-crystallisable principle can only take place, in so far as these materials exist, in the presence of a body which, without furnishing anything itself, is however necessary to the accomplishment of the combination."

This we admit, and in so far we also admit a certain correspondence between the two processes. But let us follow M. Robin into the details of his analogy, and we shall find that it will not bear examination. He further remarks :—

"The catalytic body is the already existing organic substance, similar or analogous to that which is in process of formation."

In what example of catalysis do we find a catalytic body similar to that which is in process of formation? In every case the catalytic body is of a different nature from the ultimate result of the

process. Where is the similarity between diastase and glucose, or between the latter and sulphuric acid? Where, again, is the similarity between caseine and lactic acid, the ultimate result of the lactic catalysis? We, in fact, see no reason whatever to believe that the already existing organic principles stand in a relation to the process of the new formation of principles similar to themselves, which corresponds in the slightest degree to the relation of a ferment or catalytic body to the catalytic molecular changes produced, independent of vital phenomena. Not a single example is to be found of the production of a new compound in the course of such a process, differing in chemical composition from all previously existing compounds in the same situation, except the catalytic body itself. We are, in short, under the necessity of returning to the admission with which we set out,—namely, that organic substances, when in their dynamic condition, are endowed with the power of causing the elements of principles derived from the inorganic world to combine so as to form substances similar to themselves. The phenomena and subordinate conditions of this process obviously constitute a wide field for research, which to this moment has remained almost untrodden.

Of the transformation of the organic principles, as they occur in the food of the higher animals, into the elements of the living tissues, it is the most remarkable feature, that it is brought about in two consecutive stages, which, in the order of results, are mutually converse to each other. The albuminous compounds are introduced into the stomach as articles of food, in the form in which they exist in the tissues of plants or animals (albumen, caseine, musculine); in the intestine they assume an intermediate form, under which they enter into the blood; while, in the capillary circulation, they regain the properties which they originally possessed. Thus we have, in the case of the ingestion, muscular fibre, musculine as the point of departure, albuminose[1] as the intervening condition, and musculine again as the ultimate result. The phenomena of the first stage of the process have been well observed; and we are able in so far to explain them, as we can compare them to changes which take place outside of living organisms. We are certain the action of acids similar to that of the gastric juice actually does produce the same changes in albuminous substances submitted to its influence, under the same conditions of temperature and dilutions, whether the operation be performed in the stomach or in the laboratory of the chemist. We know also that, under similar conditions, the substance called

[1] The term "Albuminose" was first applied by M. Mialhe ("Mém. sur la Digestion et l'Assimilation des Mat. Albuminoïdes," Comptes Rendus, 1846, t. xxiii. p. 260) to the form to which all the albuminous substances are reduced in the small intestines, and under which they enter the blood by the portal veins. It is incapable of coagulation, either by heat or by the action of a ferment, such as rennet. The precipitate, which is obtained on the addition of alcohol to its solution, is soluble in water.

pepsine is capable of producing the same result outside as inside of the intestinal canal. There seems no reason to dispute the correspondence between this process and that of the conversion of gluten and other analogous substances into diastase, as the point of departure, and the result produced, as well as the essential conditions, are similar. Admitting, however, that the primary digestion of the albuminous compounds is a catalytic phenomenon, we do not see how it is possible, at the same time, to admit that the converse process, — namely, the transformation of albuminose into albumen, fibrine, caseine, musculine, etc., can be explained in the same manner, or can be supposed to be of the same nature. Here we have no correspondence, either in the ultimate result or in the existing conditions. Here, again, we find a process of assimilation without analogue, and which occurs only in living beings. The same remarks apply to it as to the original development of the albuminous compounds, which we have already considered, as it occurs in vegetables.

MM. Robin and Verdeil consider all the immediate principles of the second group, those, namely, which possess a definite chemical composition, and do not exist in nature otherwise than inorganised beings, as products of disassimilation, or, in other words, of the decombination of the highest results of the assimilative process. The conclusions which we have already arrived at, with respect to the nature of this process, being admitted, it is obvious that this must actually be the case. The principles in question do not, in general, form constituent parts of the substance of the tissues, but represent the forms into which the organic principles resolve themselves, for the purpose of separation from the organism.

It is obviously impossible, in the present state of science, to indicate the very limited conclusions which the scattered facts in our possession enable us to admit, in a general view, of what may be called the disassimilative process. It may be stated the albuminous compounds correspond to the point of departure, while the ultimate results are numerous, and differ widely from each other. We shall, we think, avoid the omission of any fact of importance in discussing, as separate divisions of our subject, the mode of origin, in the first place, of the crystallisable immediate principles containing nitrogen; secondly, the fatty compounds, the sugars, and other principles derived from them. We do not, however, engage to confine ourselves strictly to this arrangement.

It will be denied by no one, that all the azotised principles of definite composition which exist in the economy are derived from the disintegration of the organic substances, and that they correspond to different stages in a process by which the elements of these compounds are restored to the inorganic world. Of the order of succession of these stages we as yet know very little. We are certain, however, that several of these principles, as, *e.g.*, urea, creatine, creatinine, etc., are actually ultimate results, as far as the disassimi-

lative process is concerned, or, in other words, they are destined to pass out of the organism without undergoing further change. With respect to urea, we cannot determine whether it is derived from the albuminous compounds, as the result of one or of several transformations. As to creatine and creatinine, we may conclude, from their constant association with muscular fibre, that they owe their origin specially to musculine. Another fact, to the knowledge of which the invaluable researches of M. Bernard have led us, is that of the formation of grape sugar, from the transformation of the albuminous compounds in the liver, to which may be added the proof of the similar origin of the neutral fats, which has been almost attained by the same observer. This formation is probably to be considered as collateral with that of the azotised crystalline principles. It can hardly be denied, that the first step in the process must consist in the splitting of the complicated organic compounds into two or more principles, probably of definite composition; and if this be admitted, we may suppose that if, on the one hand, we have the formation of an unazotised body, such as grape sugar, we shall ultimately find that, on the other, there originates simultaneously with it a principle containing nitrogen. However this may be, it is evident that all of the phenomena to which allusion has been made, and many others yet to be observed, must be taken into account before we shall be able to form a correct conception of the general nature of the disassimilative process.

We are perfectly aware that, in accordance with the doctrines of the so-called "vital chemistry" of the day, the formation of urea, uric acid, and other similar compounds, constitutes, in each case, a stage in a supposed process of combustion, of which the albuminous compounds are the subjects, and carbonic acid and ammonia the ultimate results. It is one of the principal objects of MM. Robin and Verdeil to combat this theory, which has exercised so remarkable an influence on physiology, and is looked upon, not only by chemists, but, in a great measure, even by anatomists and physicians, as the great key to the explanation of all the mysteries of living beings. Let us, before proceeding further, examine, with our authors, the mode in which this theory originated, and the reasons which can be given to account for its present ascendancy.

Every one knows that it was Lavoisier who first demonstrated that oxygen, or, as he called it, "vital air," is absorbed during respiration. He believed, but did not demonstrate, that the decomposition of this vital air gives rise to the production of animal heat, water, and carbonic acid. At this day the theory remains, although modified according to the new facts which have been since discovered. It has now been shown that in the higher animals carbonic acid is set free in the proportion of from 70 to 90 per cent. to the oxygen absorbed. Further, the experiments of Magnus, the most important in their results since those of Lavoisier, have shown that this last was mistaken in supposing that the development of carbonic acid and

water takes place in the lungs, but that both oxygen and carbonic acid exist in solution in the blood, and that consequently the latter enters into the combinations for which it is destined in the capillary system. Oxygen exists in the arterial blood in a proportion more than double of that in which it exists in the venous blood. Carbonic acid also rather preponderates in the venous blood, when compared with the arterial. Oxygen, according to the researches of Berzelius, is held in solution principally by the globules which, in common with all organised structures, whether dead or living, as was first shown by Spallanzani, have an extraordinary power of absorbing and fixing this element.

The foregoing constitute all the facts which we are possessed of in relation to the respiratory changes in the blood. We have, at one end of a series of molecular acts of the most complicated character, a highly oxidised product, and, at the other, oxygen. While we admit this as an undoubted fact, we do not, at the same time, admit that each of these intermediate changes is an act of combination. We do not admit, for example, that urea only differs from uric acid in being more thoroughly burnt than it, or that the essential dynamical distinction between albumen and the neutral fats is identical with that which exists between the noble and base metals in the mineral world,—namely, that the one is much more easily oxidised than the other.[1] Between the fixation of oxygen and the exhalation of carbonic acid, a succession of other acts is brought about, which, taken individually, are much more complicated than those to which, in its ordinary and historical sense (the production of flame), or even in its subjective sense, the term combustion is applied.

If, commencing with carbonic acid, we endeavour to follow backwards the series to which we have alluded, the first point at which we shall be arrested will be that of the alkaline carbonates, as it is certain that a great part, if not all, of the carbonic acid exhaled from the pulmonary surface, is derived from the decomposition of these salts. In process of time it may probably be established, that the alkaline re-action of the blood is owing exclusively to the alkaline carbonates, although the question remains yet to be determined positively. It is upon facts derived from the analysis of the ashes of the blood, that Liebig has founded the conclusion which he states in the "New Letters," published only last year, that in this fluid in man, and in general in all more or less carnivorous animals, alkaline carbonates do not exist. It is a fact, however, admitted by all, especially with respect to alkaline carbonates and phosphates, that, according to the method of incineration employed, great difference may be found in the constitution of the ashes of the same animal substance (see Lehmann, Bd. ii. s. 240). It is maintained by Liebig, that in man and the *carnivora* the alkalinity of the blood is owing not to carbonate of soda, but to the tribasic phosphate ($3Na\,0, P0_5$).

[1] See Liebig Nouvelles Lettres, Edition Française, p. 140.

The existence of this salt in the blood is, however, by no means proved; for it is certain that, during incineration, if both the carbonate of soda, and the neutral phosphate of soda ($2Na\,0, H0, P0_5$), a re-action will take place, carbonic acid will be disengaged, and the tribasic phosphate formed. All the expedients which have been hitherto devised to obviate this fallacy have proved more or less futile; and the only means which is left us to ascertain the condition in carbonates and other salts, capable of alteration by the process of incineration, really exist in the living organism, is that to which M. Verdeil has given so much prominence, and of the value of which he has given so many proofs in the work before us. This method consists in causing the salts in question to crystallise, by slow evaporation of the liquid or extract, after the removal of the albumen by coagulation. In this place we content ourselves with a mere allusion to it, reserving details for a future occasion. Admitting that alkaline carbonates exist in the blood of all animals in considerable quantity, as has been proved with respect to a certain number, as we have endeavoured to point out, and has not been disproved with respect to any, we have to take into consideration another class of immediate principles before we shall be enabled to assign them their true place in the series at present in question. We allude to the organic acids, whether introduced from without (vegetable acids) or of internal origin. We know that the neutral potash and soda salts of the vegetable acids, when received into the blood, undergo a series of changes which result in their conversion into the corresponding carbonates. This takes place especially in herbivorous animals, in which the carbonates thus produced are excreted by the kidneys, and appear in large quantities in the urine. If, on the other hand, free organic acids be ingested as food, as is obviously constantly taking place in all herbivora, the existing carbonates are decomposed, carbonic acid is set free, and a salt of the organic acid formed, which itself undergoes the process of change mentioned above. If, however, there be an excess of the acid in question, it passes out of the organism, and appears in the urine unchanged, it being a fact which may be considered as demonstrated, that no organic acid is capable of being decomposed in the blood, or, in the language of the chemists, burnt, otherwise than in the condition of an alkaline salt.

Without, however, referring to the ingestion of vegetable organic acids as an article of food, a phenomenon of constant occurrence only in the herbivora, we have in all animals the conditions required for the admission of the theory of the direct development of carbonic acid by the decomposition of the alkaline carbonates, in the fact of the existence of free acids as a constant condition of most of the tissues in the living organism. The existence of free lactic acid in the muscular tissue is already a fact familiar to physiologists. The acid re-action of the kidney is well known, but not altogether accounted for. The fact, however, which has above all tended to the corroboration of the theory, is that of the constant occurrence of a free

acid, of well-defined properties, in the lungs of all animals which have been hitherto examined. This acid, which was discovered only a year ago by M. Verdeil, who is at present engaged in investigating its chemical characters, exists in the free state as a constituent of the tissue of the lung, to which it communicates a very well-marked acid re-action. It crystallises from its alcoholic solution in extremely beautiful groups of prisms, which usually radiate from a common centre, each terminating in a pointed extremity. Along with the free acid, M. Verdeil has demonstrated the presence of its soda-salt, which he considers to be produced by the decomposition of the alkaline carbonates, in accordance with the theory to which we have already drawn attention. Pumic acid, like lactic acid, which probably performs the same functions in the economy, disappears in the blood after assuming the form of its soda-salt. The presence of the acid itself has been demonstrated in the blood, but it does not exist in the urine. Of the mode of its origin, M. Verdeil observes that, like the acids of the bile, it is obviously a product of the decomposition of a more complicated compound, although he is unable to indicate its nature, or that of other immediate principles, which may be supposed to originate at the same time with it.

Unfortunately, with respect to a large number of the immediate principles of definite composition, our existing knowledge does not allow us to express ourselves in a more determined manner. We know that numerous compounds, differing widely enough in characters, such as, for example, grape sugar, the neutral fats, creatine, urea, etc., are all derived more or less directly from the albuminous compounds. Considering that the latter are more complicated in their chemical composition than the former, we are justified in believing that the transition must take place by the splitting of the more complicated body into two or a greater number of more simple ones. We may also conclude, that this is rather brought about by a series of successive changes, and in a gradual manner, than by an action of so violent a nature as that to which we unavoidably attach the term combustion.

In a succeeding paper we shall complete the remarks which have preceded on the mode of origin and disappearance of the second group of immediate principles in the living organism; and, in conclusion, we shall endeavour, in the first place, to indicate the method which is to be employed in the separation of the immediate principles, and in the investigation especially of their dynamical characters; and, secondly, we shall discuss the nature of the evidence on which we ought to depend, in assigning to any chemical compound the place of an immediate principle, as well as the sources of fallacy to be guarded against.

(*To be continued.*)

An Essay on the Action of Medicines in the System, or "on the Mode in which Therapeutic Agents introduced into the Stomach produce their peculiar Effects on the Animal Economy," being the Prize Essay to which the Medical Society of London awarded the Fothergillian Gold Medal for 1851. By FREDERICK WILLIAM HEADLAND, B.A., M.R.C.S., etc. 8vo. London: Churchill. 1852. —(*Continued from page* 153.)

IN pursuance of our examination of Mr Headland's therapeutical doctrines, we come, now that we have traced his remedies into the blood, to inquire what effects they there produce. We shall allow our author to state his views almost entirely in his own words:—

"PROP. VII.—*That a first class of medicines, called* HÆMATICS, *act while in the blood, which they influence. That their action is permanent.*

"1. *That of these some, called* RESTORATIVES, *act by supplying, or causing to be supplied, a material wanting, and may remain in the blood.*

"2. *That others, called* CATALYTICS, *act so as to counteract a morbid material or process, and must pass out of the body.*

"Supposing that a medicine has fairly passed into the blood, and circulates round with it, there are now two ways in which it may behave itself.

"In the first place, it may have a tendency towards some tissues or parts of the body, on which to exert its powers, as the nerves, or the glands, or muscular fibre, and may use the blood only as a vehicle by which most readily and easily to attain to these. Such are neurotics, astringents, and eliminatives. They may not affect the blood, but they must pass through it.

"But there is another and still more important class of medicines, whose action is particularly directed towards the blood itself. The blood, after their action, is different from what it was before. It may be a change for the better or for the worse, but there certainly is a change. Medical authors, with few exceptions, have been very backward to acknowledge the existence of medicines of this description. But even those who would fain have classed all medicines as stimulants or sedatives, differing only in the kind or degree of their action on the nervous system, have in many cases been obliged to confess, that there is a set of remedies which they call 'Alteratives,' whose action, though slower, is more certain and more durable than that of the former. It is allowed that they alter the condition of the blood. To suppose that they do so by first influencing the nerves, is to adopt a circuitous and uncalled-for explanation. It is proved that they pass into the blood. It is known that when actually applied to nerves, they do not affect them. From these considerations merely, without further evidence, it would seem tolerably clear that they act by influencing the blood itself, simply and solely. But this it will be my business to prove more at length directly.

"Such medicines, then, I have designated hæmatics, a simple and expressive term which has been used by others before me.

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"Hæmatics are very numerous and very important. I shall thus devote some space to their consideration. But I must first lay down a broad distinction between the two divisions of hæmatic medicines. The diseases in which they are used appear all to originate in the blood, however they may manifest themselves.

"Now some of these diseases originate in a want of some principles or constituent of the blood, which want causes an aberration of the vital functions.

"Thus in anæmia there is a deficiency of the hæmatosin of the blood corpuscles. In simple debility a want of a similar nature probably exists. In rheu-

matic fever and other disorders an excess of acid is formed and eliminated, possibly from a want of the alkali by which it should be neutralised. In common inflammatory fever there is an abnormal oxidation of the proteinaceous compounds, possibly arising, as we shall see hereafter, from a failure of some principles which are the proper food of the oxygen. In diseases causing urinary deposits, there is a want of those principles which should naturally retain them in solution. In typhoid fever there is said to be an excess of basic matter, and a deficiency of acid, in the blood. In pulmonary phthisis there is a deficiency of fat in the system. In the latter stage of malignant cholera there is an absence of watery particles in the blood. Some suppose that in scurvy there is a want of the salts of potash in the blood.

"These diseases, then, in some of which the want is proved, in others partly hypothetical, may be treated by medicines which supply the deficient matter, and thus restore a right state of things. They may supply it to the blood directly, or else cause it to be generated there. The former of these modes of restoration seems to be the most frequent, and may possibly, when we shall know more of such matters, be found to occur in all cases. This division of hæmatics I have named restoratives (restaurantia). Their action, as we shall see, is in some cases apparent, in others more obscure. They restore the blood directly to its proper condition, if there is only a deficiency; but they do not in general seem to have the power of counteracting any morbid or active material that may exist in the blood. Nor do they, except in large doses, exert themselves any peculiar action on that fluid. In these respects they differ from the other division of hæmatics. They also differ in another important character. Each restorative has in healthy blood a substance analogous to, or identical with, itself. It replaces this when deficient.

"Not so with other Hæmatics. There is in general nothing in the blood corresponding to them—or if there be in some cases, they are not introduced with the intention of supplying its want. Thus Restoratives may remain in the system, and are intended so to do; but these may not remain. They must pass out. In so doing they come under the head of eliminatives, or that of astringents. This is their secondary action, distinct from their primary and most important operation. What then is the curative action of these remedies?

"A large class of diseases depends on the presence in the blood of a morbid material, or, what amounts to the same thing, on the constant working of a morbid process in that fluid. Some of these, as the eruptive fevers, will run a certain course, and then come to an end. These we cannot generally stop, but can only alleviate. But others, more in number, and more commonly met with, tend naturally to run on for an indefinite period, unless by any means we can arrest their progress. Some depend on a contagious virus, communicable from one person to another, as syphilis. Some, as ague, are dependant on atmospheric or terrestrial influences. Others are due to some derangement of the secondary assimilative processes, as scrofula, scurvy, gout, and rheumatism. Others again, to causes that are ill understood, as convulsive disorders and skin diseases. Lastly, some may be caused in many different ways, as common inflammatory fever.

"Now the object in the treatment of such diseases is to obtain in each case some remedy that shall be able to counteract this process, something that shall destroy the morbid influence at work, and thus restore health. Medicines that are used with this intention form the second division of hæmatics, which I have named catalytics (catalytica) from a Greek verb, signifying to destroy or to unbind."—Pp. 94–98.

It appears, we think, from these extracts, that Mr Headland is a very decided example of the partizans of the humoral pathology and consequent therapeutics which prevail so much at present in the schools. It is true that he gives at page 101 a disclaimer of his [illegible] a follower of the old humoral school, who attributed every-

thing to peccant humours; but in reality this does not amount to much, as his own words will testify.

"It must not be inferred, from what has been said on the importance of blood-medicines, that I am disposed to agree with those who would account for all diseases by some fault in the humours of the body. For though we know that many cases of convulsive disorder, as of hysteria, chorea, and epilepsy, may be reasonably accounted for by supposing the existence of a wrong in the blood, which has affected the nervous system, yet there are doubtless many nervous affections which are primarily independent of the blood, which can only be treated by neurotic medicines, and even then often without benefit."—P. 101.

We frankly confess that modern humoralism has, for a long while, been to us a subject of some anxiety; for we fear that its tendency is to exert an influence very unfavourable to the ultimate progress of pathology and therapeutics. At the same time, we are quite prepared to admit that we have derived, and are deriving from it, benefits which are not to be despised by us as practical physicians. There can be no doubt that in a great many, we might almost say in most, diseases, the blood at one time or another is in a state which is not normal; but the recognition of this most interesting and important fact has led too many writers of the present day to confound this which is only a phenomenon of disease with the pathological cause of disease; and when they can demonstrate that the blood is not quite what it ought to be, they have been too ready at once to jump to the conclusion, that the malady in question is a "blood disorder," and therefore that the remedies which cure it are hæmatic medicines. Nay, not content with this, they assume many diseases to be of blood origin, in which nothing abnormal can be demonstrated in the blood at all; and take refuge in the assumption, that there is circulating in this vital fluid a poison of some kind, but which is too subtle for our present limited powers of observation to perceive. Mr Headland appears to partake largely of these ideas, when he tells us that the diseases in which his so-called hæmatics are used, "appear all to originate in the blood, however they may manifest themselves;" and when, at page 173, he says, "the inflammatory process, the chief seat of which, however it may originate, is on the circulating blood."

Now this is precisely the pathological doctrine with which we profess ourselves dissatisfied, and which feeling we consequently extend to the therapeutics founded thereupon. We say it is most important and essential to us, as practical men, to be aware of these changes in the blood; they are of the greatest consequence to us in the exercise of our art; they must be especially borne in mind by us in arranging the dietetical and hygienic management of our cases, and are not to be neglected in planning their medicinal treatment. But as pathologists and therapeutists, we must look not only at them, but beyond them; and put to ourselves the question—How do these morbid conditions of the blood arise? Is it really true that such and such disease arises in the blood, or is not this change in the blood only a

result of something which is previously wrong in the living solids? And, consequently, is it true that our remedies act only hæmatically by adding something to the blood or subtracting something from it, or do they not rather, in curing disease, exert their influence on the living solids, and, by controlling or modifying their actions, bring back the blood indirectly, but not less surely, to its normal state? We would carefully guard ourselves against the imputation of denying altogether the existence of blood diseases and hæmatic remedies, which would be absurd with the example of anæmia and chalybeates before our eyes. What we protest against is the unwarranted extension of this doctrine. We are quite prepared to admit it to a certain extent. There are such things as diseases, if not essentially originating in the blood, at least inseparably connected with a depraved state of that fluid, and most appropriately treated by medicines which restore it to a healthy condition; but we believe that their real number is very small, and consequently that the number of medicines which are entitled to be considered as true hæmatics is very small also.

But this is a question which has a much higher import than merely to settle the *modus operandi* of certain individual remedies or classes of remedies. It involves the whole foundation of therapeutics as a science. If we are to follow Mr Headland, and the school which he represents, and are to believe that a large proportion of remedies, and these the most important in the Materia Medica, cure diseases merely by the process of supplying some deficient, or removing some redundant, matter in the blood, we look upon the living organic solids in these instances as being little else than passive, and their vital properties,—physiological or pathological—need give us comparatively small concern. Therapeutics thus becomes reduced to little better than a department of applied chemistry; our remedies are merely a set of re-agents, and our treatment of disease a process of synthesis or decomposition with which the living body scarcely has more to do than the flask of the chemist has with the compounds which he concocts within it. And this is very much the result to which the over-zealous chemical school of the present day, now in the ascendant, are leading us.

If we had nothing else to depend upon than the modern humoralism, the hopes of the advent of a great era in therapeutics, formerly expressed by us, would be small. We owe much to modern chemistry for what it has taught us as to the fluids of the body; but at the most it has unveiled to us only some of the phenomena of disease; it has taught us but little of its essential pathology. It is not to her, therefore, that we look hopefully for great general doctrines as to the real nature of disease, and the real *methodus medendi*, except so far as she is the handmaid of physiology. It is from physiology that we expect some important revelation of the vital actions of our bodies, which shall form the basis of a scientific pathology and a scientific therapeutics. It is to attain this vantage ground that the battle of

science must be fought, and modern "rampant humoralism" opposed and kept in check by a vigorous defence of solidism.

If we wished to illustrate what we conceive to be the unfortunate tendency which this humoralistic view of the nature of so many diseases and the action of so many remedies has, to arrest the full development of therapeutics as a science, we could not find anything more to our purpose than the two following sentences of Mr Headland's book, which show in the instance of this acute and reflecting writer, that the mind which can rest contented with these doctrines runs great risk of losing sight of the importance of attaining, or indeed of aiming at, general principles, which alone can effectually raise therapeutics to its proper position as a science.

Speaking of Catalytic Hæmatics generally, he says at p. 98—

"Some have tried to give a general explanation of their action, and have talked of it as if it were easily understood by their known properties. I am not of their opinion; and when I speak of Catalytics, I shall give my reasons for disagreeing with them. Now each Catalytic has peculiarities and affinities that distinguish it from all others. I have not thought that I could arrange them more truthfully than by subdividing them according to the diseases which they tend to counteract. How inadequately is the action of Mercury and of Iodine expressed by calling them special stimulants, alteratives, or absorbents! Is it not better and more correct to say at once that Mercury is useful in checking inflammation in general, and in counteracting the poison of Syphilis in particular? and that Iodine is effective in secondary forms of the latter disorder, as well as in Scrofula?

"These medicines, then, are specifics, in so far as they are particularly useful in certain disorders, and in those excel other remedies; but they are not, in the vulgar acceptation of that term, the only medicines which can be employed in such a disease, nor is their use to be restricted to it alone."

And again in propounding his classification, at p. 102 he adds a foot note to this effect—

"The details of this arrangement are founded solely upon the *therapeutic operation* of medicines, as used to cure diseases, and not in any way upon their *physiological action* upon a healthy man. Much unnecessary confusion, in works on Materia Medica, has arisen from these two different matters being taken into account at the same time. Food is the only restoration of wanting material needful to a healthy man; neither is there in such a case any need or opportunity for the counteraction of morbid agencies."

Now to our eyes nothing can be more decidedly retrograde in character than any doctrine which tends to make us regard remedies as specifics even in the sense (the only intelligible sense of the word) as used by Mr Headland. By this we are asked to content ourselves with a set of bare isolated facts, and we are thereby precluded from attempting to trace out any possible connection or similarity in *modus operandi* between this one and that other; each is a specific and nothing more. Mr Headland, who reflects and thinks, although in our opinion somewhat erroneously now and then, may be proof against such a misfortune; but what is so apt to lead weaker brethren into an unreflecting stupid routine style of practice, than the idea that such and such a remedy acts specifically on the

materies morbi of such and such a disease? Then, again, his opinion that the considering together of the physiological action of medicines and their curative effects in works of Materia Medica has led only to confusion, appears to us to be exactly the reverse of the truth. We believe that the only remedies, of the *modus operandi* of which we have at this present moment anything approaching to a clear idea, are those which we have been enabled to trace as producing their therapeutical actions in disease in virtue of their physiological action in health. Whenever we acknowledge that we cannot do this and take refuge in the doctrine that their effects are due to a mere specific action on a morbid matter or morbid process, and irrespective of any power of affecting the living tissues as constituents of the living organism, then we make simply a confession of ignorance. Contrast our ideas as to the action of opium and the action of quinine. What enables us with so much freedom and satisfaction to make so many and so varied uses of opium, but that we know its physiological action as a narcotic on the healthy body, and can with tolerable certainty calculate from this its effects on it when diseased? How comparatively limited, on the contrary, our use of quinine, and how unsatisfactory our confession that we are yet ignorant of its physiological action; and that we employ it in a few forms of disease because we know that it has been found to answer by those who have preceded us. Can it be doubted that if we knew as much of the effects in quinine in a healthy body, as we do of those of opium, that we should find for it many valuable uses besides those to which it is now applied?

Had Mr Headland in the note which we have quoted from p. 102, contented himself, *à propos* of classifications, with saying that in making a classification of remedies, we should take either the physiological or therapeutic actions as the basis of our arrangement, and not both, we should agree with him entirely. But when he says that it only confuses works on Materia Medica by taking both into account at the same time, we look upon him as gravely heterodox. Make your classification a good therapeutical one if you can, or make it a physiological one if you choose, but in the one case trace back your therapeutical effects when you possibly can, to a physiological cause for their production; in the other, trace your physiological action forward if you can to its resulting, when applied to a diseased body, in a therapeutical effect. When you cannot do this, honestly confess your ignorance, and keep this in view as a matter to be excogitated and investigated. It is when you take into account, both physiological and therapeutical action, and then only, that you aim at, and can possibly obtain a full elucidation of your subject. It is just because it leads us to fix our eyes solely on the therapeutic, and makes us shut them to the physiological, that we look upon the modern hæmatic humoralism as one of the most obstructive and dangerous shoals (for it is shallow) in the ocean of therapeutics. Oh for better soundings and truer charts!

We have been induced to offer the above remarks, not so much to put forth our ideas upon therapeutics generally, as to save much detailed discussion of the contents of the next 200 pages of the book before us. Our chief object is to continue our abstract of Mr Headland's remaining propositions, and these we shall set before our readers with a very few observations.

We have seen that our author divides his so-called Hæmatics into Restoratives and Catalytics. The following is his arrangement of the former of these :—

"I will divide them into six orders, which are all distinct and characteristic in their mode of action.

RESTAURANTIA.

Ord. 1. Alimenta.
Ord. 2. Acida.
Ord. 3. Alkalia.
Ord. 4. Tonica.
Ord. 5. Chalybeata.
Ord. 6. Solventia.

On reverting to Proposition VII., which treats of the action of the first class of medicines, it will be seen that what has been there stated with respect to the action of Restoratives resolves itself into the following simple affirmations or minor propositions.

m. p. 1.—That they act in the blood, and that their effect is permanent.
m. p. 2.—That there are naturally in the blood substances which resemble or coincide with them.
m. p. 3.—That they are not of necessity excreted, but may remain in the blood.
m. p. 4.—That they are of use when a disease depends on the want of one or more materials in the blood."

On the subject of the first order, Alimenta, we have nothing to say. Their title to the appellation of Restoratives, will not be questioned. Nor do we mean to put in a demurrer to the claims of Chalybeates to each as such. We look upon iron in anæmia, as the best example that can be found of a medicinal substance, acting by directly restoring something awanting in the blood. The truth is, its administration in cases of chlorosis, may in great measure be looked upon rather as dietetical than medicinal, and it is best taken soon after meals, so as to be digested and assimilated with other alimentary principles. We are not however inclined to agree unreservedly to the following remarks of Mr Headland.

"I believe Iron to be simply and solely a Restorative remedy. By improving the condition of the blood, it appears similar in action to Tonics ; but it is not a true Tonic, for neither in debility nor in Ague is it of any service, unless there is at the same time Anæmia. In some cases, even of Anæmia, Iron will not effect a cure. This must be because the appropriative power of the system is so weakened and degenerated, that it is incapable of consigning even this needful substance to its proper destination."

No doubt anæmia is the great indication for its employment, and it is not of much use in simple debility, from exhausting disease for instance, because there we require rather some of those bitters which

more directly increase appetite and sharpen digestion; nor is it rapid enough in its action as an anti-periodic to be of general value in agues uncomplicated with the anæmic state; but in cases of neuralgia, for example, iron has often been found beneficial where no signs of anæmia exist. We are not disposed to infer, that in these instances there must have been a hidden anæmia because iron worked a cure, and we therefore prefer thinking that, in addition to its being a hæmatic restorative, it is a tonic also. Its so-called emmenagogue effects are only an indirect result of its combined restorative and tonic action.

The following passage, in the same page from which we have just quoted, invites a single remark:—

"Anæmia is the one disease in which iron is of use. Purgative medicines form a valuable adjunct to the treatment in most cases. Dr G. Owen Rees has suggested that they may be of use by removing some of the water of the blood, so that the corpuscles, being then shrivelled by exosmosis of their contents, may be in a fitter condition to absorb the 'ferriferous chyle.' And yet, if so, the drinking of a single glass of water would probably be sufficient to swell out the corpuscles again, and thus put a stop to the process. It is just possible that a cathartic may be serviceable by purging away from the blood some effete matters, as sulphuretted hydrogen, formed by the continual decomposition of the tissues, which would have hindered the operation of the chalybeate in the blood, by decomposing it."

We make Mr Headland quite welcome to dispose of Dr Rees' suggestion as he thinks fit, contenting ourselves with merely remarking, that it was a source of amazement to us how so intelligent a physician as Dr Rees could ever have propounded so singular a hypothesis. But what we would now point out is, how thoroughly modern humoralism displays itself in the opinions of both these gentlemen as to the use of cathartics. Instead of hunting in the blood for an explanation, why not look in the bowels themselves? Is it not the simple fact, that chlorotic women, and such anæmic subjects, have not only a very impressible nervous system, but at the same time generally very torpid and ill-conditioned bowels? and is it not much more likely that cathartics are beneficial in removing from their intestines matters prone to set up reflex irritations, than either by creating in the blood-corpuscles an exosmotic vacuum into which ferriferous chyle will be ready to rush, or in removing from the chalybeate medicine so dangerous a neighbour as sulphuretted hydrogen? No doubt, wonderfully fœtid matters will be evacuated under such circumstances; but we question if they ever came from any more distant region than the intestinal canal. We shudder to think what would be the complexion of a chlorotic patient if there were sulphuretted hydrogen encountering iron in the alkaline blood. The disease would then be, we suspect, more appropriately called melanosis, were that not already the patronymic of a much more formidable enemy. We may remark, that in many of that very class of cases to which chalybeates are in general so applicable, the cathartics alone without the iron are no bad remedy. It is not long since we

cured, in the same establishment, two white-faced dressmakers each of a facial neuralgia by merely unloading their costive bowels by a few doses of scammony and calomel. Whether would we be more probably led to the very laudable practice of moderately purging such a patient—by the good old-fashioned Hamiltonian notion of getting offending matters out of the intestines and away from the nervous system, or by the new-fangled idea of altering the shape of blood-corpuscles or extracting iron-precipitating sulphuretted hydrogen from the liquor sanguinis?

Mr Headland's next section, Acids, demands, assuredly a full discussion; for it appears to us to be one of the most interesting problems in therapeutics yet awaiting solution, how acids act on the system. We cannot assent to all his doctrines regarding them, but we cannot in this matter find any great fault with him, for nothing that we have read or cogitated upon the subject has appeared satisfactory to us.

Mr Headland is of opinion that acids enter the blood as such. He rejects the idea of Dr Pereira,[1] that they are neutralised by the alkaline matter of bile, saliva, pancreatic juice, etc., in the alimentary canal before absorption. We are inclined to agree with Mr Headland thus far. But we do not receive from him any satisfactory solution of the difficulty that awaits us, as to how the acids act when they are once in contact with the alkaline fluid, the blood. That their action must be in some respect *quâ* acids, is obvious from the fact, that the same amount of acid, if administered in the form of a neutral salt, does not produce the same effects; but we must say, that the following explanation by Mr Headland does not satisfy us, nor does it even appear plausible, simply because it is founded upon an assumption (excess of alkaline matter in the blood in disease), which we believe to be without foundation:—

"The blood is alkaline; which is due either to the presence of carbonate of soda, or (according to Liebig) of an alkaline phosphate of that base. So that the acid, on entering into the blood, passes at once into combination with this alkali, and the result of this is a general diminution of the amount of basic matter in the system, and an increase in that of acid. Thus a free acid may act as a restorative in cases where there is an excess of alkali in the blood. It may either remain in the blood, after entering into combination, or it may pass off by the urine, supplying there the place of a natural acid, which it leaves behind in the system. It is on such a theory as this that the action of mineral acids in typhoid and putrid fevers has been explained. I do not mean

[1] We cannot mention the name of Dr Pereira without indulging the melancholy pleasure of passing a tribute to his personal and scientific worth. We respected Dr Pereira as an author, and esteemed him as a man,—admiring especially the catholic spirit in which he received the opinions of others, and endeavoured to give their full measure of justice and due weight to all. His sudden and premature death, at the age of forty-nine, will be lamented by all who take an interest in that department of science which he so assiduously and successfully cultivated.

to affirm positively that there is in these cases an excess of alkali in the blood. Although likely, it is not proved. The explanation is plausible."—Pp. 109, 110.

Of the action of acids on the urine Mr Headland says very little, even when discussing them, under the order of solvents, as "anti-phosphates." We allude to this interesting subject only to point it out as one which is most imperfectly understood, or rather not understood at all,—one which would well reward the labour of a patient investigator.

Acids are among the foremost of those substances which are called Refrigerants; and of them, in this capacity, Mr Headland gives the following account:—

"In febrile cases of all kinds it is often found that diluent drinks containing the free vegetable acids, or solutions of their salts with alkalies, act beneficially in lowering the pulse, and in moderating the progress of the disorder. These should both be distinguished from mineral saline drinks; for mineral salts, from their known effects on the blood, would seem to belong simply to my division of catalytics. But the effect of these vegetable acids seems to be to restore the blood to a more natural condition, and this independently of the action of the diluent with which they are administered. Now, of this effect there is no certain explanation; but a theory, which may or may not be true, may be ventured to account for it. Should it prove correct, it would seem that in such cases the free vegetable acid acts as a restorative; and that the alkaline salt of this acid has at first the same action, but adds to it afterwards a catalytic operation. Let us then attempt to clear up this matter.

"It has long been considered probable, but may now be said to have been proved by the researches of M. Becquerel, that in febrile disorders and inflammations there is excreted in the urine an excess of urea and of urate of ammonia, substances which are formed by the oxidation of the nitrogenous tissues. This extra-oxidation probably arises from a deficiency of that matter which is the proper food of oxygen in the system. This, as we have seen, being the step between grape-sugar and carbonic acid, must either be lactic acid, or something similar to it. It must be remembered that no food is usually taken in fever; this would at length quite cut off the usual source of this lactic acid, which is the starch and sugar of the food, and render it necessary that the animal tissues should continue to undergo oxidation, to maintain the animal heat. Now, if we compare the commonly received formula of proteine with those of lactic acid and tartaric acid, it will at once be seen, as was pointed out some time ago by Dr Murray, that the latter contain more oxygen, in proportion to their carbon and hydrogen, than is found in proteine or albumen.

$$\begin{aligned} \text{Proteine} &= C_{40}\, H_{31}\, N_5\, O_{12}. \\ \text{Lactic acid} &= C_6\, H_6\, O_6. \\ \text{Tartaric acid} &= C_8\, H_4\, O_{10} + 2\, HO. \end{aligned}$$

Thus, while for 40 equivalents of carbon, proteine contains only 12 of oxygen; lactic acid contains 40, and tartaric 50, of that element. So it seems that albuminous matters, containing less oxygen, would require much more oxygen for their combustion; this would produce more heat, augment the number of respirations, and keep up the fever. And though it has been proved by Wöhler that free vegetable acids pass out in the urine without having undergone oxidation, yet the condition of fever would probably be an exceptional case. Lactic acid, the natural fuel, being deficient, the alkali with which it should combine must be present in some excess; so it seems likely that a free vegetable acid would combine at once with this alkali as the lactic acid would have done, and thus be burnt or oxidised instead of the latter. Its action would

then be simply restorative. Requiring less oxygen than the proteine would need to transform it into carbonic acid, it would thus diminish the number of the respirations, the frequency of the pulse, the temperature of the body, and in this way allay the fever. It would be strongly confirmatory of this idea if it were proved that the amount of urea and urates in the urine is actually diminished by the use of acid drinks and fruits in febrile cases. This hypothesis can only be admitted on the supposition that fever constitutes an exception to the general rule, that the vegetable acids pass through the blood without undergoing change."—Pp. 111-113.

We need hardly offer any remarks on a hypothesis which requires so unwarranted assumption, as that the behaviour of vegetable acids in the system in fever is exactly the opposite of what occurs in health. We think it a much more important question to ask ourselves, whether the statement made by Mr Headland, in common with most authors, that the so-called refrigerants do lower the pulse and check the progress of fever, be a medical fact or not? Our own observations have certainly never enabled us to witness anything of the kind. The acids are most grateful to the fevered patient, who, by a natural instinct, quite independent of all therapeutic ideas, is constantly asking the doctor if he may not have something "sharp" to drink; and the benefit which the sense of a gratified desire confers, in making the patient more comfortable for a while, is not to be overlooked. We have often endeavoured to observe whether they ever do produce any real diminution of the animal heat, and whether, therefore, their title, refrigerant, is really merited. We have not been able to satisfy ourselves of this; but we know, both from active and passive experience, that they give for the moment a *sense* of coolness, which renders them truly delicious either under a fever or a hot sun. This is true equally of mineral and vegetable acids; equally of hydrochloric acid, which contains no oxygen at all; and of tartaric, the relative amount of oxygen in which is so much insisted on. We have never thought that the chemistry of the matter had anything at all to do with it. We believe that it is at most (if the circulation of the surface be affected at all) only the result of a grateful impression made on the nerves of the throat and stomach; and we are the more inclined to this belief, that the cooling and refreshing effect is always very temporary, and is felt at the moment of swallowing the acid drink, and not afterwards, as ought to be the case, were it due to any modification of the amount of inspired oxygen consumed in the system.

(To be continued.)

Part Fourth.

PERISCOPE.

PHYSIOLOGY.

Experimental Researches applied to Physiology and Pathology.

By E. BROWN-SEQUARD, M.D., of Paris.

(*Continued from p.* 178.)

NO. IX.—CERTAIN ACTIONS OF COLD, WARMTH, AND LIGHT UPON THE CRYSTALINE LENS.

Pourfour du Petit has discovered that after the death of a mammal, it is not uncommon to find the crystaline lens opaque. He has found also, that when the lens has become opaque, if we draw it near the flame of a candle or a lamp, it becomes transparent again after a few minutes. The same lens, put alternately near and far from the flame, may become alternately transparent and opaque. I have endeavoured to discover whether it is the light or the warmth of the flame which renders an opaque lens transparent. I have placed between the lens and the flame a layer of mineral salt, which is athermane and transparent. Then the light of the flame could reach the lens, but not its warmth. There has been no action. In another experiment I have exposed to the light passing through the mineral salt a fresh crystaline lens, still perfectly transparent. It became opaque. Now, in a third experiment, I have compared two crystaline lenses, perfectly transparent, one exposed to the action of light passing through the mineral salt, and the other kept in an obscure place. The former became opaque much quicker, and considerably more than the other. Therefore, 1st. It is not light which renders transparent an opaque lens; 2d. Light does not prevent a transparent lens from becoming opaque; 3d. Moreover, light appears to accelerate, if not to produce, the opacification of the crystaline lens.

As to the influence of warmth, it is certain that it is that which renders transparent an opaque lens: 1st. When left in the atmosphere, at a temperature superior to 70° F. (21° Cs.), a fresh transparent lens remains transparent. 2d. When a lens has become opaque, it is frequently sufficient to keep it exposed to the warmth of the hand, during some minutes, to render it transparent. 3d. The lower the temperature of the atmosphere the quicker transparent lenses become opaque.

These experiments give a very interesting result, *i. e.*, that light appears able to produce an effect precisely opposite to the effect produced by warmth.

From these researches I conclude:—

1st. That light, and a low temperature, are favourable conditions, if not direct causes, of the opacity of the lens in warm-blooded animals after death.

2d. That a warm temperature, *i. e.*, a temperature superior to 70° F. (21° Cs.) is a cause of the change occurring in the lens, by which, when opaque, it becomes transparent.

I will add, that sometimes an analogous opacity takes place in the cornea.

X.—ON THE NORMAL DEGREE OF THE TEMPERATURE OF MAN.

The degree of animal heat, in the human species, is stated as being between 98.°5 and 100° F. (37 and 38° Cs.) I intend to prove that it is higher.

It is impossible to take directly the temperature of most of the internal parts of the body in man; therefore many physiologists, in order to discover the temperature of these parts, have argued as follows:—According to J. Hunter, the temperature of the rectum in animals is the same as that of the right ventricle of the heart; hence it has been concluded that as the temperature of the rectum, in man—still according to Hunter—is 98°.42 F. (36°.9 Cs.) the temperature of the internal parts of the body ought to be between 98 and 99° F. (36°.67 and 37°.22 Cs.)

Some other physiologists, noting the temperature of the mouth under the tongue, and supposing that this temperature is nearly the same as that of the internal parts of the body, have concluded that the temperature of man is between 99 and 100° F. (37°.2 and 37°.8 Cs.)

We will prove that these deductions are not right:

Firstly, the degree of the temperature of the rectum is not, as Hunter says, 98°.4. It may be so in debilitated men, but in healthy persons it is more elevated. It is between 100 and 102° F. (37°.7 and 38°.89 Cs.) according to my own researches and to those of Berger and Maunoir. Besides, many experiments on dogs, rabbits, and guinea-pigs, have shown satisfactorily to myself that the temperature of the rectum is not equal to that of the right ventricle. This last organ is from 1 to 3°. F (0.56 to 1.7° Cs.) higher than the rectum.

The temperature of the rectum, in man, being between 100 and 102° F. (37°.7 and 38°.8 Cs.), and the temperature of the right ventricle, in man, being from 1 to 3° F. (0.56 to 1.7° Cs.) higher than that of the rectum, if we suppose the same difference existing in man as in mammals, it follows that the temperature of the right ventricle of the heart in man ought to be between 101 and 105° F. (38°.33 and 40°.56 Cs.)

But a closer approximation of the exact temperature of the internal parts of the body, in man, may be obtained by taking the temperature of an organ situated deeper than the rectum, and, consequently, less exposed to the influence of the atmosphere. Such is the case in the bladder. I have observed its temperature by taking that of the urine at the moment of its emission and before any sensible change had occurred in it. The urine was directly received in a vase dipped into a large quantity of water at 98° F. (36°.7 Cs.) Thus I have ascertained that the mean temperature of the urine, in man, is 102°.6 F. (39°.2 Cs.)

Now if we take notice of this well known fact, that the temperature of the lower part of the abdomen is less elevated than the upper part, we are authorised to believe that the temperature of the central parts of the body, in man, is very near 103° F. (39°.5 Cs.)

My experiments on the temperature of urine were made on ten strong sailors, in the spring, on the Atlantic Ocean, between the 43d and 45th deg. of north latitude. The lowest degree of the temperature of urine which I have observed, was 100°.9 F. (38°.3 Cs.); the highest was 103°.2 F. (39°.56 Cs.) My own urine, examined more than thirty times, in the most varied conditions, has been nearly always at the same temperature; the variations have been only between 102° and 102°.8 F. (38°.89 and 39°.33 Cs.) The ordinary degree is 102°.5 F. (39°.17 Cs.)

Long before my researches, the temperature of urine, in the human species, had been taken by some observers, but in general without sufficient care. The temperature of the urine is 94°.25, according to Braun; 98°.9, according to De Lisle; and 103°, according to Hales. Recently Berger has taken the temperature of urine in the bladder in five women. He has found it equal to 101°.48 F. (38°.6 Cs.)

As the temperature of woman is a little inferior to that of man, the result obtained by Berger is in accordance with mine.

If we take the average between the temperature of the urine of man as I have found it, and that of a woman as Berger has found it, we have a number very near 102° F. (38°.9 Cs.)

From these facts we draw the conclusion that the temperature of the thoracic and abdominal viscera, in the human species and in both sexes, is between 102 and 103° F. (38°.89 and 39°.44 Cs.), *i. e.*, some degrees higher than it is generally admitted.

XI.—ON THE INFLUENCE EXERTED UPON THE GENERAL TEMPERATURE OF THE BODY BY A CHANGE IN THE TEMPERATURE OF ONE OF THE EXTREMITIES.

The following sentence is given as an axiom by Dr W. F. Edwards:—"We cannot either raise or lower the temperature of any one part of the body, without all the other parts of the frame being affected, and suffering a corresponding rise or fall in temperature, more or less, according to circumstances." [1]

Expressed in such terms, we accept this law as perfectly true; but the author elsewhere gives an extension to this law, which we will prove to be incorrect. No doubt, when the temperature of the blood, coming to the heart from a remote part of the body, has been modified in that part, the thoracic viscera ought to have their temperature modified; but to what extent? It is on this very important point that we do not agree with Dr Edwards. He was of opinion, that the influence exerted by a small part on all the other parts of the body was considerable. He says that the chilling of a single part, such as the hand or the foot, may cause a loss of temperature in all the other parts of the frame even far beyond what could have been presumed as likely or possible; and that in a number of experiments where one hand was plunged in water cooled down by ice, the other hand, which was not subjected to the action of the cold bath, lost nearly 5° R. (11°.25 F., 6°.25 Cs.) in temperature.

It will be easily understood how important it would be that practitioners should be able to act on the general temperature of a patient, so as to increase or diminish it by means of a hand or a foot bath. I regret to say, that if the facts observed by Edwards are exact, his conclusion nevertheless is incorrect.

I have performed, both alone and with the help of Dr Tholozan, physician of the hospital Val-de-Grace, at Paris, numerous experiments, with the view of knowing whether Dr Edwards was right or not. I have found that the chilling of one hand plunged in water at the temperature of freezing point, acted very strongly on the temperature of the other hand. But, at first, there is no regularity at all in the quantity of degrees of temperature lost by the hand which remains out of the water; and secondly, we have found once that this hand did not lose any fraction of its temperature. In one case we have observed that the hand kept in the atmosphere did lose 22° F. (12°Cs.) in seven minutes. The ordinary loss of temperature has been of between 6° to 3° F. (3°.33 to 4°.44 Cs.) In one case there has been only a loss of 2° F. (1°.2 Cs.) In another case there has been no loss, and, on the contrary, there has been an increase of temperature of 1°.4 F. (0°.8 Cs.)

If, like Edwards, we consider the loss of temperature of the hand not plunged in water as a sign and as a measure of the diminution of the general temperature of the body, we must conclude from our experiments that the chilling of a small part of the body may be unable to diminish the temperature of the body sensibly, and that in other cases it may act extraordinarily upon it. But the supposition, that the hand kept in the atmosphere was able to give a measure of the modification of the body, is incorrect. By taking the temperature of the mouth during the time that one hand was dipped in very cold water, Dr Tholozan and myself have ascertained that the temperature of the body does not sensibly change. The greatest diminution of the temperature of the mouth has been nearly 1° F. (0°.7 Cs.), and this only in one case. In that experiment, in which the hand not plunged in water lost 22° F. (12° Cs.), the temperature of the mouth was not diminished more than the fifth of a Fahr. degree.

As it is quite certain that the temperature of the bod is constantly changing,

[1] Article "Animal Heat," in Todd's Cyclop. of Anat. and Physiol., 1839, t. ii., p. 660.

it is easy to understand why we do not find a small but a constant diminution in the temperature of the mouth, when one hand is subjected to a notable chilling. When, under the unknown influences that are constantly modifying the temperature of the body, there is a tendency to increase that temperature, then the tendency to its diminution originating from the chilling of one hand may exist without producing any effect. We have there two causes operating in various directions, and if they are equal they annihilate each other. If they are unequal, we perceive only the difference between them. When these two causes act in the same direction, then their efforts are added to one another, and it is probably in a circumstance of that kind that I have once found a diminution of 1° F. (0°56 C.) taking place in the mouth.

We have now to examine how the diminution of the temperature of a hand is produced, when the other hand is dipped into cold water. *A priori* it is evident that the chilling of the hand kept in the air exists in consequence, either of the arrival of a cooler blood, or in the diminution in the quantity of blood. That hand, being exposed to a cold air (for it is only in such a case that the experiments succeed), loses its temperature by the action of that cold air. At first, the blood that arrives in the hand is not cooler, as Edwards had supposed it was. This we prove by the fact, that the temperature is but very little changed in the mouth. The supposition then remains, that the quantity of blood arriving in the hand is smaller than usual. This may happen by two modes, one of which is, that the heart sends less blood; and the other, that the blood-vessels of the hand are contracted, and prevent in part the passage of blood. It is certain that the heart continues perceptibly to send the same quantity of blood. Therefore we are induced to admit that the hand's blood-vessels are contracted. But now, what is the cause of that contraction? We will try to show that it is in an action of the nervous system. Every one knows that, under the influence of a sensation or of emotion, the hands, and sometimes the feet, become cold. The nervous system, in consequence of that sensation or emotion, acts upon the blood-vessels and excites them to contract. The calibre of the visible vessels is sensibly diminished. The same phenomena take place in the two hands when one is dipped into very cold water. An exceedingly violent pain is felt, the nervous centres are strongly excited, and they act then as under the influence of an emotion. Dr Tholozan and myself have observed, that the greater the pain felt the more the temperature was diminished in the hand left in the air.

As to the influence of partial heating, Dr Edwards relates the following experiment:—

"The hand, being immersed in water heated to the temperature of 34° R. (108°.5 F., 42°.5 Cs.), rose one degree of the same scale, and the temperature of other remote parts not immediately exposed to the influence of heat were found to have risen to a corresponding degree."

I have repeated this experiment of Dr Edwards, and I have found no evident elevation in the temperature of remote parts, as the mouth and the hand, not immersed in water.

I conclude, then, from the facts contained in this note, that in general the temperature of the body is not sensibly modified by the chilling or the heating of a small part of the frame.

XII.—ACTION OF COLD ON THE COAGULABILITY OF BLOOD, AND PERSISTENCE OF LIFE IN FROGS, AFTER LOSING HALF OF THE VENTRICLE OF THE HEART.

Dr Marchal (de Calvi) and other physiologists have recently asserted, that cold diminishes the quantity of fibrine, and consequently the coagulability of blood. I have observed a curious fact, which shows that blood may possess a very great coagulability, although exposed to the action of cold. I have found that after the removal of the half of a ventricle in frogs, the blood may coagulate at the surface of the wound, and the animal may continue to live. After the mutilation has been made, the lips of the wound are drawn upwards and inside, in con-

sequence of the muscular contraction. The blood flows very abundantly, but its coagulation quickly begins, and a layer of solidified blood is soon formed on the entire surface of the section, and by this process the wound is rapidly obliterated. The hemorrhage has been frequently stopped in a few minutes.

This experiment is successful only in cold seasons, probably because the batrachia are able to bear much better a loss of blood at a low than at a high temperature. The pulsations of such mutilated hearts continue with regularity and strength, but the impulse given by the remaining of the ventricle ought to be diminished. Nevertheless the circulation of blood is accomplished well enough to allow the animal to live many months.

I have shown to the Societé de Biologie at Paris two frogs on which the wound of the heart was cicatrised after fifteen days.[1]

XIII.—ON A SINGULAR CASE OF ANIMAL GRAFT.

Every one knows the experiments by which the cock's spurs or many other animal textures have been grafted on the body of an animal, and especially on a cock's comb. I have succeeded in grafting the tail of a young cat on a cock's comb. I performed this experiment in France in 1850.

After dividing the tail of a young cat, I made a longitudinal section on a cock's comb, and I united these two parts one to the other, by stitching the cut surface of the cat's tail to the cut surface of the cock's comb. The skin of the cat's tail had been turned a little over itself, so that its internal surface was in contiguity with the cut surface of the cock's comb. Eight days after, I punctured the skin of the tail at a distance from the cock's comb, and blood escaped, so that it was evident that circulation was already established. The tail had been cold during all the day of the operation, but it became warmer gradually from the second day. The union appeared much advanced on the third or fourth day. The tail was entirely fixed on the eighth day.

Unfortunately, on the eleventh day, the cock had a fight with another cock, and the cat's tail was torn out from the ground on which it had been fixed. I was thus deprived of the opportunity of knowing what transformations should have taken place in the tail.

By examining it I found that all its tissues were fresh, and that its blood-vessels contained blood.

XIV.—ON A CONVULSIVE AFFECTION PRODUCED BY CONSIDERABLE INJURIES OF THE SPINAL CORD.

I have discovered that a very violent convulsive affection is the constant result of a considerable wound of the spinal cord in certain animals. It is more especially in guinea-pigs, in whom a transversal section of a lateral half, or a complete section of the spinal cord has been made, that this is the case.[2] The section was made at the level of one of the last six dorsal vertebræ, or the two first lumbar.

When the convulsive fit begins, the muscles of the face and neck are the first which contract. The convulsions occur alternately in all the muscles of the eye, the face, the tongue, the jaws, and the neck; so that the cause of these movements ought to act on the parts of the encephalon, from whence the facial, the trigeminal, the hypoglossus, and the motor nerves of the eyes originate. The head of the animal is alternately bent on both sides of the body; and lastly the limbs are agitated in every direction. In the case of a section of the lateral half of the spinal cord in the lumbar region, or at the end of the dorsal region, three limbs only are strongly convulsed; the two anterior and one of the posterior—the one that is on the side of the body opposite to the side of the section of the

[1] See Gaz. Med. de Paris, 1850, t. v., p. 109.
[2] See Gaz. Med. de Paris, 1850, t. v., pp. 651, 895.

spinal marrow. The other posterior limb has only very slight movements. When the spinal cord has been entirely divided in the dorsal or lumbar regions, the two anterior limbs only are convulsed, with the muscles of the face and neck. These convulsions may come without any exterior excitation; but it is very easy generally to provoke them by frightening the animal, or by pinching, burning, or otherwise exciting it. The part of the body upon which a mechanical excitation acts more powerfully is the skin of the face or the neck. This convulsive affection has a great analogy with epilepsy; but it has also some distinctive features,—for instance, the animals during the convulsions do not appear to lose their consciousness, and they frequently cry when they are pinched.

The affection begins generally eight, ten, or twelve days after the spinal cord has been wounded. The convulsions then are not very strong, and it is usually four or five weeks after the operation that the fits are violent and easily produced. Three or four months after, the violence and frequency of these convulsions are diminished; yet in two or three instances they have increased in strength and frequency more than a year after the operation. The affection may last a long time: in one case it still existed two years after the operation.

Generally the fits last from five to fifteen minutes. The stronger they are the shorter is their duration. For an hour or two, and sometimes for one or two days after a violent fit, it is impossible to produce another one by any kind of excitation. When a fit takes place a very short time after a violent one, it is always weak or very short.

I have made the following experiment on some guinea-pigs having this convulsive affection. I put them in a small box, so that they had but little room to move, and I gave them food in great abundance. They then ate very much and were deprived of exercise, and, in consequence of that mode of living, they had exceedingly frequent convulsions. One of them had a fit nearly every quarter of an hour. These fits sometimes were strong, but they did not last long. The others had fits three or four times every day. I changed their mode of living, and put them all in a large room, where they had only a small quantity of food. The influence of this new regimen was nearly immediate; the same day the number of fits was diminished; and in the course of the third week afterwards they had only one or two fits; and before long some of them were cured.

I must relate also a very interesting fact, analogous to the proceeding. In several guinea-pigs and frogs which had the spinal cord transversely divided, I have observed that when the animals remained very quiet during many days, instead of the regular reflex movements, they had a violent tetanic convulsion in their posterior limbs, when the skin of these limbs was pinched.

Such a tetanic movement never occurred in these animals when I excited the regular reflex movements every day.

I have no room in this *résumé* to try to explain the facts related; I will do it in a special paper. I will only add, that all the readers of this note who know the views of Dr Marshall Hall about convulsive diseases, will remark how strongly the facts I have pointed out are confirmatory of these views.

XV.—ON THE RELATIONS EXISTING BETWEEN THE ORGANISATION OF NERVE TUBES AND THEIR VITAL PROPERTIES.

It is well known that when the nerve tubes are examined in a very fresh state, their contents, the *medulla* or *white substance* of *Schwann*, appear pellucid and homogeneous, and of a fluid consistence; but a kind of coagulation soon takes place in that medulla, making it easily distinguishable from the tube itself, solid, grumous, and much less transparent. Water has the power of producing very quickly that kind of coagulation of the white substance of Schwann, and the more quickly the warmer it is.

It was interesting to ascertain whether a nerve-fibre keeps or loses its vital

properties when its contents have been transformed by coagulation. I have performed two series of experiments in order to solve this question; one on the motor nerves, the other on the nerves of sensibility.

1st. After having amputated the two limbs of a frog, I laid bare a long portion of the sciatic nerve in both. Half an hour afterwards these two nerves were still able to act perfectly well. Then a microscopical examination of some fibres from one of these nerves demonstrated that the coagulation of the medulla had begun, but was not yet complete. Ten minutes later the nerves were still capable of acting, and the microscope showed that the coagulation was complete. A similar experiment repeated a great many times has always furnished the same result.

2d. After having divided the two sciatic nerves, not far from the knee-joint, in a living frog, I have dipped the central part of these nerves into water. After a few minutes, some fibres taken from one of them were beginning to coagulate; yet these nerves possessed their sensibility. Ten minutes after, there was a complete coagulation of the medulla, while the sensibility was very little, if at all, diminished.

I draw from these facts the conclusion, that nerve-fibres are capable of acting nearly as well when their contents are coagulated, as when they are still liquid.

This result appears to be important, inasmuch as it contributes to show that the white substance of Schwann has not a great part in nervous action. Three reasons concur in demonstrating that the essential part, the truly active one — that which possesses the vital properties — is not the white substance of Schwann.

1. We see that the vital properties of the nerve-fibres do not appear altered, although there is a very material change in this substance when it coagulates.

2. If such a change as that which takes place in this substance when it coagulates, existed in a part possessing the vital properties belonging to nerves, there should certainly be a movement or sensation accompanying it, and yet there is none. This material change is certainly more considerable than the change effected by a slight mechanical or galvanic excitation, which is capable of producing a movement or a sensation.

3. The microscope has already proved that the white substance does not exist in all the nerve-fibres, and that it is wanting, for instance, in the very fine fibres.

Therefore, if it be not the white substance of Schwann which is active in the nerves; and as it is not the *cylinder axis* which possesses the vital properties of the nerves, because it is wanting in many fibres, it results that it is the membranous layer, the paries of the nerve-tube, to which these vital properties belong, unless there is another substance, still unknown, and existing in the tubular canal of the nerve-fibre.

(*To be continued.*)

Part Fifth.

MEDICAL NEWS.

EDINBURGH MEDICO-CHIRURGICAL SOCIETY.

SESSION XXXII.

MEETING IV.—*Wednesday, 19th January* 1853.—Dr BENNETT in the Chair.

DILATED BRONCHI.

1. *Dr W. T. Gairdner* exhibited a specimen of pulmonary disease, consisting in the formation of very numerous excavations throughout the left lung, which was

very much diminished in bulk, being about six inches in extreme length, by three or four in breadth. Neither the excavated lung nor the opposite contained anything like tubercle, the fatal disease being acute bronchitis, which in the right lung had led to all its usual consequences, and likewise to an appearance of slight recent ulceration around the bronchi at some of their terminal branches. In the excavated lung the upper lobe was entirely reduced to a flaccid but tough fibrous tissue, without any trace of air-vesicles; the bronchi immediately beyond their cartilaginous primary branches opened out into sacculated dilatations, which communicated for the most part freely with one another, and with other excavations nearer the surface of the lung, but having altogether the same character. Over the greater number of these cavities there could be traced distinctly a limiting fibrous membrane, consisting of fibres quite similar in all their anatomical characters to those lining the bronchi, except that they were much less regularly disposed, and more widely separated from each other. A very careful microscopic examination, however, determined that this membrane, where it lined the cavities, had no true epithelium, nothing being attached to its surface but a little amorphous debris; while the inner membrane of the cartilaginous bronchi showed, as usual, abundant epithelium cells, although many of them were less perfectly formed than in the normal condition. The most careful examination could scarcely define the exact point at which the epithelium ceased, but it appeared to correspond to the commencement of the dilatations. One of the cavities had perforated the pulmonary pleura and abutted upon the adhesions, by which the two pleuræ were everywhere connected. At this point the coarsely fibrous investment of the cavity was deficient, and it was lined by a quantity of granular, slightly fetid, dark greenish debris. The lower lobe was much more indurated than the upper, and contained much fewer and smaller cavities, with a greater proportion of white, dense, resistent fibrous tissue. The cavities in the lower lobe had much the same relation to the bronchi as those in the upper; but they were relatively much smaller, involved in a much more indurated fibrous substance, and showed less distinctly the appearance of the circular fibres, so universally present in the upper lobe. They likewise contained at many points an olive-green pulpy substance, evidently the discoloured sloughs separated from some portion of the disorganised pulmonary tissue.

Dr G. remarked very shortly on this preparation, that it was the best example he had yet seen of the lesion called "dilatation of the bronchi" by Laennec, and "cirrhosis of the lung" by Corrigan. In this case the muscular fibres of the bronchi were split up, and separated from one another, and in this condition formed a lining to all the cavities in the upper lobe; but the remains of sloughing tissue in some of the cavities, the communication of one of them with the pleura, and the loss of the mucous membrane in all of those examined, showed that an actively disorganising process within the bronchi had paved the way for the dilatation of the muscular coat; and that this was not, as Corrigan and others supposed, a mere consequence of external traction. He had no doubt that dilated bronchi often resulted from what he had termed "bronchial abscess," and in general from local disorganisation of the mucous membrane in the course of bronchitis; and many of the conditions included under the name of bronchial dilatations had their origin in more considerable ulcerations when more or less completely healed.

2. *Dr W. T. Gairdner* read a communication which he had received from *Dr Gibson*, of Dundee, referring to a case of aneurism of the aorta, in which tracheotomy was performed under very urgent circumstances, with the effect of prolonging life, and removing the symptoms of laryngeal obstruction for twelve days. The patient died of dyspnœa and exhaustion, evidently produced by the increasing pressure of the aneurism on the left bronchus. It was found also, as in other cases of aneurism with laryngeal affection, that the recurrent nerve was involved in the fibrous tissue of the tumour. There were no physical signs of aneurism until shortly before death; but a correct opinion had been formed as to the nature of

the case, principally from the obstinacy with which the laryngeal symptoms resisted the treatment directed to what was at first supposed to be laryngitis. After reading the narrative of the case, Dr Gairdner remarked upon it, as an interesting example of a *physiological* diagnosis in aneurism, and as showing that even, under very unfavourable circumstances, a certain measure of relief was obtained from tracheotomy. The circumstance which interfered with more permanent relief was clearly the pressure of the aneurism on the bronchus.

Dr Andrew Wood had heard nothing to convince him of the propriety or expediency of having recourse to tracheotomy for the relief of laryngeal dyspnœa, in cases of aortic aneurism. He could not look upon tracheotomy as a slight operation; even its mere surgical performance was not always so; whereas in almost all cases it was likely to prove injurious, even when bronchitic complication was, in the first instance, absent, by exciting that disease, and by interfering with expectoration. He ventured again, as formerly, to suggest the cautious exhibition of chloroform as a substitute for tracheotomy. He had seen a very violent case of hysterical laryngismus relieved in this way.

Dr W. T. Gairdner said that he had no objection whatever to offer to a trial of chloroform in cases of laryngeal spasm from aneurism or any other cause. He had indeed himself tried it, though without any large amount of success, in one case of paroxysmal suffering from aneurism, in which tracheotomy was inadmissible, the larynx not being affected. But in the laryngeal cases, he confessed he would rarely trust to chloroform, except in cases where circumstances peculiarly contra-indicated an operation. The purpose of chloroform inhalation in these cases was different from that of tracheotomy. The latter measure removed permanently all chance of laryngeal spasm, however it might leave the patient open to other dangers. Chloroform inhalation, on the other hand, was only a remedy for the individual paroxysms, and this only, if active and safe assistance could be produced at a moment's notice. Supposing that chloroform so administered were always as effectual as in the case detailed by Dr Wood, he (Dr G.) had seen more than one case of laryngeal spasm from aneurism, in which the sudden accession of the paroxysms showed that life was not for a moment safe. The patient might be attacked in an unguarded moment, while eating, for instance (as in one case which occurred in the Edinburgh Infirmary), and might be cut off before chloroform could possibly be applied. He had seen a patient living in this state of perpetual anxiety, and in terror of instant death, for weeks, suffering under frightful paroxysms of dyspnœa, at intervals of every four hours, and ultimately perishing by sudden orthopnœa. Repeated instances had occurred in which death had taken place suddenly from this cause, notwithstanding the most prompt assistance. In such cases he thought that to trust long to chloroform, or any other palliative remedy, was merely to trifle with the symptoms. The class of cases referred to by Dr Wood, as giving such good results from chloroform, was of a very different character. Hysterical dyspnœa, of whatever kind, though often presenting a most formidable appearance, was known by experience to be rarely fatal, or even dangerous. Aneurismal dyspnœa was probably the immediate cause of death in a majority of the cases of aneurism in which it occurred; and it was also the cause of the most harassing and terrible suffering. It was impossible to stand by in such cases without feeling that the operation of tracheotomy was imperatively demanded, whether regarded as a measure of relief to suffering, or as a rescue from speedy death; it was only the presence of an organic disease, and the prospect of death at no distant date, that made the difference between such a case and that of laryngitis, for which it had been so often mistaken. He (Dr G.) thought that this difference should not to any great extent influence the treatment; and if the dyspnœa in aortic aneurism was ascertained to be laryngeal, and not dependent on an affection of the lungs or bronchi, it should be treated, so far as tracheotomy was concerned, on the same principles as laryngitis, —that is to say, by placing a tube in the trachea, so soon as the failure of other remedial measures, or the violence of the paroxysms, shall appear to render life unsafe without such a precaution. Of course the operation ought to be confined

to those cases in which the symptoms were chiefly or exclusively laryngeal; but on this subject a careful examination could generally furnish data sufficient to guide the practitioner.

Dr Mackenzie showed a drawing of the parts concerned in the disease, in a case of aneurism of the *arteria innominata* and arch of the aorta, in which he had performed tracheotomy, but without relieving the patient, who died a few hours after the operation. He had not seen the case which was under Dr Douglas's care, till he was called to open the trachea, on account of urgent dyspnœa. The drawing was chiefly interesting in a surgical point of view, as showing the difficulties which the operation under such circumstances might present. The aneurismal tumour bulged over the trachea, and could not be held aside, so as to render the trachea accessible with the knife. The incision was accordingly extended upwards, and the tube introduced through an opening made in the cricoid cartilage and crico-thyroid membrane.

3. *Dr James D. Gillespie* read a paper "On the Epidemic of Scarlatina which occurred in Donaldson's Hospital during the Autumn of 1852."—(See Original Communication in present Number of Journal.)

Dr Andrew Wood said, that there was no subject which could engage the attention of the profession of more importance than that of scarlet fever, considering the frequency of its epidemics, their wide-spread prevalence, and the high rate of mortality which still attended the disease, notwithstanding all that had been written on the subject by a succession of able and judicious observers. Were the mortality confined to the lower classes, we might be inclined to explain it by a reference to the various depressing influences which poverty notoriously exerts on the course of diseases, and to carelessness in following out the proper treatment; but even the most recent experience taught us, that amongst the higher classes, "with all appliances and means to boot," scarlet fever was still one of the most fatal diseases with which the practitioner had to deal. Every contribution, therefore, to our knowledge of the disease must be considered as of value, and they were indebted to Dr Gillespie for his elaborate and carefully digested report of the epidemic which had lately occurred in Donaldson's Hospital. It was remarkable that amongst so large a number of cases—viz., seventy—there had occurred hardly one of a grave character, and this at a time, too, when the general type of the disease in this city had been severe. It was difficult, if not impossible, to account for the difference of severity which characterised, not only different epidemics of scarlatina, but also different groups and individuals attacked with it in the same locality, and in the same epidemic. On referring to the works of Sydenham, we find that during his long career he must have met with nothing but mild epidemics, as he dismisses the disease with a very cursory notice—all that he considered necessary in regard to what he terms a mere "nomen morbi." Heberden's experience coincides more nearly with what is observed in our own day, as may be inferred from his very neat and true statement, "that there are many cases of scarlatina which require no treatment, and many cases which admit of no treatment." These considerations it was of importance to keep in view in making deductions as to the efficacy of particular modes of treatment. A long succession of fortunate cases may perhaps lead the practitioner to imagine that he has learnt to master the disease, when all at once a case, or even a series of cases, will occur, which mock his efforts, and serve to dissipate the delusion of any infallible system. Dr Gillespie had, in his paper, drawn conclusions as to treatment from the cases he had seen; but he (Dr W.) would warn him against the expectation that he would find these available in those severe forms of the disease which caused such fearful anxiety to the practitioner, and which, even when combated by all that skill, attention, and vigour, and the resources of a large past experience could afford, were too often struggled with in vain. Perhaps he (Dr W.) might be pardoned for shortly stating what were the chief circumstances under which scarlatina had the greatest tendency to prove fatal. 1. Cases in

which the patient became suddenly collapsed, cold, pale; quick indistinct pulse; no power of re-action; sometimes vomiting, sometimes diarrhœa, proving fatal, if unchecked, in one, two, or three days. There may be, and often is, no affection of the throat. This state also occasionally supervenes in the course of a case where the disease has seemed to go well, with good eruption and good powers of re-action. Sometimes we cannot account for this sudden change. I have myself seen it induced by the use of mercurial purgatives. 2. Cases where the collapse is more moderate, but where the re-action is imperfect; the heat of surface either below the natural temperature, or at least not much above it; the eruption only partial, of a dusky or sometimes port-wine colour; the throat ulcerated and foul; foul and irritating secretion from the nose and eyes; often violent delirium, or stupor; death ensuing in from five or six days to a fortnight. 3. Cases where there is considerable re-action; eruption general and bright, but having a somewhat purplish hue; the throat early affected; the cervical glands swollen, and diffuse cellular inflammation of the neck, the larynx sometimes becoming affected. These are often distressing cases, attended with much suffering to the patient, death taking place generally within ten days. 4. Cases in which the primary symptoms may have abated, and in which suppuration in the cervical glands becomes extensive, and the patient dies from exhaustion and hectic fever. 5. Albuminuria and its consequences, whether with head symptoms, coma, convulsions, etc.; inflammation within the chest, of lungs, pleuro-pericardium, etc., generally coming on between the fourteenth and twenty-third day. Dr W. said that these divisions seemed to him to embrace the chief sources of fatality in scarlatina, though there might be others which, for brevity's sake, he had necessarily omitted to mention. Now, it was a melancholy fact, that many of these cases, especially primary cases, ran on to a fatal issue with greater or less speed, in spite of every method of treatment that had as yet been suggested; and he would be a bold man, and not a very candid one, who should endeavour to claim infallibility for any "methodus medendi" which he might propose. The great principle which ought to guide us in treatment seemed to be this, the using of means calculated to eliminate from the system a specific poison. Nature herself pointed out the skin as the proper emunctory, and it was universally recognised as a fact, that where the eruption appeared early, and was general over the body, and where the powers of re-action were active, the greater the tendency to a favourable issue. Theory, then, led to the conclusion, that the rational treatment of scarlatina was without delay to use every means for shortening the period of collapse, and for determining to the skin and exciting powerful diaphoresis. Of late years this system had been pretty generally adopted. The object to be attained seemed to be settled; regarding the means of best attaining it, there were differences of opinion. On a former occasion, Dr W. said he had ventured to suggest what he had found useful in his own practice—viz., the excitement of powerful sweating, by means of a modified vapour bath, which could be used while the patient lay in bed, could be repeated again and again, and was available under almost any circumstances. As the method of its application seemed to have been misunderstood, probably from not having been clearly explained on a former occasion, he would just shortly explain now that this was the method of using it—viz., the patient was in bed, having a blanket below and above him, to prevent the possibility of damp sheets (which had this evening been suggested as an objection to the plan); long worsted stockings, or flannel bags, about a yard long, and of capacity enough to hold several common bottles, or, what answered better, stone foot-bottles, are wrung out of hot water; they are then filled with bottles full of boiling water, and then are introduced within the blankets and laid alongside the patient, but of course not in contact with the skin; three or four stockings are generally sufficient, but more may be had recourse to, if necessary. In a period varying from ten minutes to half an hour powerful sweating will be occasioned, and this may be maintained by drinking copiously of water, or by diaphoretics, such as the spirit of Mindererus combined with sweet nitre. The bottles may be removed within

an hour. The relief is often very speedy. In cases where the power of re-action is feeble, it is necessary to give stimulants, either carbon. ammon., in doses of four or five grains every three hours, or, where vomiting occurs, brandy may be had recourse to. Sometimes it may be necessary to use the vapour bath twice or even thrice a-day. One effect of the use of the vapour bath which Dr W. had observed was this, that where it was used freely the tendency to subsequent albuminuria was greatly diminished. With regard to the local treatment of the throat, Dr W. had discarded in his practice leeching, or the application of any stimulating gargles. He believed the application of poultices externally, and frequent sponging of the mouth and fauces with water acidulated with dilute nitric acid, to be all that was necessary in most cases. With regard to the treatment of the albuminuria which follows scarlet fever, that was now well understood by practitioners, and the mortality from that source had been greatly diminished. Many lives must have been lost in former days, when its pathology was misunderstood, and when it was treated, as a passive dropsy, with stimulants; but now, in the use of depletion, general or local (if circumstances required it), of the warm or vapour bath, of antimonials, and of brisk purgatives, we possessed a system of treatment accordant with sound theoretical principles, and in a large number of cases, even of great severity, eminently successful. With regard to the question of the occurrence of albuminuria in scarlatina, the experience of Dr Gillespie in Donaldson's Hospital tended to prove, in addition to other observations, that albuminuria was by no means an essential attendant, or consequent, on scarlatina. Probably there was a greater tendency to it in some epidemics than in others, and certainly it was most apt to occur in cases where the patient had either been exposed to cold, or had been permitted prematurely to have animal food. By proper treatment in the primary fever, and by subsequent care, it might probably in most cases be altogether avoided. At all events Dr W. had no doubt on this matter, that the presence of albumen in the urine in scarlatina was always to be regarded as an untoward occurrence. He thought it the more necessary to make this remark, because he had lately seen it remarked in Ranking's "Abstract of the Medical Sciences" (vol. xvi. p. 113), as a deduction from Dr Warburton Begbie's recent paper on the subject, that "albuminuria is often a temporary, and not unfrequently a salutary, symptom, instead of being, as is supposed by many, a constant indication of serious organic disease." This seemed to him (Dr W.) a very dangerous doctrine, and not warranted by observation of facts. The only other matter to which he felt it necessary to advert was the use of belladonna as a prophylactic. Recent observations, especially those made by Mr B. Bell, of this city, in Watson's Hospital, seemed to prove that belladonna neither prevented scarlatina nor mitigated its symptoms. This conclusion completely tallied with what he (Dr W.) had observed in Heriot's Hospital, where the experiment had been made in somewhat a different mode from that of Mr Bell, as, instead of giving belladonna to all the inmates, he gave it only to one-half. The result was not favourable. In fact the experiment was only continued a short time, in consequence of the occurrence of a fatal case (the only fatal one during an epidemic in which about forty cases of scarlatina were observed), and in which case the boy had had belladonna, in doses of one-eighth grain twice a day, for three weeks after exposure to infection.

Dr Newbigging said that he had had the honour, during the Session of 1849, to lay before the Society a few observations on scarlatina, as it appeared during the epidemic of that year, more especially at John Watson's Institution, and referred to his experience of the apparent efficacy of belladonna as a prophylactic in this disease. Having ascertained that this substance had afterwards entirely failed in the hands of his friends, Dr Andrew Wood and Mr B. Bell, he did not consider that it was worthy of further credit, and could not but feel regret at these results. A case of scarlatina occurring on the 4th December, in the same institution from which Dr Newbigging had drawn his former experience, he could not resist trying again the alleged efficacy of this drug. He accordingly gave it to those children—fifty-seven in number—who had not had scarlet fever, com-

mencing it on the occurrence of the case alluded to. No farther instance of the disease has taken place. The child was removed immediately to the sick-room, and every precaution was taken to prevent contact; but the room reserved for the sick opens from the same corridor as the other dormitories, and therefore Dr Newbigging believed that it was somewhat difficult to make a very perfect isolation. Dr Newbigging stated that he was by no means anxious to adopt any one-sided or exclusive view on this subject; but he believed that a conclusive opinion would be best obtained by a series of well-recorded observations, on this as well as other points, in the history of a malady which occasionally proves so formidable. And he remarked, that great indeed would be the boon, should advancing science provide us with some such substitute, as vaccination was for small-pox. Dr Newbigging took occasion to disapprove of the minute heads into which Dr Gillespie had divided scarlatina. He did not consider that the instances given by Dr Gillespie, of scarlatina latens, were to be recognised as examples of that disease at all. Dr N. observed that doubtless scarlet fever varied, as regards the manifestation of the disease, according to the peculiarity of the epidemic; and, as an illustration of this, he stated that, constant as had been the presence of albumen in lesser or greater quantity during the epidemic of 1849, he had failed to discover it to any thing like the same amount during this season. Dr N. drew attention to the importance of distinguishing between the presence of epithelium in the urine and the more serious sign—albumen, which was frequently the forerunner of urgent symptoms, if not attended to. Dr N. expressed a very favourable opinion of the value of chlorate of potass in this disease, employing it as a drink, and that it was not incompatible with the administration, at the same time, of other remedies. He also alluded, in strong terms of commendation, to the method of treatment by the application of heat and moisture to the surface, believing that the plan first suggested by Professor Thomson, and so ably advocated by Dr Andrew Wood, was the best and most convenient method of producing the desired effect. Dr Newbigging quoted one or two cases illustrative of its efficacy.

Dr Warburton Begbie attributed great interest to Dr Gillespie's observations on the urine. The absence of albumen differed very materially from his own observations, made in 1848–49. He thought it not improbable that, during certain epidemics, the albuminuria might be more frequently present than during others; and also, that its occurrence might be affected by treatment. He had been led to connect the albuminuria with the desquamation of the cuticle, from observing the intimacy of their occurrence and continuance in the cases he had himself seen. In such cases there was certainly no evidence of congestion in the renal capillaries; but there always was more or less epithelium from the kidneys in the urine. It did not appear, from Dr Gillespie's narrative, in what proportion of cases desquamation of the cuticle had occurred, nor how frequently he had examined the urine.

Dr Moir said a few words corroborative of Dr Wood's remarks on the use of the warm or vapour bath. In the convulsions which occur as sequelæ of scarlatina, Dr M. had found copious purging successful. From experience in two instances, Dr M. expressed an unfavourable opinion of belladonna.

EDINBURGH PHYSIOLOGICAL SOCIETY.

SESSION III. (*Winter* 1852–53.)—MEETING IV.—*8th January* 1853.

Dr BENNETT, President, in the Chair.

REPORT ON THE LOCAL EFFECTS OF PUS ON THE BLOOD.

1. *Dr Bennett* brought forward the report of the committee appointed to repeat Dr Henry Lee's experiments on an ass, with a view of determining the local effects of pus on the blood:—

Experiment 1.—The saphena vein of an ass was exposed, and a tube intro-

duced confined by a ligature. Fresh and healthy pus was then slowly injected *upwards* towards the heart, from a syringe holding an ounce. A slight obstruction was now perceived, and the vein above the ligature could be seen to be somewhat swollen. This swelling, on being felt, was very soft; and on pressing the vein from below upwards, the mixed blood and pus was readily pushed before the finger, when all obstruction to the passage of pus from the syringe was removed. The syringe was again filled, and another ounce of pus injected, without occasioning any further local effects. The animal was then allowed to get up, and exhibited no change in its normal condition whatever.

Experiment 2.—The same ass was the subject of this experiment a fortnight later, having been perfectly well in the interval. Six inches of the jugular vein in the neck were carefully dissected and exposed; and a minute aperture was then made in the upper end of the exposed vein, and the bent tube of the syringe introduced without a ligature. The coats of the vein were so transparent that the flowing blood could be seen through them. An ounce of fresh and perfectly healthy pus was then slowly injected *downwards* towards the heart, and, owing to the transparency of the vein, the yellow opaque fluid was seen to join the blood, to continue a few moments running side by side with the crimson current, until at length the vein became full of pus. On removing the syringe to obtain a fresh supply, the blood from above could be seen to join the pus, to continue side by side with that fluid, presenting a streaked red and white appearance, without any coagulation, until all the pus was carried forwards and downwards towards the heart, and the vein was again full of blood. Another syringe-full of pus was then injected, which could once more be seen first to flow with the blood, then, as its quantity increased, to take the place of the blood, and then, on the syringe being exhausted, to receive blood from above; the two mixing together, and continuing their course without coagulating, until once more the vein contained nothing but blood. The wound was now closed, and the animal allowed to rise, which he did without apparent suffering. He presented no unusual symptoms whatever during the next four days, when he was killed, and the parts carefully dissected. The vein was pervious, presented no thickening, nor cording or abscesses, and the external wound was nearly healed.

This experiment appeared to be so decisive, and so clearly opposed to the idea that the contact or mixture of pus and blood necessarily induced coagulation in a living animal, that it was thought unnecessary to repeat it. With regard to the slight coagulability apparently occasioned in the first experiment, it was attributed partly to injecting contrary to gravity, whereby the mixed pus and blood were allowed to fall backwards and remain stationary, while the ligature prevented any flow of blood from being continued. No such phenomenon was observed in the second experiment, where no ligature was employed, and where the effect of gravity was avoided by injecting downwards. In a communication, however, received from Dr Henry Lee, he had informed Dr Bennett that no ligature was employed by him.

The second experiment performed by the committee was in its nature the same as the seventh and eighth experiments of Dr Henry Lee, and yet none of the appearances observed by that gentleman resulted. There was no fulness or cording of the vein, no acceleration of respiration or constitutional symptoms; and after death no coagulation of the blood, no obliteration of the vein, nor local inflammation. What are the circumstances which occasioned this difference, the committee are not prepared to say; but the positive fact of having introduced the pus on two separate occasions, as recorded in Experiment 2, of having seen the pus mix with the blood and the blood with the pus, through the transparent vein, without producing coagulation, is in the opinion of the committee sufficient to negative the general proposition, that whenever pus is mingled with blood in a living animal, coagulation of the latter fluid is the invariable result.

Dr Gairdner remarked, that these experiments did not necessarily disprove the conclusions arrived at by Dr Henry Lee, of London, from his observations; but he (Dr G.) considered, that further investigation into the mode of experimenting adopted by different individuals might tend to reconcile the apparent discrepancies in their statements as to the effect of pus on the blood.

Dr Gregory agreed with Dr Gairdner in thinking, that the apparently different results in experiments of this kind might be explained, partly at least, by different modes of experimenting.

COLLOID TUMOUR IN THE HEART.

2. *Dr Bennett* exhibited the heart of a woman, who had died in the clinical ward of the Royal Infirmary, labouring under disease of that organ, as indicated by a blowing murmur with the first sound, loudest at the apex. The incompetence of the mitral valve was occasioned by a tumour, the size of a duck's egg, firmly adherent to the auricle, a little above and to the outside of the auriculo-ventricular opening. It was of gelatinous consistence and appearance, of a semi-transparent light amber colour, of slightly fibrous structure, closely resembling simple colloid matter. On microscopic examination, it was ascertained to consist of bundles of minute transparent filaments, crossing and anastomosing with each other, and forming round or oval loculi. These were filled with a transparent viscous fluid, in which numerous nucleated cells, of various shapes—round, oval, or fusiform—were imbedded, varying in size from the $\frac{1}{100}$th to the $\frac{1}{40}$th of a millimetre in diameter. On the addition of acetic acid, the cell-walls were partially dissolved, whilst the nucleus was unaffected. He regarded it as a transformation of the coagulum of the blood into a colloid mass.

Dr Cobbold stated, that the tumour appeared to him to consist of little else than fibro-plastic cells (like those figured by Lebert), and that on the addition of acetic acid, the cells, fibres, nuclei, and granular contents, became more distinct, while a single nucleolus appeared in the centre of each nucleus.

3. *Dr Bennett* read a communication from Dr Drummond, now at Bonn, regarding the contractile properties of the iris as explained by the action of the optic and motor nerves of the orbit, in which the recent experiments of Professor Budge of that university were detailed.

After some remarks by *Dr Bennett*, and a short discussion, in which *Mr Struthers* joined, a committee, consisting of Mr Struthers and Dr Sanders, was appointed to examine into the subject as connected with Dr Drummond's statements, and to report.

4. *Mr Dalzell* made a communication on a peculiar structure observed by him in calcareous spicula from the integument of a *doris*, where he had seen these bodies surrounded by distinct membranes, probably the secerning tissues which produced them. In some instances, by regulating the pressure and the addition of dilute muriatic acid, the author had succeeded in rupturing their envelopes, so that a continuous stream of carbonic acid was evolved, apparently from one aperture; after the escape of which, traces of the organic tissue were still discernible. He mentioned that Mr Bowerbank had described certain siliceous varieties as containing in their interior a membranous core, which heat converted into a film of carbon. The present observation, however, indicated that the animal membrane was sometimes external. He also remarked, that the blue colour of the marginal tubercles of actinia mesembryanthemum, which Cocks and some others supposed to be eyes, and in which tubercles the spermatozoon-like bodies were closely packed, was due to a dense layer of pigment, placed superficially, as already described by Johnston, which the alkalies first reddened and afterwards bleached. The muscular element of actinia, Mr Dalzell stated, was placed beneath a mucoid epithelium, and was arranged in sphincters and perpendicular bands, presenting only a very indistinct striation

upon the addition of acetic acid. His remarks were in connection with a series of observations into the dermal structures of animals.

5. *Dr Cobbold* announced that he had discovered the existence of spiral fibres in the tissues of the *actinia gemmacea;* he had shown this structure to Professors Bennett and Goodsir, and was of opinion that (if not structurally) it bore functionally the character of muscular fibre. Dr Cobbold stated, that he should demonstrate this from fresh specimens at next meeting.

6. *Dr Cobbold* exhibited a coloured wax-cast of a multilocular ovarian tumour, which had inflamed, suppurated, and discharged part of its contents into the peritoneal cavity, causing death. Microscopically, the tumour presented a mass of broken-down cells and fibres, but amongst the debris were observed numerous forms of cylindrical epithelium, precisely like that found covering the intestinal villi. The occurrence of these cylindrical cells under the same circumstances had been mentioned by Dr Bennett at a former meeting of the Society. The history and post-mortem appearances in this case are reported by Professor Simpson in the "Monthly Journal of Medical Science" for December 1852, p. 524.

Dr Sanders, who had examined the tumour, remarked that, in the present instance, these cells were found lining the interior of the loculi.

7. *Dr Cobbold* exhibited a series of microscopic preparations of crystalline salts, which had been prepared by Professor Gregory.

No. 1. Hydro-chlorate of Roseo-Cobaltiack. Formula,—Co_2, Cl_3, 5 NH_3, HO. This (the red) salt presented numerous modifications of the octohedron.

No. 2. Hydro-chlorate of Luteo-Cobaltiack. Formula,—Co_2, Cl_3, 6 NH_3, HO. This (the yellow) salt exhibited varieties of the right rhombic prism.

No. 3. Copper-coloured Platino-Cyanide of Potassium. Formula, — 2 K Cy + P_2 Cy_3 + 5 HO. The peculiarity of this salt consists in its presenting a metallic lustre when viewed at an angle of about 45°. It crystallises in a feathery form.

No. 4. Thionurate of Ammonia. Formula,—(C_8, N_3, H_5, O_8, 2 SO_2) + 2 NH_4 O + 2 HO. These crystals assumed a great variety of shapes, often, as in the present specimens, resembling well known organic forms; their typical character seems to be that of a right rectangular prism.

Dr Gregory stated that the first *two* of the above salts had only recently been discovered by Claudet, Genth, and Franz.

8. *Mr Mercer Adam* exhibited under the microscope some blackish grumous fluid matter, which was vomited shortly before death by a woman, who died of excessive anæmia, induced by a large fibrous tumour of the uterus. This fluid was slightly acid in re-action; and in it were observed several beautiful specimens of the *sarcina ventriculi* of Goodsir, surrounded by numerous granules and loose epithelial scales.

9. *Mr M. Adam* also stated to the Society the particulars of an interesting case of a child, otherwise in perfect health, whose body was covered with numerous small nodulated swellings, of various sizes. When one of these was pricked with a lancet, a fluid exuded, of a pearly white colour, and of the consistence of thick cream, which soon solidified. When examined by the microscope, it was found to contain fat cells, interspersed here and there with crystals of margarine. Mr A. stated, as his opinion, that these appearances were probably caused by obstruction of the sebaceous follicles of the skin. Mr Adam also mentioned, that Dr Drummond had informed him that he had once seen similar appearances in a child, in whose body—on post-mortem examination—encephaloma was discovered in several tissues. Probably in this case the microscope would have detected cancer-cells in the fluid of the swellings; but in the present in-

stance it afforded a valuable means of diagnosis, and showed the affection to be purely non-malignant.

10. *Mr Barlow* exhibited a dissection of a horse's injected liver, showing the vascular connection existing between the vena portæ and posterior cava, as first described by M. Bernard.

MEETING V.—22*d January* 1853.—DR BENNETT, President, in the Chair.

ON THE TOUCH-BODIES OF WAGNER.

1. *Mr Dalzell* read a communication on the *tastkörperchen* of Wagner. Physiologists were aware that these bodies had excited considerable attention, and given rise to a good deal of speculation. Kolliker, Nuhn, and Gerlach had subsequently described them; and he himself had recently made them the subject of observation. Touch-bodies had been looked for in the tongues of several of the Ruminantia, Rodentia, and Carnivora, but without success. Probably they would be found in the fingers of Quadrumana; this supposition, however, he had not had an opportunity to confirm. The principal points to be attended to in searching for them were, to select a lean, fresh, distal phalanx, moderately dry, and to make thin sections perpendicular to the dermis, so as not to cut the papillæ obliquely. More or less throughout the whole tactile surface of the integument, but especially half way between the concentric rugæ and free extremity of the nail, touch-bodies were to be found, seldom in elongated slender papillæ, oftener in those irregularly conical, but perhaps most frequently in the compound papillæ, in which they sometimes occurred in pairs. Viewed after the addition of acetic acid, they appeared as distinct, more or less oval structures, occupying either the upper part or side of the papillæ, and covered externally with very distinct, elongated, transverse nuclei. Sometimes they presented a regular outline; at others, were lobed transversely; and occasionally that appearance was complicated by a vertical furrow, as described by Nuhn.

Ether and turpentine did not affect them; but Gerlach's process of preparing the section with alcali, though perhaps necessary in injected subjects, seemed to him to alter considerably the appearance of the parts; for the abstraction of the fatty matter was followed, on the addition of acetic acid, by a certain amount of coagulation and contraction of the tissues.

On transverse section, these bodies presented two appearances, which might, however, be the same in different stages of development. In one, as described by Nuhn, there was a distinct hyaline centre, surrounded by a nucleated cortex, composed of fibres and spindle-shaped cells. In such sections the author had never detected the cut ends of nerves. The other form, which was equally common, only differed from that already described by the absence of a distinct central portion, and appeared to be entirely made up of elongated spindle-shaped cells, with irregular nuclei, smaller striæ, and occasional granules. In some of such sections he had very distinctly seen two or even four regularly-edged pale openings, *which might be the ends of tubes*. He had never seen, in connection with these bodies, the division of the double contour nerve into pale fibrils, as described by Wagner—an appearance which Gerlach had only seen twice, and neither Kolliker nor Nuhn had detected at all.

Mr Dalzell stated that some possible causes of fallacy were present in viewing the so-called touch-bodies in transverse section. Nuhn had described them optically as funnel-like hollows. These hollows, he had no hesitation in saying, were not the structures of Wagner, but either the depressions between the papillæ, from which the deep columnar cells of the mucous layer had been detached, or the ends of the tortuous portions of sweat ducts as they entered the corium. This statement was corroborated by the fact, that these funnels occurred in all the corresponding parts of the integument, whilst the bodies of Wagner were found only in some.

With regard to their relation to the nerves, the author stated that a nervous bundle was generally seen at no great distance from the papillæ in which they were contained. Owing to the difficulty of making the section in the line of the ascending nerve, it was only occasionally that one or more nerves could be seen to pass continuously to the base or side of the corpuscle, and still less frequently could these be traced returning from it. From a great number of observations, however, he had arrived at the following results:—That sometimes from two to four perfect nerve-tubes reach the base or side of the touch-body, and either loop upon or over it, or abruptly disappear. That still oftener they pass external to it, and seem to terminate on the surface of the papillæ, as described by Kolliker. But, that it must not be forgot, that the passage of the knife across the section or the pressure afterwards used, might influence either of these appearances,—*first*, By bringing the nerve in contact with the body; and, *second*, By removing it from it.

He begged to call the attention of the Society to the following facts:—

First, That Wagner's division of the papillæ into purely nervous and vascular had no anatomical foundation, for even those in which his touch-bodies were found very frequently contained one or more vessels.

Second, That we have good reason to conclude that every papilla in which a touch-body is lodged, receives from one to four nerve-tubes, with which it sometimes is, and sometimes is not, in immediate contact.

Third, That the parts of the integument in which these structures are found are eminently tactile; but that their number varies considerably in different individuals, and that they sometimes are either not present at all, or escape detection, even when every portion of the integument is carefully searched.

Fourth, That they are not fatty or nervous convolutions, as supposed by Gerlach, but are composed of condensed imperfectly-developed cellular tissue, with or without a hyaline centre; and that, so far as his observations went, the existence of nerves within them had not been detected, but that these were either looped externally, or cut off abruptly at various points.

From these facts he concluded, that whatever view was adopted of the histological relations of these bodies to the nervous element; and even admitting the somewhat doubtful observation of Wagner, as to the filamentous division of primitive nerves within their substance, Kolliker's theory seemed to convey the most probable explanation of their function,—viz., that they perhaps act as *cores* against which the nerves are pressed, a process by which, just as a foreign body produces irritation, so an organic subcutaneous induration might exalt the peripheral sensibility of the part. He did not feel prepared to admit that they were special apparatuses of tactile, as opposed to common sensibility, at least in the degree to which Wagner and others had elevated them. Observations on their development appeared likely to throw light on their real character.

SPIRAL STRUCTURE OF MUSCLE.

2. *Dr Cobbold* exhibited under the microscope spiral muscular fibres, from the tentacula of *actinia gemmacea*. Dr C. stated, that at first sight they appeared to consist only of single coils or threads; but on a more careful examination he was satisfied that the spirals in question were severally made up of twin fibres, such as Dr Martin Barry had pointed out as obtaining in the case of ordinary muscular fibre. The different appearances presented by these fibres was, he believed, fully illustrated by the series of wire models (on the table), which Dr Barry had politely forwarded. Dr Cobbold also showed several preparations of muscular fibre from the human fœtus, and from a lobster (*astacus norvegicus*). The appearances presented by these he also believed to be confirmatory of Dr Barry's observations. The only structural difference traceable between the muscular fibres of the human subject and those of actinia just mentioned being,

that in the former the double coil is surrounded by a delicate transparent membrane (hyaline), whereas in actinia no such covering exists.[1]

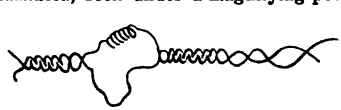
3. *Dr Cobbold* exhibited the spermatozoon-like bodies so abundant in the stomachial membrane and other parts of actinia. These bodies, described by Wagner (in Weigman's *Archiv*, f. 1835, Bd. ii., S. 215) as true spermatozoa, are now generally recognised as stinging organs. The Society's attention is called to them, as they have been frequently confounded with the mineral bodies alluded to at a former meeting of the Society; and further, because in their unexpanded condition they have been mistaken for the "spiral muscular fibres" just demonstrated.

4. *Dr Gairdner*, on behalf of *Mr John Kirk*, made a communication, accompanied by drawings and a demonstration, to illustrate the apparent spiral structure of muscular fibre in the fœtal calf. Mr Kirk had in the course of last autumn been engaged in the examination of fœtal muscular fibre, and had become convinced that in the mammalian fœtus (the calf being the one examined by him) a double spiral might be distinctly demonstrated within the sarcolemma, and occupying the situation of the so-called transverse striæ of adult muscle. He had viewed this spiral structure under every conceivable aspect, had employed numerous re-agents, and had, by repeated examinations, secured himself against accidental sources of fallacy. He had, moreover, observed the mode of formation of the ultimate fibrillæ of muscle, from the spiral structure above mentioned. By the addition of tincture of iodine, and afterwards of acetic acid, the spiral fibre was distinctly resolved into a succession of minute beads or particles arranged spirally in linear series. When the spiral was completely closed up, the beads or particles of successive coils formed also linear series in the axis of the fibre, just as the ideal spiral in the stem of a plant sometimes produced an arrangement of its buds so as to form a series, apparently independent of the original type. In the adult, a primitive fasciculus was capable of splitting under different conditions, either into longitudinal fibrillæ, or into transverse discs; the latter were separated rings of the closed spiral, the structure of which could be clearly demonstrated only in the fœtal muscle, in which it could be drawn out to a considerable extent without fracture. Mr Kirk admitted the impossibility of showing these facts satisfactorily in one or two demonstrations, but was anxious to have an opportunity of bringing them before a committee of the Society.

After considerable discussion, arising out of the appearances presented by these demonstrations,

Dr Bennett observed, that attentive examination of the various demonstrations now before the Society had convinced him that the structure discovered by Dr Cobbold in the integument of actiniæ was really that of a spiral filament;

[1] The accompanying figure is a representation of one of the "muscular fibres" from the tentaculum of *A. gemmacea*, seen under a magnifying power of 300 diameters. In



this instance the twin coil was partially unwound, and in such a manner as to explain several appearances, concerning which different views have been taken. Mr Busk (to whom I forwarded a preparation of these fibres) informs me, that he has been long familiar with these "spirals" as the "*prehensile filaments*," or threads, which are abundant in the medusoid or hydroid class of animals. Notwithstanding the function thus ascribed to this tissue, Mr Busk believes that they have nothing to do with the muscles of actinia. Mr Busk does not recognise the "*double*" coil, to which I attach much importance.—T. S. C.

but he saw nothing which exhibited the existence of spirals in mammalian muscular fasciculi in the numerous beautiful demonstrations of that tissue, either by Dr Cobbold or Mr Kirk. The various drawings of double spirals shown by Dr Cobbold, although in his opinion confirmatory of Dr Martin Barry's views, were in no way confirmed or supported by the structures now exhibited to the Society. But as the subject was one which could not be determined by discussion, but only by investigation and careful observation, he proposed that a committee be named for that purpose.

This was agreed to, and Dr Dobie, Dr Sanders, and Mr Barlow, were appointed a committee to examine Dr Cobbold and Mr Kirk's preparations, and report on the subject at a future meeting of the Society.

5. *Dr Gairdner*, in the absence of *Dr Dobie*, exhibited a specimen of melanotic cancer, occurring in a toe, which had been amputated by Mr Syme. The tumour had been growing for upwards of two years; it commenced on the outer side of the great toe, in the form of a small corn, subsequently it became painful, and occasionally bled pretty profusely. At the time of the patient's admission to the hospital it nearly surrounded the great toe, and extended over its metatarsal bone, which was removed along with the tumour. The tumour was only partially melanotic; it contained numerous large cells, which, in the melanotic parts, contained the usual granular pigment.

MEETING VI.—*5th February* 1853.—Dr BENNETT, President, in the Chair.

1. *Dr Bennett* exhibited a calvarium, of curious irregular shape. There was a depression at the superior angle of the frontal bone; and the bulgings of the bone laterally destroyed the symmetry of its right and left halves. The specimen was from the body of an aged female.

THE STRUCTURE OF THE HAIR.

2. *Mr Dalzell* communicated to the Society the result of his observations on the structure of the parts involved in the production of the hair. They were for the most part confirmatory of the statements of Kolliker. The appearance of the longitudinal fibrous, transverse nucleated, and structureless coats of the follicle, were exactly what Kolliker had described; but Mr Dalzell had determined the continuation of the basement membrane *over the papilla*, where Kolliker had not been able to trace it. There could be no doubt that a distinctly defined papilla was present in every follicle, and not a mere "layer of papillary pulp," as suggested by Tod and Bowman. The form of this structure was that of a sphere, slightly elongated superiorly, and attached by a thin pedicle to the base of the follicle; and the author believed that the upright elongated cells, forming the external layer of the outer root sheath, were repeated more or less distinctly upon its surface. Kolliker had mentioned that no one, without the assistance of injection, had ever seen the vessels of the papilla; and many observers, as Hassal and Günther, had, for the same reason, even denied their existence. He had, however, frequently observed them in the tactile follicles of the young mouse, where they appeared as red lines in the pedicle, and might be followed even into the central pulp, provided the animal was newly killed. The author stated that Henle's description of the fenestrated membrane was generally accurate, but that the apertures which he had described as oval were in reality narrow splits. He could not account for Kolliker's describing it as a polygonal cell membrane, with occasional openings; for it was, except at its inferior limits, composed entirely of laterally anastomosing fibres of a clear hyaline texture, which, when undisturbed, appeared as a viscous layer, but was easily resolved by pressure into a net-work of flattened bands. A curious variety of Huxley's cells was present in some of the lower Rodentia, in which from five to nine nuclei were found, and the cell-walls exhibited puckerings at the points where these

bodies were attached. Unless attention was paid to the relation of the parts, it was possible to mistake the open striæ of the hair cuticle (found at the neck of the bulb) for the netted fibres of Henle's sheath. Mr Dalzell illustrated this statement by drawings, and described the importance of the imbricated layer, which inferiorly, by its transversely elongated cells, formed a band of restraint to the fibres of the shaft, and superiorly protected the hair from mechanical injury, by its flattened and impervious horny plates. Respecting the innervation of ordinary hairs, the author believed that the pain experienced on extracting them depended on their connection with the surrounding parts; and that the traction of the follicle, rather than the mere extraction of the hair, was the cause of pain. Occasionally, however, and especially about the lips and cheeks, nerves might be seen passing upwards among the follicles, into which they might possibly be directed; but he was not aware that they had ever been seen to enter them. He had, however, satisfied himself, in reference to the tactile hairs, that after two or three nervous bundles of considerable size had entered their outer coat, the nerve-tubes were spread out on its inner surface; that they next divided into two or more branches, after which, losing the white substance of Schwann, they appeared as pale lines, or parallel striæ, and ultimately faded into the surrounding tissue, but never, so far as he could discover, had their termination in loops. In many instances no division of the nerve could be detected, but the other appearances were constant. To the tactile bristles of the seal the nervous supply was very large, but for observation Mr Dalzell preferred the young tactile follicles of smaller animals where the parts were clear and no dissection necessary. His observations on this subject received confirmation from the discovery of similar terminations in other parts by Henle, Schwann, Hannover, and Tod and Bowman; but it was to be regretted that the disappearance of the physical character of the nerve-tube, which followed the loss of the white substance of Schwann, put it out of the power of the observer to trace them entirely out.

3. *Dr Haldane* gave the result of some experiments on the urine of a horse to which large doses of tart. of antimony had been administered It was a remarkable fact, that doses of an ounce, or even larger quantities, of tartar-emetic could be administered to the horse for a length of time, without producing any effect whatever. In the urine of a horse so treated, Dr H. had, by careful analysis, detected antimony, but only in very minute quantity.

PARISIAN MEDICAL SOCIETY.

The election of office-bearers for the present year took place on the 18th and 24th days of December 1852, when the following gentlemen were elected :—

President,—George Harley, M.D. Vice-President,- William O. Priestley, M.R.C.S.L. Treasurer,—John S. Sanderson, M.D. Hon. Secretary,—Robert Bowman, M.D. Council,—Charles Murchison, M.D.; James Barnston, M.D.; John S. Erskine, M.D.; Thomas Wheatley, M.R.C.S.; A. T. Jones, Esq.

The Society, from the increase of members, has been obliged to remove to more commodious rooms, in Rue Monsieur le Prince, No. 44; and at the begininng of this, its sixteenth session, is in a most flourishing condition.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Pathology of the Bronchio-Pulmonary Mucous Membrane.* By C. BLACK, M.D., Chesterfield, Bachelor of Medicine, and formerly Medical Scholar in Physiology and Comparative Anatomy in the University of London; Fellow of the Royal College of Surgeons of England, etc. etc.—(*Continued from page* 202.)

Pulmonary Cellulitis.

BY the above term I propose to designate inflammation confined to the epithelium of the pulmonary cells,—a disease which is extremely prevalent amongst children, and often regarded as pneumonia, and which is not rare even in adults. It indeed bears the same relation to pneumonia as epithelial bronchitis does to the severe variety of that disease, inasmuch as the products of the inflammation, like those of epithelial bronchitis, appear on the free surface of the membrane; whilst, in pneumonia, intercellular exudation, which corresponds with the submucous exudation of severe bronchitis, occurs. Owing, however, to the anatomical arrangement of the pulmonary cells, and to the immense distribution of vessels on their walls, it is evident that epithelial inflammation in this situation must be more intense than in that of the bronchi; and that, consequently, exudation, instead of not exceeding the demands of an excessive cell-growth, is poured out in the same copious manner, and undergoes the same changes, as that on the free surface of the membrane, in the severe kinds of bronchitis.

There are in this affection, as in the varieties of bronchitis already detailed, two pathological conditions of the membrane, each of which is associated with particular phenomena, which it will be proper to notice under their respective heads. But before doing so, it may be observed, that cellulitis is more common in the lower than in the upper half of the lungs; in the posterior than in the anterior portions of the lungs; in infants and children than in adults; that it may occur suddenly and at once; or that it may supervene on either the epithelial or the more severe variety of bronchitis; that when it does so, it is owing to the extension of inflammatory action by continuity of membrane, and is associated with capillary bronchitis,

which has superseded, in a great measure, the bronchitis of the larger tubes; and that, when it occurs in the upper half of the lungs, it is generally dependent on tuberculous or other deposit in the pulmonary tissue.

First Pathological Condition of the Bronchio-Pulmonary Membrane in Cellulitis.

The first pathological condition of the bronchio-pulmonary membrane in cellulitis is, in every respect, similar to that of the corresponding stage of bronchitis, and consists of an extremely vascular engorgement, accompanied by total suspension of the accustomed secretion. When the capillary bronchi are likewise implicated, spasm of their muscular fibres is associated with the above-mentioned conditions of the membrane. The appearance of the latter is observed at this period to vary in colour from a deep reddish-brown to a bright crimson.

The degree of shading, however, is not equal throughout the affected pulmonary cells; the edges of their apertures, where the membrane is folded on itself, being of a more deeply reddish-brown or mahogany colour than their interior, which is of a brighter crimson hue. When the capillary bronchi are also affected, their lining membrane is similar in coloration to the interior of the cells, the depth of colour gradually diminishing until it is lost in the natural hue of the fourth or fifth gradation of the bronchi. The whole structure is turgid and puffy, the natural diameter of the pulmonary cells and minute bronchi is diminished, and the cilia of the latter tubes are erect, and are seen by the microscope to project at right angles from the surface of the epithelium on which they are set. This diminution in the capacity of the pulmonary cells and minute bronchi alters the breathing power of the lungs, which undergoes a corresponding deficiency, and hence the sensation of the want of breath is excited. Hence also increased action of the lungs, to compensate for the deficient inflation. Owing now to the dryness of the membrane, and consequently to the direct application of air, surcharged at times with floating particles of dust, to the sentient membrane, which is rendered doubly irritable by the pressure of its engorged capillaries on the terminal fibres of its nerves, cough is present. From the excess of blood in the affected part, a sense of weight, and sometimes of burning, is experienced by the patient. Owing, too, to the same cause, the percussion-note of the chest is reduced, and the conducting power of the lung increased.

This dulness on percussion is confined to the seat of disease, which, as a general rule, is the posterior and lower half of the chest; whilst on its confines the percussion-note is not unfrequently of elevated pitch. If one lung only suffer, the opposite lung may take on a compensatory action, and its function will consequently be increased. The breathing is now deep; the ribs on the sound side are unusually

elevated and everted during inspiration; the intercostal spaces are swelled out, and the number of respirations is less than when mere activity of function is the compensatory mode adopted by nature. Where the above mode of compensatory action prevails, the rhythmic movement of the two sides of the chest is unequal, that of the diseased side stopping short of the degree of elevation of the sound side, with which, however, it descends in rhythmic order from its own point of elevation. But when mere activity of function is the compensatory mode adopted, the rhythmic inequality of the two sides of the chest is not so strongly marked; the elevation and eversion of the ribs, on the sound side, exceed but very slightly, and sometimes not at all, those of the diseased side; but the number of respirations is always increased. The particular type of the disease determines the particular compensatory mode of action, since we invariably find that the former method is associated with the sthenic, and the latter with the asthenic, form of cellulitis.

Although the condition of the bronchio-pulmonary membrane is, at this stage, such that it produces a manifest and palpable deadening of the percussion-note, a diminution, from the first, of the vesicular murmur, when the capillary bronchi are implicated, and the reverse, at the very commencement, followed very quickly by a diminution of the vesicular murmur, when the cells alone are affected; yet it never proceeds, like pneumonia, to the extent of producing the characteristic physical signs of perfect consolidation of the pulmonary tissue. Such is the diameter of the pulmonary cells, and such the anatomical structure of their walls, that the mere engorgement of their vessels is of itself insufficient to produce complete pneumonic consolidation. The vesicular murmur is consequently always present; nevertheless it is invariably diminished in intensity after the tonicity of the pulmonary capillaries is, in a great measure, destroyed by the continued pressure of the blood upon their walls. Until such is the condition of the capillaries, the capacity of each cell is sufficiently great to allow a quantity of air to make considerable pressure on its walls during inspiration, and the contractility of its walls is sufficiently active to oppose such expansile distension; consequently these two forces, acting in opposition to each other, generate an increased vesicular murmur, which the increased conducting-power of the lung readily transmits to the walls of the chest, and to the ear of an observer. With, however, the increasing exhaustion of the vital tonicity of the capillaries, there is such a reduction in the capacity of each cell, owing to excessive vascular engorgement, that but comparatively little air is admitted into it; whilst, owing to the lateral pressure of the capillaries on the individual fibres of the cell-walls, and to the partial exhaustion of the contractility of such fibres by previous over-action, and probably now by defective nutrition, the anti-expansive power of the cell-walls is diminished, so that a concurrent diminution of the two generating causes of sound gives rise, as a necessary consequence, to a diminished vesicular murmur.

When the capillary bronchi are affected, the air, during inspiration, is in consequence unable to enter the pulmonary cells in quantity sufficient to excite the contractility of their walls by distension; therefore, as the capacity of each cell is diminished, as the generating causes of sound are but feebly opposed to each other, the vesicular murmur is from the first below the standard of health. But the signs which take their expression more particularly from the absolutely physical condition of the lung, rather than from this condition in conjunction with the vital forces of the lung, undergo a steady increase in proportion to the increasing density of the lung. Thus, as a general rule, the vocal resonance of the affected portion of lung augments in proportion to the increase of vascular engorgement; consequently the vocal resonance in cellulitis is always increased whether the vesicular murmur be so or not. For the same reason, the percussion-note is always deadened; whilst tubular breathing and bronchophony are always present in the affected part, in which, however, they are modified by the extent and degree of vascular engorgement, by the particular portion of lung, and also by the relation of the bronchial tubes to the surface thereof.

It is certain that, when this pathological condition of the bronchio-pulmonary membrane is at all extensive, not only are the functions of the affected organ deranged, but the whole system will, in consequence of the many intimate and distant sympathies which exist between its different parts, feel the loss of equilibrium thereby occasioned. Hence the effect produced on the vascular and nervous systems, and through them, on the different secretions, appetites, propensities, and feelings.

As to the indications of treatment, they are in general so similar to the corresponding pathological condition of the membrane in severe bronchitis, that reference only needs be made to the treatment of the latter.

Second Pathological Condition of the Bronchio-Pulmonary Membrane in Cellulitis.

The second pathological condition of the bronchio-pulmonary membrane in cellulitis is characterised, as in the different forms of bronchitis, by complete exhaustion of the vital tonicity of the affected capillaries, and by exudation. The membrane has now lost its dryness, and to some extent its puffiness and deep coloration. A bright pink or lake-red hue has superseded the deeply reddish-brown or mahogany tint of the previous stage. The interior of the pulmonary cells, their angles, and the terminal bronchi generally, when the latter are implicated, exhibit more or less of a uniform depth of colour.

The occurrence of exudation produces an alteration in the auscultatory signs of the chest, which, in reference to the resulting rhonchi, are moist in their character, as in the second stage of bronchitis.

Their mode of production, although the subject of much difference of opinion, is readily deducible from the pathological condition of the pulmonary cells. These (the cells) are more or less occupied by a fluid, their free surface is slightly irregular from the coagulation of exudation, whilst their walls are surrounded by vessels more or less engorged with blood. It is therefore evident that the air, on entering the pulmonary cells, has to encounter the resistance of this fluid and of the cell-walls, to which may be added whatever resistance the irregularity of the free surface of the cells can offer. The resistance offered by the fluid is overcome by the entering air insinuating itself between its particles, which envelop it in a thin film, constituting a minute bubble, which bubble, on reaching the surface of the fluid, encounters the resistance of the cell-walls, by which it is broken, and sound is emitted. The quantity of fluid in the air-cells, its viscidity, and the mode of breathing, influence the character of this sound, the differences in which are expressed by the terms crepitant and subcrepitant. If the quantity of fluid is sufficient to fill the cells at the extreme of expiration, if it is very viscid in its nature, and if the breathing is hurried rather than deep, the bubbles formed are small and numerous, the bursting of which keeps up a succession of fine sounds or cracklings, similar to that produced by slowly drawing the back of the closed hand over a shortly bearded chin. This is the crepitant rhonchus—a sound but seldom heard in cellulitis.

If, on the other hand, there is the same quantity of fluid present in the cells possessing the same viscidity; but if the breathing, instead of being hurried, is somewhat deep, the bubbles formed are larger and fewer than in the previous instance, and thus, by their bursting, a coarser sound than the crepitant is emitted, to which the term subcrepitant has been given. This is the sound most frequently present in the second pathological stage of cellulitis; but the conditions of its production are not always those above stated. They, on the contrary, rather consist in the presence of a quantity of moderately tenacious fluid, accompanied by slightly hurried and forcible breathing. These conditions of the fluid and of the breathing are, as a general rule, present in the second stage of cellulitis; therefore the subcrepitant rhonchus is the generally characteristic moist sound of this pathological stage of the membrane. If, however, with this condition of the fluid contents of the cells, the breathing, instead of being hurried, is either hurried and deep, or the latter only, larger bubbles than are necessary to constitute the subcrepitant rhonchus are formed in the pulmonary cells, in which, by their bursting, a still coarser sound than the latter is produced, to which the term submucous rhonchus is given. This sound is heard in the sinking stage of the disease, in which it is not only formed in the pulmonary cells, but also in the minute bronchi, owing to the accumulation of fluid in the latter tubes.

Clinical observation further shows, what pathological deduction

seems to confirm,—namely, that the quantity of fluid in the air-cells influences the duration of the particular rhonchus present. If the quantity of fluid is small, it, in consequence of the provision made by the contractility of the fibrous tissue, favoured occasionally by position, of the cells, for the outward tendency of fluids, accumulates at the apertures of the cells, owing to which the entering and emergent air only is enveloped in films of fluid, and thus the rhonchus is limited to the commencement of inspiration and the end of expiration. But the inspiratory rhonchus is certainly more intensified than the expiratory, owing to the inward current of air encountering greater opposition than the outward current, and having, as a consequence of such opposition, to overcome the resistance of the cell-walls, which thus furnish a condition for the production of sound. This sound is, in the above-mentioned condition of the cells as to fluid, composed partly of a moist and partly of a dry rhonchus in the following manner. The air, on entering the pulmonary cells, encounters the small quantity of fluid collected at their apertures, on passing through which a moist rhonchus is produced; but the cells not yet being distended to their full extent, the inspiratory air lifts their walls above the surface of the contained fluid, in doing which it encounters the passive resistance of such cell-walls free from fluid, and also whatever resistance the irregularities of their free surface, caused by inflammatory exudation, can offer, whence arises a dry, rough, and harsh murmur, as the extreme of inspiration, but as this only. On the contraction of the cell-walls in expiration, the only resistance offered to the emergent air is by the fluid situated at the apertures of the cells, and here, therefore, sound is produced, which, owing to the pathological condition, and to the situation of the parts, is of a crepitant or subcrepitant character, and limited to the last third of the expiratory movement. On a careful analysis, however, of the sounds heard during the movements of the pulmonary cells in such instances, it is frequently observed that, whilst the rhonchus, produced by the air entering the cells through the fluid collected at their apertures, is of a subcrepitant character, that heard as the air passes out of the cells, through the same fluid, is distinctly crepitant. This difference in the quality of the two rhonchi is owing to the difference in the force with which the air enters and emerges from the pulmonary cells. The entering column of air, moving with greater force than the emergent column, produces larger bubbles than the latter, and therefore gives rise to a coarser (subcrepitant) rhonchus.

If a greater quantity of fluid than that which has already been stated, occupies the cells, the rhonchus is heard during two-thirds of the duration of the respiratory movements; but if the whole cavity of the cells is engaged, the sound is limited to the extreme of inspiration and the commencement of expiration, at which periods the bubbles, rising to the surface of the contained fluid, come into contact with the cell-walls, by which their disruption is occasioned. It

is, however, seldom indeed that anything approaching to complete distension of the cells occurs in cellulitis; therefore the rhonchus heard has not the limited duration named; but, on the contrary, it is generally present during two-thirds of the inspiratory, and the latter half or third of the expiratory movement. The cause of this partial distension of the cells only, seems to be, in a great measure, owing to the particular nature of their contained fluid, which being less viscid and tenacious than the sputum of pneumonia, is more readily propelled into the minute bronchi, in which it furnishes a condition for the production of a submucous rhonchus. But, although complete distension of the cells with exudation never occurs in cellulitis to the extent of producing perfect consolidation of the pulmonary tissue, yet we find, on microscopic examination of the sputa, that perfect casts of individual cells are frequently present, and that such a condition has existed, although not in that degree which produces the physical signs of perfect consolidation.

As to the resonant property of the lungs, in this pathological condition of the bronchio-pulmonary membrane, we find, that the continued presence of an excess of fluid in the pulmonary cells and minute bronchi, still keeps the percussion-note below the standard of health; and, at the same time, so far muffles and chokes the conduction, to the surface of the chest, of the vibratile motions originating in the production or propagation of sound from the subjacent bronchi, that bronchophony and tubular breathing are but indistinctly heard. These sounds, however, together with a diminution in the vesicular murmur, a harshness in the quality of the latter during inspiration, particularly forced inspiration, and a degree of prolongation of the sound of expiration, exist for some time after the total cessation of the more manifest signs of disease; and this they do, because the basement structure of the mucous lining of the pulmonary cells is hypertrophied by inflammatory deposit, which causes the increased thickness of the cell-walls to encroach on the normal capacity of the cells, to impede their contractile power, to render the pulmonary tissue a better conductor of vibrations than it is in health, and to modify, as above stated, the static and dynamic properties of the chest. I have found that, in such instances, the hypertrophy of the basement membrane gives to the latter a diameter varying from $\frac{1}{10000}$th to $\frac{1}{16000}$th of an inch; that, under a steadily progressive tendency to a restoration of the normal condition of the structure, such hypertrophy requires weeks, and not unfrequently months, for its complete removal; and that it occasionally leaves a degree of permanent thickening of the cell-walls, which thickening, in estimating the breathing-power of the lungs, is appreciable in proportion to the extent of surface involved, to the consequent reduction of capacity in the pulmonary cells, and to the degree of compensatory power which is manifested by the uninjured portions of lung, in comparison with the deficient action of those portions, the breathing-power of which has been injured by disease.

With respect to the physical appearance of the sputa discharged in cellulitis, and to the pathological conditions on which they depend, it is found, that the very first portion discharged, presents to the naked eye the same colour and consistence as the first portion of sputum expelled in bronchitis; and that its microscopic appearance shows it to consist of the epithelium of the pulmonary cells, of well-developed mucus-corpuscles, and of a few points and masses of coagulated exudation, suspended in a fluid menstruum. (Fig. 7). The first pathological step, then, towards the establishment of a discharge from the affected membrane, consists, in cellulitis as in bronchitis, of the partial shedding of the epithelium, of rapid development of those portions of epithelium which retain their vitality into mucus-corpuscles, and of the coagulation, on the denuded portions of the basement membrane, of the fibrinous parts of the exudation, some of which are detached by the efforts in coughing, and consequently appear in the first sputa expelled. If the inflammation is confined to the pulmonary cells, the next quantity of sputum discharged is more consistent, viscid, and tenacious, presenting to the naked eye a yellowish or greenish-yellow colour. This kind of sputum has been described as a more frequent sign of pneumonia than even the rust-coloured expectoration; but, according to the pathological views here adopted, such is not the case. On the contrary, it, in conjunction with the physical signs before named, is characteristic of cellulitis; whilst pneumonia proper—*i.e.*, inflammation involving, by exudation, the whole substance of the cell-walls, and of the intercellular spaces and connecting tissue—is all but invariably accompanied by rust-coloured sputum during the first half of the expectorant stage. Blood, therefore, is an accidental constituent of the sputum of cellulitis; but as a general rule, a constant, and therefore a necessary, one of that of pneumonia.

Under the microscope, the former kind of sputum is seen to contain fewer epithelial patches and mucus-corpuscles than that first discharged. The epithelial-rather now give place to basement patches, which are commingled with numerous exudation masses and corpuscles, and a few casts of individual pulmonary cells. (Fig. 11.)

The intensity of the inflammation is represented by the quantity of exudation masses and corpuscles present; whilst the vital energy of the system is determined by the shape, and, to a certain extent, by the degree of development which these corpuscles have attained. In sthenic cellulitis they are perfectly spheroidal in shape, and exhibit a development in size proportionate to the length of time which the matrix, in which they arise, has been attached to the denuded basement membrane. Hence they vary in size from $\frac{1}{1800}$th of an inch to a diameter but little larger than the nuclei from which they arise.

If capillary bronchitis is associated with cellulitis, there are, in addition to the above-mentioned structures, casts of the minute

bronchi, which may measure from $\frac{1}{235}$th to $\frac{1}{1800}$th of an inch in their longest diameter, and which are commingled with a greater

Fig. 11.

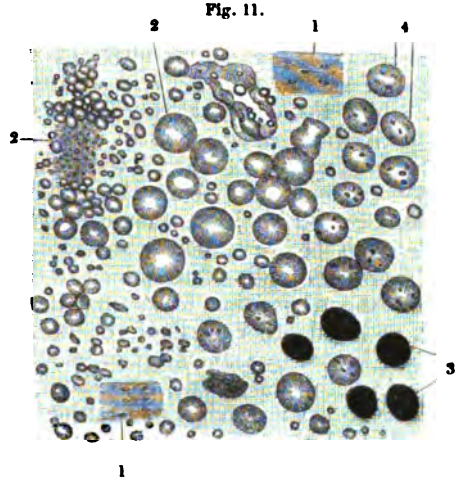


Fig. 11.—1. Portions of basement membrane.
2 Exudation masses and cells.
3 Casts of pulmonary cells.
4. Mucus-cells.

number of mucus-corpuscles than is present in cellulitis only. It is in cases of this kind that the lesion termed "bronchial abscess" is apt to occur. After desquamation of the epithelium of the affected bronchi and pulmonary cells, exudation is poured out in quantity sufficient to effectually obstruct the former tubes, and consequently to prevent the air from gaining access to the corresponding pulmonary cells. A complete or partial collapse of the latter takes place accordingly, as they are free from exudation or occupied by it. It has been already shown, that exudation seldom or never occupies the whole cavity of the cells; nevertheless it does so, in reference to individual cells, in every case of cellulitis. The explanation, however, of this occurrence is, that such individual and complete cell-distension is not the result of exudation from any one particular cell, but that it rather results from the accumulated contents, in one cell, of several cells opening the one into the other, in connection with terminal bronchi, and that the distension occurs in cells which open directly into the bronchi; because, during the establishment of pulmonary collapse, the contractility of the cell-walls produces an outward tendency of the contained fluids, which are thus urged forward to the immediate seat of obstruction in the bronchi, into which they cannot of course pass. In the course of a terminal bronchus several such distensions of immediately communicating cells may exist; and, as the pulmonary tissue in connection with them is now in a state of collapse, and as the exudation, from its inherent vitality,

passes more or less into cell development, such occurrences obtain the features of bronchial abscess, to which further consideration will hereafter be given.

In the asthenic form of cellulitis, the total exhaustion of the vital tonicity of the affected capillaries allows fluid in such abundance to escape, that the sputa are much less viscid and tenacious than in sthenic cellulitis; whilst the comparatively deficient vitality of the exudation limits, as it were, cell-growth to a development of less diameter, and of a less spheroidal shape, than that of healthy or sthenic exudation. Under a persistence of capillary engorgement, after exudation has occurred in either sthenic or asthenic cellulitis, there is, for the reasons before stated, a strong disposition in the exudation to pass from its primitive form of cell to that of the plastic and pus type; but the disposition to assume the last-mentioned character is here, as in all other parts of the bronchio-pulmonary membrane, more manifested by asthenic than by sthenic exudation.

When, however, in both sthenic and asthenic cellulitis, the sinking stage of the disease has arrived, the condition of the system is one and the same in both instances; therefore its vital manifestations, as evidenced in the nature of the pulmonary discharge, are also the same in degree and speciality of character. At this stage of the disease we find that, so long as the power of expectorating continues, the sputa are increased in quantity, and altered more or less in consistence and colour. From a thick yellowish-white, the fluid now discharged has passed to a dirty brown, or reddish-brown, colour; and, although moderately tenacious, it is less viscid and consistent than at an earlier stage. Its microscopic characters are well delineated in the following figure, which represents the appearance of the sputum, in a case of extensive cellulitis, forty-eight hours before death.

Fig. 12.

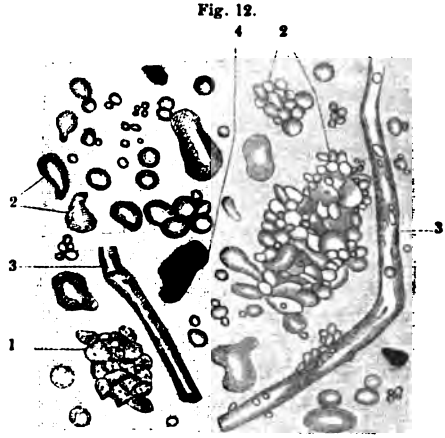


Fig. 12.—1. Epithelial patch.
2. Exudation masses and cells, showing their deficient vitality by their irregular shape.
3. Bronchial casts.
4. Mass of simply coagulated exudation.

At a more advanced period of the sinking stage, the sputa are yet more copious, of a thinly viscid character, and of a prune-juice colour, or of various modifications of this colour. Under the microscope, such sputa are observed to have manifested but a mere attempt at cell-growth, and to present shrivelled and abortive cells, cell-walls, minute basement patches, and occasionally bronchial casts, as in the subjoined figure, which represents the microscopic appearance of sputa discharged, during the last hour of life, in a case of cellulitis supervening on bronchitis of ten days' duration.

Fig. 13.

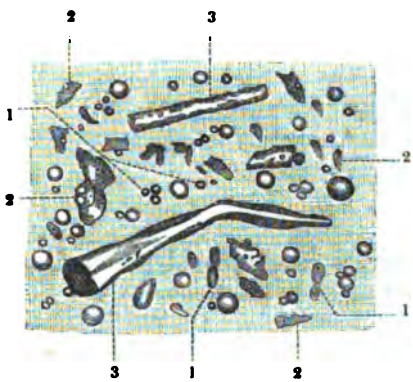


Fig. 13.—1. Minute and abortive cells and cell-walls.
2. Minute basement patches of exudation.
3. Bronchial casts.

The pathology of the above change, as contrasted with the appearance of healthy exudation, is rendered intelligible by reference to the failing powers of respiration, to the consequent deterioration in the quality of the blood, to the almost total arrest of nutrition thereby occasioned, and the consequent depression of functional energy, and to the particular effect of carbonic acid gas on the nervous system. In the establishment of these conditions, the first change is in the quantity of the nutritive elements of the blood, which, owing to the excess of egesta over the quantity of ingesta, is brought below the standard of health; next, the quality of the blood is deteriorated by the partial retention of carbonic acid gas, which destroys, to a certain extent, the inherent vitality of its organic constituents. As a consequence of this increased fluidity of the blood, and of the deficient vitality of its organic elements, the nutritive blastema poured forth, either in the process of nutrition or of morbid action, is deficient in quantity and incapable of perfect development. Hence arise structural deterioration and consequent modification of function, to which latter the poisonous effect of carbonic acid gas on the nervous system greatly contributes. To the action of this gas on the colouring matter, and also on the inherent vitality of the cell-

walls, of the red globules of the blood, to the mutual interchange of fluid between the serum and the fluid contents of the red globules, in the maintenance of their respective specific gravities, to the complete exhaustion of the vital tonicity of the pulmonary capillaries during the sinking stage, to their now passive engorgement, and to the ready escape of fluid through their inter-molecular spaces, must be ascribed the frequent presence of the colouring matter of the blood in the sputa discharged during the last hours of life.

As to the chemical composition of the sputa in cellulitis, after the full establishment of expectoration, the mean of eighteen analyses, uninfluenced by hereditary taint or constitutional peculiarity, gave,—

	In 100 parts.
Water, ...	94·80
Organic matter, ...	3·68
Chlorides of sodium and potassium, Alkaline sulphates and phosphates, ...	1·47
Sulphate and phosphate of lime, ...	·05
	100·00

When, however, a constitutional peculiarity, a particular diathesis, or another disease, is associated with cellulitis, the exudation from the pulmonary membrane contains frequent evidence of the presence of certain products, dependent on such peculiarity of constitution or other disease in the system.

I have, consequently, found the uric acid diathesis furnish urate of ammonia to the sputa of cellulitis,—the oxalic acid diathesis, distinct crystals of the oxalate of lime, together with the occasional presence of cystine,—and jaundice, cholesterine and the colouring matter of the bile.

On these points the following cases are interesting:—

CASE I.—*Acute Articular Rheumatism, associated with Pulmonary Cellulitis, with the Occasional presence of Urate of Ammonia in the Sputa.*

L. P., ætat 55, of a nervo-sanguineous temperament and intemperate habits, was seized with articular rheumatism on September 4th, 1852. The attack commenced in the ankle-joints, and affected the larger joints in succession. Present also epithelial bronchitis. On the second day the quantity of urine discharged in twenty-four hours was eighteen ounces. Colour, deep amber; sp. grav. 1·024; temperature, 94° F.; deposits on cooling a thick yellowish-red sediment of urate of ammonia. Expectoration of a thinly viscid, frothy sputa, consisting of epithelial patches and fully developed mucus-corpuscles. During the following night breathing became oppressed, accompanied by a sense of weight and burning in the posterior and lower half of the right side of the chest, by cough and partial suppres-

sion of expectoration. The physical signs indicate the existence of cellulitis in this portion of the lung. On the morning of the 3d day urine of a reddish-brown colour; sp. grav. 1·022; temperature 94° F.; a sediment of urate of ammonia on cooling. Expectoration re-established, and hourly becoming more copious. Under the microscope, it is seen to consist of a few epithelial patches, mucus-corpuscles containing distinct globules of urate of ammonia, free urate of ammonia, exudation masses and cells, and irregularly shaped flakes and masses of fibrine, as represented in the adjoining figure.

Fig. 14.

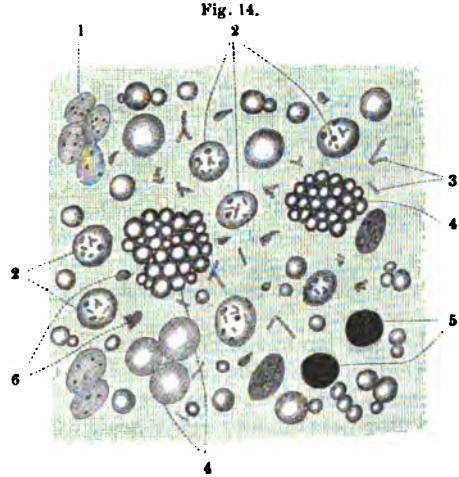


Fig. 14.—1. Epithelial patch.
2. Mucus-cells containing urate of ammonia.
3 Free urate of ammonia.
4. Exudation masses and cells.
5 Casts of cells.
6. Nodules of simply coagulated exudation.

During the third and following day, the urate increased in the urine and decreased in the sputa. On the fifth day, although the sputa were very carefully examined, no trace of it could be discovered. On the ninth day it re-appeared in the sputa, at which time the urine was quite free from it; but on the morning of the eleventh, and during the remainder of that day, a copious excretion of the urate took place from the kidneys; and on the following day there was no appearance of it in the sputa. From this time the urate was confined to the urine, in which, on the fourteenth day of the disease, a very copious sediment occurred. The cellulitis henceforth rapidly recovered; and, on the twenty-second day, convalescence from rheumatism was established.

To prove chemically the existence of the above salt, an ounce of the sputa was evaporated to dryness, at a gentle heat, in a porcelain capsule, over a sand bath; the dry residue was triturated in a mortar, and afterwards digested in warm distilled water. The whole

was next thrown on a filter, and to the filtered fluid animal charcoal was added, and digestion, at a gentle heat, again employed for an hour. After procuring, by repeated filtrations, a clear fluid, the latter was concentrated. A few drops of the concentrated solution were allowed to crystallise on a slip of glass, on which the following crystals were seen under the microscope. (Fig. 15.)

Fig. 15. Fig. 16.

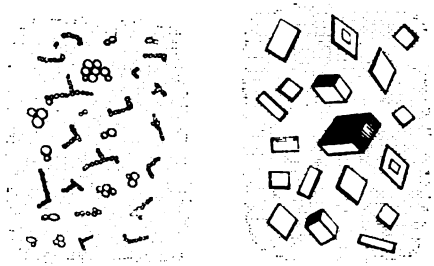


Fig. 15.—Urate of ammonia.
Fig. 16.—Crystals of uric acid.

To the remaining portion of the concentrated solution a few drops of hydrochloric acid were added; and in a short time crystals of uric acid, as above represented, were procured. (Fig. 16.) These were soluble in liquor potassæ and nitric acid, which latter, by evaporation, gave a pink residue, which became purple on being exposed to the vapour of ammonia, thus demonstrating the presence of the purpurate of ammonia of Prout, and the murexide of Liebig.

It is clear that, in the above case, which is but one of a number which has occurred to me within the last three years, the presence of rheumatism produced an occasional modification of the pulmonary discharge which existed as a consequence of cellulitis, and that this modification was manifested by the presence of urate of ammonia in the sputa, whenever it had, in consequence of a temporary suspension of the eliminating action of the kidneys, accumulated in undue proportion in the blood. So soon as the excretory power of these organs was again excited, an abundant deposit of the urate appeared in the urine, and all trace of it quickly disappeared from the sputa. When present in the latter, it existed both in a free state and as a deposit from the contents of the mucus-corpuscles, whilst the exudation masses and individual exudation-cells were invariably free. This fact seems to depend on the difference in the conditions of growth between the mucus-corpuscle and the exudation-cell, of which the following explanation may be given:—

The germinal point or centre of the future epithelial or mucus-corpuscle is surrounded, in its nutritive matrix, by the necessary

organic elements for its perfect development, which elements require the incessant yet gradual action of the saline constituents of the blood to render them subservient in the process of growth. As soon, therefore, as the smallest particle of the superficial layer of the basement membrane around a germinal centre has been rendered subservient to growth by the action of the saline fluid which permeates the basement structure, it is applied by such germinating centre to effect the perfect development of the latter, in doing which a portion of its organic elements is exhausted by the vital extension of the cell-wall; whilst its saline constituents, not thus necessarily localisable, pass in combination with the unappropriated organic elements into the cavity of the cell. Now, if the saline constituents of the blood are in excess, as they necessarily are in similar cases to the above, they must therefore exceed the natural demand in the growth of the epithelial cells of the bronchio-pulmonary membrane, and this excess will result in the deposition or crystallisation of the salt, which is non-essential to the existence of such cell. But this deposition or crystallisation will not take place so long as the cell is in absolute growth; because the same degree of temperature and of moisture, which maintained the solubility of the salt during its passage into the cell, is still present to maintain its solution within the cell. When, however, the cell has acquired a perfect development, and when, in consequence, it becomes a detached and isolated body, its temperature immediately falls, the fluidity of its contents diminishes by evaporation, which may cause a portion of its contained organic constituents to assume the semi-solid condition (hence the formation of granules and nuclei), whilst any abnormal salt is at once thrown down by amorphous deposition or distinct crystallisation.

In the germination and growth of the exudation-cell, however, the conditions are different; for here the germinating point is not situated within the substance of the basement membrane, but upon its free surface left bare by the shedding of its epithelium; and its connection with the basement structure is not a normal, but simply an abnormal, accidental, or adventitious connection, which results from the particular pathological condition of the part, and which resembles, in a great degree, the connection which exists between a plant and its parasite.

Growth, therefore, takes place by imbibition of the organic constituents, which, with the germinating point, or points, form the coagulated portion of the exudation, and which are rendered subservient to the vital activity of such germinating points or centres, by the resolvent action of the fluid portion of the exudation poured out upon the denuded basement membrane.

As, however, this fluid portion of the exudation is immediately exposed to the direct contact of the air, by which its temperature is reduced and a part of it evaporated, it is evident that any abnormal salt of the blood, present in the fluid exudation, will undergo imme-

diate deposition or crystallisation, and will not, therefore, enter the exudation-cell. Hence the reason why, in the above case, urate of ammonia was present in the epithelial or mucus, and not in the exudation-cell: and hence also, in explanation of the immunity of the latter cell, the reason why this salt existed as a free constituent of the sputa.

CASE II.—*Oxaluria—Pulmonary Cellulitis—Occasional Presence of Oxalate of Lime in the Sputa.*

W. S. B., a medical student, ætat 20, of a tuberculous constitution, and nervo-sanguineous temperament, consulted me on October 6th, 1852, for an occasional sharp, cutting pain in the loins, followed by a discharge of very bright, amber-coloured urine, which became extremely turbid on cooling. He stated that he had thus suffered for three months, that the pain generally affected him very suddenly, and that it was so lancinating and acute as to cause him involuntarily to sink beneath it. He had lost flesh, and of late his general health had rapidly been getting worse. His countenance is anxious and haggard; skin pale, harsh, and shrivelled; tongue large, flabby, red at the tip and edges, but covered with a white creamy fur in the centre; appetite bad; bowels irregular, but the evacuations generally of natural colour and consistence; pulse 89, small and irritable; disposition fretful; sleep disturbed by dreams; frequent desire to pass urine, which is of a clear, bright amber colour when first voided, but deposits on cooling a heavy white sediment; sp. grav. 1·026; temperature, 93° F. On heating the urine gently, the greater part of the sediment is dissolved, whilst almost the whole of the remainder is soluble on the addition of nitric acid; but a small proportion resists the combined action of both heat and nitric acid. A drop of the sediment, placed under the microscope, exhibits great numbers of the usual crystalline forms of urate of ammonia and beautiful octahedral crystals of oxalate of lime, crossed diagonally by light-coloured bands. In the supernatant fluid, the ovoid and dumb-bell forms of oxalate of lime abundantly exist. There are also present in the urine epithelial scales both from the lining membrane of the bladder and uriniferous tubes, mucus-corpuscles, and a few exudation-cells and uriniferous casts. After being under treatment for a fortnight, he took cold from incautious exposure, and pulmonary cellulitis supervened. On the establishment of the expectorant stage the sputa were daily examined, the result of which proved the occasional presence of the above salts, particularly at the time when they were being eliminated by the kidneys in small proportion, and when, in consequence, their accumulation was taking place in the blood. Crystals of the oxalate were present in the mucus-corpuscles to a limited extent, being for the most part free in the sputa; but the urate of ammonia was not infrequent as a constituent of the contents of the mucus-cells.

Fig. 17.

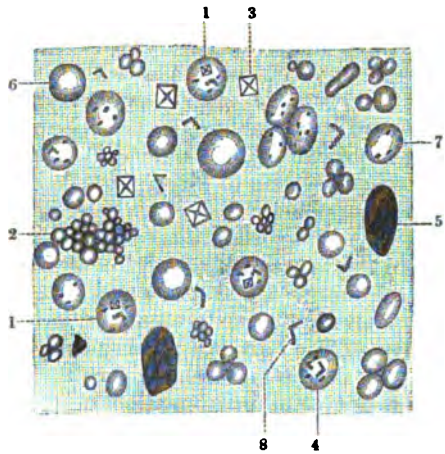


Fig. 17.—1. Mucus-cells containing urate of ammonia and octahedra of oxalate of lime.
2. Mass of exudation-cells.
3. Free crystals of oxalate of lime.
4. Mucus-cell containing urate of ammonia only.
5. Coagulated fibrine, showing the commencement of cell development.
6. Exudation-cell of full growth.
7. Mucus-cell of full growth.
8. Free urate of ammonia.

To prove chemically the existence of the oxalate of lime, two ounces of sputum were evaporated to dryness over a sand bath, and the dry residue was incinerated. To the ashes thus left, distilled water was added, which dissolved out the soluble salts. The insoluble salts were then collected, and thrown into nitric acid and water, when a distinct effervescence occurred, together with the escape of carbonic acid gas and the solution of the salts. To a few drops of this solution in a watch-glass, a drop or two of a solution of oxalate of ammonia were added, which produced an immediate white precipitate of oxalate of lime, which underwent no change either in boiling acetic acid or liquor potassæ.

To a drop of sputum between two slips of glass, a single half-drop of dilute nitric acid was added, and the solution of the oxalate was watched under the microscope.

The above case was clearly one of oxaluria, in which cellulitis supervened as a mere accidental occurrence from exposure to cold. It was, however, evident that the bronchio-pulmonary membrane became at times a partial outlet for the urate of ammonia and oxalate of lime, which action, although the result of disease in the membrane itself, nevertheless exerted a salutary influence in the purification of the blood. But the eliminative action of the membrane was here, as in the case first detailed, only manifested when

the excretory action of the kidneys was insufficient to prevent an accumulation of morbid products in the blood, and it invariably ceased for a time directly after the appearance of an unusual quantity of the above salts in the urine. The elimination of such products, therefore, by the bronchio-pulmonary membrane, can only be regarded as an extraordinary occurrence, and vicarious for the time being of a deficient renal excretion. It, nevertheless, shows the conservative tendency of the system at large, and the disposition which exists between structures of a similar nature to sympathise with each other, and to assist, by a compensatory action dissimilar to their own, any part or structure the functional activity of which is for the time unable to prevent the undue accumulation of injurious products in the blood. In the above case the presence of uriniferous casts in the urine would have afforded ground for serious apprehension as to the structural integrity of the kidneys, had not their existence been rightly referred to a degree of inflammation of the mucous lining of the uriniferous tubes, occasioned by the elimination and passage of the oxalate of lime. The vascular engorgement thereby produced, led to epithelial desquamation in the uriniferous tubes, and to exudation upon the denuded basement membrane, and sometimes into the uriniferous tubes themselves, producing casts of the latter in a manner similar in all respects to what has already been stated, in reference to inflammation of the bronchio-pulmonary membrane.

The presence of uriniferous casts was invariably associated with an abundant deposit of the oxalate; and at this time, too, the urine became slightly albuminous. When but a scanty deposit of the oxalate existed, the casts and albumen disappeared almost entirely from the urine, in which they re-appeared on the recurrence of the salt in unusual quantity. These results clearly showed the relation of cause and effect between the oxalate and the other morbid products observed; and the inference thence drawn, was fully justified by the complete and permanent disappearance of the latter, when the natural and healthy action of the kidneys had been fully restored.

CASE III.—*Jaundice—Pulmonary Cellulitis—Cholesterine and Biliphœin in the Sputa.*

S. S., ætat 36, sanguineous temperament, married, and mother of six children, was attacked with jaundice about three weeks before her last confinement, which happened on October 2d, 1852. Medical attendance was not requested until five days after labour, when, in consequence of having taken cold on the previous day, she complained of cough, straitened breathing, together with a sense of weight and uneasiness in the left scapular and subscapular regions. On visiting her, I found the following symptoms and physical signs:—Skin and conjunctiva of a deep yellow colour; tongue coated with a dirty, yellowish-white load; thirst, loss of appetite,

frequent vomiting, constipation, alvine evacuations knotty and clay-coloured; urine, deep greenish-yellow. Respirations twenty-four per minute, somewhat laboured; cough frequent, short, and dry; pulse 94, full and strong; skin hot and dry. In the regions above noted there is comparative dulness on percussion; the vesicular murmur is diminished in intensity, but is nowhere absent in the affected parts; bronchial respiration and bronchophony are heard in low intensity over the seat of disease, together with puerile respiration at the apex of the affected lung.

On the establishment of the expectorant stage, the sputum expelled was similar in consistence to that of ordinary cellulitis, but its colour was of a strongly marked greenish-yellow, from which it never varied during the whole course of the disease. Viewed under the microscope, the usual bodies of cellulitis were observed, in addition to which plates of cholesterine were present.

To prove chemically the existence of the latter, three ounces of sputum were evaporated to dryness over a water bath, and the dry residue was digested in ether for several hours. The solution thus obtained was next decanted and allowed to evaporate. The residue left by evaporation was washed with cold alcohol, which removed a small proportion of oily non-crystallisable fatty matter, leaving the cholesterine almost free from impurity. A small portion, placed under the microscope, exhibited the usual transparent crystalline plates of this substance.

To another portion of sputum a few drops of nitric acid were added, when, after a few minutes, the yellowish hue of the former gave place to a permanent green, thus indicating the presence of bile.

From the foregoing cases, which are but single examples of a number of such that has occurred to me, the following deductions seem to be justified:—

1. That the manifestation of a particular diathesis, or the presence of a particular disease in the system, is capable of influencing the character of the sputa in inflammation of the bronchio-pulmonary membrane.

2. That this influence is manifested by the presence, in the sputa, of the morbid products of the particular diathesis or disease with which inflammation of the bronchio-pulmonary membrane is associated.

3. That such morbid products are eliminated by the epithelial cells of the membrane; that they, therefore, appear as accidental constituents of the mucus-cells; that they likewise escape in the exudation from the denuded surfaces of the basement membrane; and that, for the reasons before-named, they are never present in the exudation-cells.

4. That the bronchio-pulmonary epithelium can thus act the part of an excretory organ; but that this peculiarity of action is

probably rather dependent on the particular character of the blood, than on any selective power inherent in the epithelial cells.

5. That, before such eliminative action takes place, an undue accumulation of morbid products occurs in the blood; that the action of the different organs, intended for the elimination of such morbid products, is for the time being deficient; and that, on its becoming more vigorous, the sputa quickly regain their simply morbid character.

The indications of treatment to be deduced from the foregoing observations, are the same in the second pathological stage of cellulitis as in that of bronchitis involving the submucous tissue, to which latter the reader is therefore referred.

(*To be continued.*)

ARTICLE II.—*On the Improvements which have been Introduced into the Practice of Surgery in Great Britain within the last Thirty Years.* By JAMES SYME, Esq., Professor of Clinical Surgery in the University of Edinburgh.

(*Communicated to the Edinburgh Medico-Chirurgical Society.*)

THE great improvements which in recent times have been so profusely contributed to the well-being of society by other departments of practical science, frequently suggest the question, Has the practice of surgery advanced in a proportionate degree? The older members of our profession, who have not quite kept pace with the march of progress, are apt to express doubts as to any real advantage having been gained of late; and our younger brethren, who are occupied more with learning present opinions than in tracing their origin, although inclined to believe that things are better than they used to be, feel rather at a loss, when called upon, to explain the grounds of their persuasion. In these circumstances, I have thought that it might not be improper for a member of this Society, who can speak from his own observation and recollection, to mention some of the improvements which have been introduced into the practice of surgery within the last thirty years.

In attempting to execute this task, I do not propose to record those particular achievements of operative surgery which merely reflect credit upon the skill and intrepidity of the gentlemen who performed them, or those new modes of treatment which are either decidedly objectionable or of questionable expediency. The excision of ovarian tumours; the aperture of the larger intestine to afford relief from obstructions of the bowels; the opening of the abdominal cavity in search of internal strangulations; the excision of the knee-joint instead of amputation; the remedy of anchylosis by forcible rupture of the adhesions between the articulating surfaces,—and other less pro-

mising proposals, have all had their respective advocates; and it is not improbable that some of them at least may ultimately be established in practice. But in the mean time, I think it is better that our attention, upon such an occasion as the present, should be confined to points of advantage so well ascertained as to leave no room for doubt in any reasonable and unprejudiced mind.

Thirty years ago, a dresser commenced the discharge of his daily duty in the hospital, by spreading a quantity of calamine cerate upon pieces of lint or linen, and cutting some sheets of adhesive plaster into narrow strips. He then proceeded to dress all the suppurating sores, by applying to each a pledget of ointment, next a cushion of carded tow, and finally a long bandage to keep all secure for twenty-four hours. At present the use of greasy applications and elaborate bandaging is almost entirely abandoned in such cases; the water dressing—or wet lint covered with oiled silk—being substituted with great advantage, in regard not only to convenience, but also to promotion of the healing process.

The strips of adhesive plaster prepared by the dresser were devoted chiefly to the treatment of indolent or callous ulcers of the legs, according to the method of Dr Baynton. This was a process so troublesome and expensive, as very much to limit the relief afforded to such cases by rendering them unpopular subjects of hospital treatment. But it is now well known that the hard swelling of the limb, which gives these ulcers their peculiar character, may be quickly removed by the application of a large blister, so as to allow speedy healing and sound cicatrisation without further trouble, and thus place the means of recovery within the reach of the poorest and most destitute of patients. Varicose ulcers of the legs also used to be treated by the same process of strapping with adhesive plaster, but are now known to heal readily under the simple application of the black wash.

There was another class of ulcers held to require very severe means of treatment. This was the syphilitic, or more correctly the mercurial sore, of a sinuous or burrowing character, which is so common wherever the delusion of mercury prevails, in all parts of the body, but especially the inner side of the legs and thighs. Its remedy was believed to be impracticable, except through the agency of caustic potass, a stick of which was pushed into all the winding sinuosities of the sore, and rubbed over the discoloured skin surrounding its orifices, so as to destroy the vitality of the unhealthy textures, with what pain to the patient may be easily imagined, and not readily forgotten by any one who has heard the walls of an hospital resounding with the shrieks of agony so excited. Now-a-days, instead of this severe discipline, a gentle administration of the hydriodate of potass, to the extent of two grains, twice or thrice a-day, and a blister applied over the sore, afford the relief desired not only without suffering worthy of mention, but much more quickly than in the other way. The same sort of constitutional and local treat-

ment is now found sufficient for the remedy of another description of ulcers, which used to be counteracted by measures still more formidable. This was the disease of the shin bone—then confounded with the incurable condition of caries, and supposed to require removal or destruction of the part affected, by rasping, scraping, and the red-hot iron. Chauffers bristling with cauteries are no longer brought into the operating theatre for this purpose; and the only treatment of a local kind deemed requisite, is the application of one or two blisters, followed by water dressing.

A change no less remarkable has taken place in the treatment of simple incised wounds, or those desired to heal by the first intention. Formerly, immediately after their infliction, the edges were closely approximated and retained in contact by strips of adhesive plaster, and covered with pledgets of lint, cushions of tow, and tight bandages. As a necessary consequence of this system, blood always accumulated in the cavity sufficiently to cause separation of the respective surfaces, so that an abscess resulted, instead of the anticipated union. Now, the greatest care is taken to allow the blood free vent so long as it is disposed to escape, any sort of impermeable covering being withheld, while pressure is diligently applied to maintain the raw surfaces in apposition, so as to be within reach of the adhesive effusion by which their union is effected. Instead of suppuration being the almost invariable rule, and cicatrisation a very rare exception, the results are now reversed; and the primary healing of wounds is looked for, if not with certainty, at least with confident expectation.

Not many years ago, one of the most painfully discouraging subjects of surgical study or practice, was that mortification which is so apt to attack the toes of old people, and has been called the *gangrena senilis*. The insidious commencement of its attack; the agonising distress which attends its progress; the inefficiency of measures employed for its remedy, and the inevitably fatal result to which it led, rendered this disease indeed one of the *opprobria* of surgery. According to the ideas then entertained, nothing could be done, except keeping up the patient's strength, by an ample allowance of wine and brandy, dulling his sensibility with opium, and preparing his friends for the issue. But now the case is very different. The morbid action being attributed to excessive irritability, dependent upon weakness proceeding from an imperfect supply of blood, instead of attempting to supply the deficient vigour by stimulants that can do no good, and must hurry on the disease, the treatment is directed with a view to lessening the tendency to overaction by the use of soothing means. The patient is confined to bed, debarred from animal food in every form, with the exception of milk, which, with water, constitutes his only drink, supplied with doses of morphia in proportion to his pain and restlessness, and has the part affected covered with a linseed poultice. Under this system the distressing symptoms gradually disappear, the slough ceases to extend, and after its separation the sore cicatrizes soundly, no difficulty

or bad effects being subsequently experienced in returning to the ordinary habits of diet and exercise. The student, therefore, no longer turns over this page of his principles with a blush for the imperfection of surgery; and the practitioner, instead of being called upon to undertake a hopeless task, has frequently the satisfaction of conducting his patient safely through what might formerly have well been called the valley of the shadow of death.

Within the last thirty years a great revolution has taken place in regard to the performance of amputation. Formerly, upon all occasions, whether for the removal of a finger or one of the larger limbs, this operation was done by circular incision; but now it is no less generally accomplished by the formation of flaps. The advantages urged in favour of the latter method by those who succeeded in establishing it were the facility, rapidity, and consequently smaller amount of pain, attending its execution, together with the superior covering to the bones which was afforded by the resulting stump. Of these considerations, the former has lost its value through the introduction of chloroform; but the latter, which was always the more important of the two, still maintains its ground; and, with the exception of the lower part of a muscular thigh, and the upper part of the leg, where the circular method appears to be preferable, leaves no room for hesitation between the old and present modes of performing the operation.

The ankle-joint and bones of the tarsus are so liable to the incurable condition named caries, which peremptorily requires removal of the affected part, that amputation of the leg was not many years ago one of the most common operations witnessed in hospital practice; and as the patients could not afford the expense of a substitute more costly than the simplest form of wooden pin, the limb upon such occasions was taken off immediately below the knee. But it is now well ascertained that when the disease or injury does not extend above the articulating extremities of the tibia and fibula, the patient may be relieved by depriving him merely of the foot, while the thick integuments of the heel are preserved to form a cushion for supporting the weight of the body. It further appears that upon the stump thus formed the patient may stand and walk with a degree of comfort and security hardly less than when the limb remained entire. And, what is of still more consequence, that the operation, instead of frequently proving fatal, like amputation of the leg, is almost entirely free from danger.

Next to the ankle-joint, the elbow is the articulation most subject to incurable disease of the osseous tissue, and hence used frequently to entail upon surgeons the painful duty of performing amputation of the arm, while the hand was perfectly sound. The inferior extremity being chiefly employed in supporting the body, may be replaced with a substitute by artificial means, much more effectually than the arm, and indeed, so far as both appearance and utility are concerned, with one preferable to the natural limb when it

is much shortened, distorted, or weakened. But the case is very different with regard to the hand, which in its most imperfect condition far surpasses in usefulness the most ingenious contrivance of art, and therefore preservation of the arm, even in the most crippled state, would be a great object for the patient's comfort. It is now, however, established on the most ample experience, not only that an arm suffering from disease of the elbow-joint may be saved from amputation by cutting out the diseased bones, but that the limb thus preserved may retain its mobility and power so little altered or impaired as to be hardly distinguishable from one in its natural state. Excision of the elbow-joint, therefore, instead of being regarded as an experiment of such doubtful result that it had never been performed in Great Britain, is now the established rule of practice. Indeed the operation is deemed so safe in its performance, and so satisfactory in its effect, that even in cases where there is no existing disease, but merely stiffness of the joint in a straight or some other inconvenient position, it is undertaken in order to restore mobility and usefulness of the arm.

The morbid condition which requires excision of the articulating extremities of the bones occurs in adults most frequently from the disease which, to distinguish it from others, has been named ulceration of the cartilages; and, to prevent any cavil on the part of those gentlemen who view all questions of pathology through the microscope, and never seem so happy as in endeavouring to unsettle the ideas of practical men by altering the nomenclature of things familiar to them, I beg to explain that by this term I understand merely a particular form of disease, characterised by peculiar symptoms leading to certain changes of texture, and remediable by certain means of treatment. Leeches, blisters, soothing means, and all the ordinary forms of counter-irritation have been found productive of little more than palliation in cases of this kind; but the actual cautery, when employed before suppuration has taken place, may be relied upon with confidence as a nearly certain and effectual remedy. The ankle, knee, wrist, and elbow joints are most liable to the disease, especially the one last-mentioned, and here the beneficial effect of surgical interference may often be witnessed in circumstances of the most impressive character. Yet thirty years ago the actual cautery, so far as I know, had never been used in this country for the purpose of counter-irritation; and even now I suspect that it is chiefly confined to practitioners who have derived their education from Edinburgh.

In the more frequent forms of articular disease which depend upon scrofulous degeneration of the textures, a no less conspicuous improvement has been introduced, not through the employment, but from the disuse, of active measures. It is now admitted that in such cases depletion and counter-irritation are not only productive of no benefit but actually injurious, from the weakening effect which they produce on patients whose weak state of system is the source of de-

rangement, and should be the great object of reparation, all that the affected joint requires being protection from motion or other causes of disturbance.

In the treatment of aneurism, and more especially that of the femoral and popliteal arteries, a great addition has been made to the resources of surgery in the employment of pressure, not only with regard to the principles of its application but also in respect to the means by which this is accomplished. Indeed, I think it may be admitted, that recent times have afforded no greater boon to the members of our profession than they have done here to those who aspire only to mediocrity in the practice of their art. I think it cannot be doubted that the ligature, when properly applied, is the simplest, safest, and most effectual mode of remedy. But pressure is certainly preferable to an imperfectly performed operation; and as many of our brethren can accomplish the former who are scarcely equal to the latter, it is plain that the treatment of this disease may now be practised with safety much more generally than it was wont to be. In retention of urine depending upon stricture, it may not be possible for all the members of the medical profession to afford relief by the introduction of a catheter, but there are few of them who would experience any difficulty in thrusting a trocar from the rectum into the bladder, which, although attended with no small danger, is certainly preferable to persevering in attempts productive of no better results than making false passages, or leaving the patient to his fate. In the same way, if the question were to lie between exposing the femoral artery by scratching down to it with blunt instruments, such as copper or silver knives, and employing pressure, there should be no hesitation in preferring the latter method for the remedy of aneurisms admitting of its application.

The loose cartilaginous bodies which are occasionally met with in the joints, and especially that of the knee, were formerly regarded as most unpleasant subjects of practice, from the urgency of the symptoms for relief, and the danger attending the employment of effectual means of remedy by incision and extraction. In a large proportion of cases—perhaps a third, or even half—inflammation of the joints resulted from this operation, and resisting all measures of counteraction, proceeded to the destruction of the limb or life. It was hoped that a subcutaneous opening of the synovial membrane, so as to allow displacement of the moveable body, might accomplish the object more safely; but experience has taught us that, although in some cases practicable, it is generally impossible thus to effect removal from the articular cavity, without causing such disturbance of the textures as to incur the risk of bad consequences. In these circumstances, it is satisfactory to know that complete and permanent relief may in most cases, if not always, be afforded, by bringing the cartilage into a part of the joint where it can be retained by external pressure, and where the coverings are thin; then mak-

ing a free subcutaneous incision through the synovial membrane and the cartilage, and finally applying a small blister over the part. By this process the body becomes fixed, so as no longer to cause annoyance, and soon suffers absorption, partial or complete.

The bursa which accompanies the flexor tendons of the fingers from the wrist into the palm of the hand, is liable to morbid thickening and distension by fluid, together with solid bodies resulting from effusion into the cavity. As this condition renders the fingers nearly quite useless, its remedy is an object of no small consequence, but was formerly deemed almost impracticable, from the means employed proving ineffectual and dangerous. It is now ascertained that the disease may be cured easily and certainly by making an incision, about an inch or little more in length, through the integuments and subjacent textures, including the annular ligament of the wrist, on the integrity of which the peculiar obstinacy of the case appears to depend.

The great toe is liable to two morbid states which are productive of extreme inconvenience, and at no very distant date were reputed irremediable by any means short of amputation. In one of these the corner of the nail causes irritation and ulceration of the skin so as to render walking painful, and wearing a shoe almost impossible. A great variety of means were formerly employed, with very little advantage, for the palliation of this troublesome complaint, but have been completely replaced by the simple expedient of removing a portion of the nail, by dividing it longitudinally from the free margin quite up to and through the soft texture from which it grows. This procedure not only affords immediate relief, but, when properly executed, effectually protects the patient from any further uneasiness.

The other ailment to which I have alluded is an exostosis of the distal phalanx which projects the nail, and renders any degree of pressure on it intolerable. The contents of museums will show that amputation was formerly practised for the remedy of this condition; but it has been long ascertained that simply removing the growth is quite sufficient for the purpose.

The treatment of deformities proceeding from a contracted state of the muscles, more particularly wry-neck and the different forms of club-foot, has been greatly facilitated, or rather rendered easily practicable, instead of being nearly impossible, by the employment of subcutaneous incision for dividing the tendons concerned. Two-and-twenty years only have elapsed since the first operation of this kind in Great Britain was performed on the sternal attachment of the sterno-mastoid, and now tenotomy is almost the daily duty of a surgical practitioner—with what advantage can hardly be imagined by any one who cannot recollect the tedious and unsatisfactory attempts that were made in former times to accomplish the object by the unaided effect of mechanical support. It must be admitted, however, that the introduction of this system has led to a most serious

evil in developing a new set of specialists, under the title of Orthopædists, who are doing an incalculable amount of mischief to the public and the profession by reviving and carrying to an unprecedented extent the use of apparatus in cases where it can never be otherwise than either hurtful or productive of no benefit. It is indeed surprising that the apathy of medical men should tolerate the practices of these persons, especially in regard to lateral curvature of the spine; and I can account for their silent endurance only by attributing it to the slavish fear of running counter to popular prejudice, and incurring the enmity of those who thrive on the delusions which spring from this source.

The bad effect of withdrawing a particular operation, or the treatment of a particular disease, from the general practice of surgery, is well illustrated by what has happened in regard to the cure of squinting, by division of the muscles which cause this deformity. The public was greatly delighted with the prospect of obtaining relief from an unseemly and inconvenient condition, which had baffled all previous attempts to effect its remedy, and readily submitted their eyes to any who undertook the reparation of their obliquity on the new system. From the lowest level and thickest obscurity, through the united influence of impudence and credulity, a host of squint cutters started into notice; and although they might never have had occasion before to use the knife or scissors on a living human body, freely practised on the patients who crowded round them. But the process required being a surgical operation of great nicety, and entirely useless unless perfectly complete, it is not surprising that in such hands failure was more frequent than success; and the former result, instead of being attributed to its true source, was accounted for by alleging that the relief was not always permanent, and that relapse could not be prevented. The operation, therefore, got a bad name; and although deserving the greatest confidence, is even now still labouring under the obloquy incurred by its self-constituted and unworthy professors.

Both the upper and lower jaws are liable to morbid growths, of the same nature as those which occur in other parts of the osseous system, and have long been remedied by amputation. Sometimes they are soft and cerebriform, at others of a firm fibrous or fibro-cartilaginous consistence. In both cases removal was formerly attempted by laying open the cheek, exposing the tumour, and digging it out so far as possible by scissors or chisels, and finally applying caustics or cauteries to destroy any portion that might remain. But such proceedings, however bloody, severe, protracted, and dangerous, hardly if ever accomplished the object in view; and so far from even prolonging the patient's existence, generally shortened it, under circumstances peculiarly revolting and distressing. In one of the cases which I recollect of seeing thus treated, the actual cautery was applied forty times, and the daily dressing, with its stench and maggots, was a scene not soon to be forgotten. About thirty years ago,

a better plan of affording complete extirpation was introduced, upon the principle of ascertaining the limits of the growth, and then dividing the bone beyond the disease, where it was known to be sound. This principle was applied first to the lower, and then to the upper jaw, with the most distinguished success and advantage, tumours of the most formidable character being removed from both situations easily, safely, with little deformity, and with permanent relief. Like other good measures, this operation has been abused, by employment in cases either not requiring or not admitting of its beneficial performance, as when the tumour depended upon a glandular enlargement adherent to the bone, or upon growths proceeding from the bones of the skull, and simulating enlargements confined to the maxillary region. But the untoward results of such unwarrantable doings should not be allowed to lessen the credit of what ought to be regarded as one of the greatest improvements of modern surgery.

Both the superior and inferior maxillary bones are liable to the formation of serous cysts, which originate within the osseous substances, and so expand it as to resemble solid growths through the firmness of their walls, which, however, are always, in part of their extent, so yielding to pressure, that a careful examination is sufficient for detecting the nature of the case. These cysts were formerly thought to require very severe treatment, and at least laying open throughout the whole of their extent, together with the application of caustic, or the actual cautery, to destroy their secreting action. But now it is well known that such strong measures are quite unnecessary, the only essential requisite for contraction and obliteration of the cavity being an ample aperture so situated as to allow free vent to its contents, and that for this purpose it is sufficient to remove an elliptical portion of the parietes, to prevent the speedy closure which would result from a simple puncture or incision.

Suppuration of the maxillary antrum also is now treated much more satisfactorily than it was wont to be,—since it has been ascertained, that although a small opening may suffice for palliation, a free one is essential for complete relief. The old practice, therefore, of drilling a hole through the socket of a tooth, and throwing up injections by this narrow channel, in the thick alveolar ridge, to wash out the cavity, has been superseded by the much easier and far more effectual plan of lifting up the lip, and cutting parallel with the gum, above the biscuspid teeth, so as to make a free opening into the antrum, where its anterior wall is thinnest. If this be properly done, no further treatment is required; and the lesson thus afforded is useful in regard to the healing of all sinuses, whether of the hard or soft parts of the body, by showing the importance of effectual drainage, which will always be found preferable to injections and other expedients of an unsound pathology.

The tumours of the nasal cavity, which are comprehended under the title of polypus, were formerly supposed to be of only two kinds—the simple or mucous, and the malignant, so that the peculiar sort

of growth, characterised by its firm texture and hemorrhagic disposition, which is now known by the name of fibrous, not being distinguished from the others of a bleeding nature, was along with them deemed irremediable. But we now know that of all polypous tumours, this is the one which most requires and admits of relief, whether we regard the gravity of its symptoms, or the permanence of recovery after its removal. Indeed there are few cases of surgery in which the efficiency of its interference is more distinctly shown than when a patient oppressed by obstructed breathing, and exhausted by frequent hemorrhage, is instantly transferred, by a proper application of the forceps, to comfort and security.

In extracting the ordinary kind of polypus, there has also been a great improvement in the instrument employed, and the mode of procedure. Formerly the forceps were large, with windows or apertures in their blades, to increase the security of grasp, and the operator aimed at seizing the body of the tumour. But now these growths being known to originate from a small portion of the cavity, however large they may be, and whether they project forwards, or hang down into the pharynx, forceps of the smallest possible size consistent with the requisite degree of strength, are gently insinuated towards the root, which they separate by a combined movement of torsion and evulsion. The object in view is thus accomplished not only with infinitely less suffering, but far more effectually, than according to the old system; and the patient, instead of being tormented by a succession of sittings, as the abortive attempts were named, is at once freed from trouble. It is hardly necessary to add, that the old plan of noosing a polypus, by slipping over it loops of ligature or wire introduced into the nose, or through this cavity into the pharynx, has been entirely rejected from the rational practice of modern surgery, though it may still decorate the pages of those compilations for the use of students, which perhaps assist them to pass their examinations, but can never convey the sound principles of practice which they do not contain.

In restoration of the nose, and other important parts of the face, destroyed by disease or injury, two methods were formerly taught, although only one of them was, so far as I know, ever practised in this country. These were the Taliacotian and Indian operations, of which the former was effected by borrowing the requisite portion of integument from some other part of the body; while the latter accomplished the object by turning round a flap from the neighbouring skin so as to supply the deficiency. The procedure last mentioned is the one that was formerly always employed upon occasions of the kind in question, but has now been superseded by another, which, wherever practicable, seems greatly preferable. This, which may be called the plan of lateral displacement, supplies the defect by separating flaps of skin from their subjacent connections, and transferring them to the side where they are required, without any twisting of the connecting isthmus. Any one who has practised the

latter method, or witnessed the facility with which it is accomplished, will find difficulty in conceiving how the other could ever have been employed.

Enlargement of the tonsils occurs very frequently at an early period of life, and occasions great inconvenience by impeding respiration, especially during sleep, by rendering the voice husky, by causing a disposition to sore throat, and occasionally by producing a degree of deafness. Thirty years ago the practice of surgery in Great Britain did not afford any effectual remedy for this disease. The French method by excision had not been adopted; and what was called the English operation by ligature, though taught in the schools, was never attempted, at all events, more than once by the same practitioner. Removal by the knife was introduced in Edinburgh, and through the influence of gentlemen educated here, is now widely extended, notwithstanding a certain degree of discredit which it has acquired, from the proceedings of those specialists who profess to cure cases of deafness usually reputed incurable; and among other means for this purpose, are alleged to cut tonsils in nowise redundant, or in any respect warranting interference. The operation, when properly performed, in circumstances really requiring it, affords, with perfect safety, such an amount of speedy and permanent relief, as justly to merit the title of a substantial improvement in the practice of surgery.

Opening of the chest for evacuation of fluids accumulated in its cavity, was formerly accomplished by an incision in one of the intercostal spaces most convenient for the purpose, but with results so unsatisfactory, as to be regarded a very nearly, if not entirely, hopeless undertaking. In the first instance, there was considerable risk of fatal inflammation, and at a later period, if he escaped this danger, the patient was almost sure to be carried off by the exhaustion of continued discharge. Instead of this unpromising procedure, a great improvement has been introduced, by substituting for the knife a slender trocar, which is thrust directly through the parietes of the chest without any preliminary incision. The wound thus inflicted does no more harm than the puncture of a needle, and may be repeated without any fear of bad consequences, however often it may be requisite for the patient's relief. Thus upwards of 600 ounces of purulent fluid have been withdrawn with the effect of complete recovery, while in others one or two tappings have been sufficient for the purpose. It cannot be doubted, therefore, that here also the resources of surgery have been rendered more beneficial in recent times than they were previously.

The operation for hernia is now frequently performed without opening the sac; and although this modification of the procedure is unquestionably not applicable to all cases requiring relief, there can be no less doubt that, if judiciously employed, it tends greatly to lessen not only the difficulty of performance, but also the danger of bad consequences. There seems indeed to be some room for fear,

that the division of the constricting textures has thus been rendered so simple as to deprive the groin of that careful attention which in times past was devoted to it by students of surgical anatomy. But as nothing can steady the hand of an operator so effectually as knowledge of the structure concerned in his proceedings, I venture to hope that the rising generation of surgeons will not neglect the inguinal fasciæ, which it cost their fathers so much trouble to unravel. In the meantime, it must be admitted that here too an important step has been taken for advancing the relief of human suffering by the exercise of surgical art.

In the treatment of hydrocele, a great improvement has been introduced by the substitution of iodine for the port wine formerly employed, since the process of injection is thus rendered much more safe and certain than it was wont to be. Various mixtures and combinations are still employed for the purpose; but it appears to be fully established by experience, that from one to two drachms of the tincture injected into, or allowed to remain in, the *tunica vaginalis*, afford the easiest, least painful, and most effectual means of curing the disease. The various measures, therefore, of which the contending claims for confidence were formerly the constant subject of discussion, no longer embarrass the course of practice; and candidates for medical honours have ceased to select the treatment of hydrocele as the theme of their inaugural dissertations.

Fungus of the testicle, although removed from the category of malignant diseases, to which it was formerly referred, continued until lately to be deemed remediable only through removal by knife or caustic. It is now, however, well established, that the protrusion admits of recovery by inducing absorption of its interstitial effusion; and that for this purpose an easy and effectual method is to detach the scrotal integuments from their subjacent connections sufficiently for allowing them to unite over the tumour, and thus subject it to the influence of compression. In this way the object is speedily attained, and the portion of gland preserved may perhaps be still able for the performance of its function.

The extraction of urinary concretions from the bladder has been much improved, in the first place, by the introduction of lithotrity or stone-crushing; and, secondly, by the establishment of sound principles for the performance of lithotomy. When the stone is small it may be easily and safely crushed; but if it is of a size so large as to require frequent repetition of the process for the removal of fragments, there is a great risk of disagreeable consequences, both immediate and remote; and the cutting operation, therefore, not being superseded, should be rendered as perfect as possible. Formerly a mysterious uncertainty brooded over this subject; and surgeons who successfully achieved all the other difficulties of practice, failed signally in cutting for the stone; while others, notoriously deficient in the ordinary qualities, not only of head but also of hands, acquired the reputation of skilful lithotomists. When the latter were

asked to explain their mode of procedure, they gave the most conflicting accounts of it, some maintaining that "the less that is cut the greater the patient's safety," and protesting against more than notching the apex of the prostate, while others insisted upon the necessity of obtaining ample space by free incision of the vesical coats. But when the operation was conducted upon the first of these principles, it was almost certain to prove fatal from suppurative inflammation at the neck of the bladder; and when the second was taken for a guide, extravasation of urine no less regularly followed. It may be added, that operators who supported these different doctrines freely charged each other, no matter how successful, with not knowing the actual extent of their incisions, so that doubt and discomfiture were unavoidable by all except those fortunate individuals who happened to acquire the knack of overcoming the difficulty in question. But now the truth has been fully ascertained, and the reason of failure, as well as success, is correctly referred to its proper causes. The great source of difficulty and danger is found to be a particular texture, which lines the interior of the prostate at its base to the extent of about half an inch in thickness. This sort of ring admits of being stretched to a certain degree, but then resists further expansion with great force; and if torn by violence, is sure to excite a fatal inflammation of the neighbouring parts,—while, if it is divided by incision, no bad consequence results, and the other tissues at the neck of the bladder readily yield to any expanding force, so as to permit the extraction of even large stones easily and safely. The surgeon, therefore, after making an adequate opening through the subjacent fasciæ and muscles of the perineum, has merely to make sure of cutting this important part, which may be done most safely by a knife, guided on the finger, and then with the latter dilate the other textures sufficiently for his purpose. This procedure was formerly hardly practicable in adults, except by an operator possessing a long finger, but may now be always easily accomplished through the aid of chloroform, which prevents the straining and contraction of the *levator ani*, that in a conscious patient withdraws the neck of the bladder deeper into the pelvis.

In the treatment of stricture it has been found that all contractions of the urethra which yield to dilatation may be remedied most easily and safely by the simple bougie, and that those which resist the proper application of this means admit of no effectual relief, except by external incision upon a grooved director, conveyed through the narrow part of the canal. These points being duly established, will, it is hoped, prevent professional attention from suffering distraction by the conflicting claims of less effective measures, and, concentrating it upon what appear to be the simplest and safest, throw completely into the shade those objectionable precedents that would never have been tolerated except as forlorn hopes, in circumstances deemed otherwise entirely desperate. It may be

now confidently said, that if any man endures continued distress, or loses his life, from stricture, the blame is no longer imputable to the defective condition of surgery; and if the Marquis of Argenteuil had still to make his will, I hardly think he would found a prize of L.500 to be conferred at stated periods for practical improvements in the treatment of this disease.

Diseases of the rectum, from being much in the hands of specialists and irregular practitioners, have been slower in their progress towards improvement than most of the other departments of surgery. Indeed, until a comparatively recent period, they were regarded as a sort of incomprehensible mysteries, unworthy of professional attention as subjects of scientific inquiry. Thirty years ago, fistula-in-ano was thought to require division of the septum, between it and the bowel to the whole of its extent. An internal opening, if present, was supposed to be at the summit of the cavity, but, whether present or absent, was considered of secondary consequence. The surgeon, therefore, arming his finger with a horn gorget, thrust it up the rectum, and then inserted into the fistulous canal a sharp bistoury, having its point guarded by a pea or bit of wax, until it reached the extremity, when pressure was made upon the gorget, and both instruments were drawn out together. Profuse hemorrhage frequently followed this deep incision, and great care was requisite, for weeks or months, to prevent adhesion of the edges by the daily introduction of dressing between them. This operation often required to be repeated, and not seldom proved entirely abortive. Now we know that the fistula always takes its origin from an abscess in close connection with the mucous coat of the intestine, which, in the first instance, with few exceptions, opens outwardly, and after a time establishes a connection with the cavity of the bowel by a small aperture, seated very regularly at the distance of an inch, or inch and quarter, from the orifice of the anus. All that is necessary for the patient's relief is division of the septum between the internal and external openings, and the operation performed upon this principle is not only very easy, perfectly free from any risk of hemorrhage, and so superficial as absolutely not to require any dressing at all, but, what is of still more consequence, always proves successful.

Internal hemorrhoids, which occur so frequently, especially in the upper ranks of society, and occasion so much distress by causing protrusion, bleeding, or pain, or all of these effects together, were formerly subjected to a great variety of treatment — by excision, ligatures, caustic, and the actual cautery, with various and often unsatisfactory consequences. It now appears to have been long established, that of all these means the ligature, when properly applied, affords the relief desired with most ease to the patient, with the most permanent advantage, and with the greatest security from danger. It is true that no operations whatever can be con-

sidered absolutely free from risk; but I am not acquainted with any one of the minor kind which is so seldom followed by a bad result as the ligature of internal hemorrhoids.

Fissure of the anus is the source of such excessive pain and annoyance that the severe means formerly held to be essential for its remedy were occasionally practised, although both patient and practitioner generally shrank from the performance of so serious an operation for so trivial an ailment. Instead of a complete division of the sphincter, and a confinement of months to bed, it is now well ascertained that a small incision, slightly exceeding the length and depth of the little ulcerated fissure, is sufficient for its remedy, without any after treatment, or restriction from exercise, beyond a day or two of quiet.

Spasmodic stricture of the anus, which was formerly either regarded as incurable, or tortured by ineffectual attempts to afford relief by dilatation, is now remedied at once by division of the external sphincter, with an amount of comfort, and an immunity from suffering, that can hardly be conceived by any one who has not witnessed such cases.

The only organic stricture of the rectum which admits of remedy is now known to be situated within the distance of little more than two inches from the anus, and the preposterous proceedings to which credulous patients were formerly subjected by the rapacious empirics who pretended to cure contractions far up the bowel, or even in the sigmoid flexure and ascending colon, are no longer heard of, and if any man were now to wield a rectum bougie, two feet in length, he would do so at the risk of being deemed worthy of accommodation in Newgate or Bedlam. The amount of human misery which has been, or may be, saved by the alterations thus introduced into the treatment of diseases of the rectum, it would not be easy to calculate, but is certainly sufficient to warrant the conclusion, that in this department also surgery has made an advance of which it is not likely to retrace the steps.

In offering this summary of practical improvements, I have not attempted to apportion the share of credit due to their respective authors. Such a task would have been foreign to my present purpose, and will probably be better executed by those who come after us than if undertaken by a contemporary. The facts are all upon record, and, when personal considerations shall have ceased to exercise their influence, will, I have no doubt, lead to a just appreciation of the merit, wherever it may be found due.

In the meanwhile, enough has, I trust, been said to show that surgery during the last thirty years has not stood still, but, on the contrary, introduced into practice so many useful improvements, that the practitioners of this time have no reason to be ashamed of it, or to fear its comparison with any other period of similar extent in medical history.

ARTICLE III.—*Anatomical Considerations on the Mode of Action of Local Blood-Letting in Affections of the Internal Viscera.* By JOHN STRUTHERS, Fellow of the Royal College of Surgeons, Lecturer on Anatomy, Edinburgh.

(*Read before the Edinburgh Medico-Chirurgical Society, February* 17, 1853.)

MY object in the following observations is, to inquire into the mode in which local blood-letting can be of service in affections of the internal viscera.

The practice generally followed in this country is, to apply leeches or to cup over the affected part, whether in the trunk or limbs; and it will be generally admitted that this is done with the view of affording special relief to the affected part, by the more or less direct withdrawal of blood from its distended blood-vessels,—that is to say, in following this practice it is generally believed that the blood-letting is a *local*, and not a general, one.

Before proceeding with this inquiry, I shall assume two points as the groundwork of my argument. 1. That the relief in local blood-letting is through the blood-vessels or vascular system, and not through nervous agency; and 2. That, as blood can come from one part to another only by blood-vessels, there can be no special relief by local blood-letting, unless the blood-vessels of the one communicate or anastomose with the blood-vessels of the other.

It is evident, therefore, that this inquiry must be founded on a careful consideration of the arrangement of the blood-vessels of these parts.

In following out these principles in the endeavour to discover the *modus operandi* of local blood-letting, we notice that there is a wide difference between the local blood-lettings practised by the surgeon in affections of the limbs and external parts, and those by the physician in affections of the internal viscera. In the former parts, the blood-vessels communicate more or less directly from the surface inwards to the affected part, and we can therefore easily understand how leeches, applied, for instance, over the knee or ankle, can afford direct relief to the distended vessels in or about these joints. But in the latter parts there are no such communications. The internal viscera of the chest and abdomen on the one hand, and the walls which enclose them on the other, are supplied with blood from entirely different sources, and the two sets of blood-vessels do not communicate with each other except through the general circulation, or, what is the same thing, through the parent trunk; and it would therefore appear that we cannot specially relieve these internal viscera, or any one of them, by local blood-letting.

I shall now demonstrate this by referring briefly to the sources of vascular supply to the viscera and walls of the abdominal and thoracic and cranial cavities.

1. *The Abdomen.*—Here the viscera are supplied from behind, and solely by large arterial trunks, which spring from the front and sides of the aorta. The spleen, liver, pancreas, stomach, and duodenum, are supplied by the cœliac axis, the rest of the small intestines, and first half of the great, by the superior mesenteric artery, and the remainder of the great bowel by the inferior mesenteric, down near to the anus. The kidneys receive their blood entirely by the corresponding renal artery, direct from the aorta, and return it direct to the vena cava by the renal vein, whilst the blood returned from the other organs above named is again distributed to the liver by the vena portæ before it is returned to the vena cava.

Thus, to generalise, all the viscera of the abdomen receive their blood from behind direct from the aorta, and return it, directly or indirectly, to the vena cava,—an abdominal circulation by itself from the aorta to the vena cava, and that without any communication with the wall of the cavity, except indirectly through the parent trunks, the aorta and vena cava.

To this, one exception must be noticed,—viz., the anastomosis along the rectum and down to the anus, by which the vessels of the perineum communicate freely with branches of the superior hemorrhoidal artery and vein; the artery communicating freely with the arteries along the intestine in the abdomen, whilst the vein communicates with the other pelvic veins, and, passing up, joins the splenic vein, whence its blood is conveyed to the liver. This arrangement has led to the French practice of applying leeches to the perineum to relieve the abdominal viscera, and, so far as anatomy is concerned, there are good grounds for its adoption, as indeed the only means by which a local blood-letting for the alimentary part of the abdominal viscera can be practised; but the further discussion of this point does not form part of the present inquiry.

The *wall* of the abdomen is supplied from altogether different sources, from above, from below, and from behind. Behind, from the aorta come the lumbar, and lower intercostal, and diaphragmatic arteries. Below, from the internal iliac comes the ilio-lumbar branch, from the external iliac close to the groin, the circumflex iliac, and deep epigastric; and from the common femoral the superficial epigastric, from which the skin of the front of the abdomen is chiefly supplied. Lastly, from above there is the end of the internal mammary branch of the subclavian.

All of these arteries are accompanied by veins which return the blood to the venous trunks, corresponding to the arterial trunks, from which the branches are supplied. Thus the abdominal wall is supplied with blood from the subclavian and iliac arteries, and from the aortic trunk itself; but these branches from the aortic trunk in no way communicate with the visceral branches from the same trunk. It is evident, therefore, that any increased flow of blood along these parietal arteries cannot act on one visceral artery more than on another, nor indeed on any of these visceral branches, ex-

cept simply by a diminution of the general current within the aorta.

The kidney is perhaps the best example with which to illustrate this argument. Lying in its bed of fat and cellular tissue, it has no vascular connections but by its artery from the aorta and its vein to the vena cava. It can, therefore, be reached and acted on through the renal artery and vein only, and these only through the aorta or vena cava circulating the general current, and any diminution of this general current could not tell on the current to or from the kidney more than on that to the stomach or the lower extremity.

There cannot, therefore, be a local blood-letting for the kidney; it can be influenced only through the aorta or vena cava—that is, through the general circulation.

The same remark applies to the other abdominal viscera already named, and we are, therefore, shut up to the conclusion, that blood-letting on the abdominal wall acts as a general, and not as a local blood-letting, as it must not only fail to act on any particular organ, but it does not appear how it can even relieve the circulation within the abdomen more than that of the hand or foot.

2. *The Chest.*—The same line of argument is applicable to the chest. The wall and viscera are supplied from different sources, and the blood-vessels of the two do not communicate in their distribution.

The *heart* it supplied solely by the two coronary arteries, which arise from the aorta immediately above its valves. We can, therefore, have no special command over the supply of blood to the substance of the heart.

The *lungs* are entirely separated from the wall by the pleural cavities, being attached only by their root, through which the blood-vessels pass. The lung has two sets of blood-vessels, the large pulmonary and the small bronchial arteries and veins. The former we assume to be concerned chiefly in the conditions referred to as congestion of the lung and pneumonia, whilst the distribution of the bronchial arteries renders the inference a fair one, that they are the vessels concerned in affections of the bronchial membrane,—that is, the one set of vessels appear to be chiefly concerned in pneumonia, and the other in bronchitis.

Now, it is evident that blood taken from the wall of the chest cannot drain or derivate from the pulmonary system of vessels, but can influence them only by acting through the general circulation, and that by lessening the quantity of blood to be returned to the right side of the heart and thence sent to the lungs. But it may be said, that the pulmonary vessels may be relieved by means of their anastomosis with the bronchial vessels. As to the nature or even existence of such communications between the two sets of vessels of the lung, anatomists are not quite agreed, although the result of minute injections has led most to believe that the deeper set of the

bronchial vessels, ramifying on the smaller bronchial tubes, do communicate with the pulmonary capillaries or veins. Very little weight can be attached to an explanation depending on this obscure anatomical point; but supposing, for the sake of argument, that the pulmonary veins could be drained to a slight extent by drawing blood from the bronchial veins, this again could be of no service unless these veins themselves can be locally acted on from without. Is it, then, in our power to relieve the bronchial vessels by local blood-letting? The arteries are usually three in number,—two on the left side, which spring from the thoracic aorta, and usually one for the right side, which may arise either from the aorta, or in common with or from the first aortic intercostal artery of this side. In such an argument it is scarcely worth while to mention the occasional varieties of these arteries in their origin, nor probably would physicians be disposed to explain any supposed benefit from local blood-letting by referring to the occasional or frequent origin of the right bronchial artery from one of the arteries going to the wall. The bronchial arteries, then, must be regarded as deriving their blood direct from the aorta, so that they could not be relieved either by draining or derivation in local blood-letting to the wall, this serving merely to draw off so much more blood from the general current of the aorta, exactly as in the case of the abdominal aorta in leeching the abdominal wall. Nor could local blood-letting to the side or back relieve the bronchial veins. These end on the right side in the vena azygos, on the left in the left superior intercostal vein, on its way to terminate in the left innominate or in the lesser azygos vein. Now these same azygos and superior intercostal veins receive the blood returned from the side and back of the chest by the ordinary intercostal veins. But, in order to relieve the bronchial veins through the intercostals, the current in the latter would require to be reversed, which local blood-letting could scarcely produce, even although the intercostal veins were not, as they are, provided with valves.

The *wall* of the chest is supplied from various sources, chiefly by the aortic intercostal arteries, which spring separately from the back part of the aorta. The other arteries come chiefly from the subclavian. The superior intercostal supplies the two higher intercostal spaces; the internal mammary the anterior part of the thoracic wall, coursing down behind the cartilages of the ribs; the posterior scapular partly supplies the surface of the upper part of the back, where, however, the chief vessels come from the posterior divisions of the aortic intercostals; and lastly, the axillary aspect of the chest and the pectoral region are supplied from the outside by the thoracic branches of the axillary artery; and all of these arteries are accompanied by corresponding veins.

When, therefore, blood is drawn from any part of the thoracic wall, it comes either from the subclavian or axillary arteries, on the one hand, or, on the other, the thoracic intercostal arteries, the latter

springing from the back part of the aorta, so that any increased flow by them would simply diminish the general volume and current within that vessel.

We see, therefore, that the vessels of the lungs can be relieved through the general circulation only, either, on the one hand, by a little derivation from the general current in the aorta, but which will not relieve the lungs more than the lower extremities; or, on the other, by lessening the quantity of blood returned by the veins to the right side of the heart.

I think, then, it has now been sufficiently shown that local blood-letting on the chest cannot act on the heart and lungs except as a general blood-letting, and that there is, therefore, no such thing, in the proper sense of the term, as local blood-letting for these organs.

To render complete the demonstration of the absence of communication between the blood-vessels of the walls and those of the viscera of the chest and abdomen, it may be deemed necessary to take notice of the connection between the viscera and the wall by means of the reflections of the serous membranes, as it might be supposed that by these a means of affording local vascular relief is established.

In the abdomen the peritoneum is reflected from the wall on to the viscera at various places. On each side it is reflected on to the ascending and descending colon, so as to leave their posterior third or fourth uncovered and in contact with the posterior wall. But it is not the conductor of vessels between the wall and the colon. It possesses its own nutrient vessels only, and the vessels of the colon are derived from the colic branches of the mesenteric arteries which do not anastomose with the vessels of the wall, as they run outwards behind the peritoneum to reach the intestine.

The liver is connected to the diaphragm by means of its suspensory and posterior ligaments, each consisting of a double reflection of peritoneum; and these contain minute blood-vessels, derived from the internal mammary and diaphragmatic arteries. But these blood-vessels are very minute, and their distribution is very limited, supplying at most but a small region of the coats of the liver where it is joined by its serous ligaments; and I am not aware that any connection has ever been demonstrated by injection between these minute vessels and the proper internal blood-vessels of the viscus. Besides, these vessels come from the diaphragm only, the vessels of which again are but very remotely connected with those of the skin of the trunk; and, even supposing that these minute vessels did join with those of the liver, physicians will probably not be disposed to look to them as affording an explanation of the relief from local blood-letting, any more than to the additional fact, that often one, or occasionally both, of the diaphragmatic arteries may arise from the cœliac axis, instead of separately from the aorta; or to attribute the benefit derived from leeching the epigastrium, in a supposed affection of the stomach, to any minute anastomosis that might obtain between the vessels of the diaphragm and œsophagus, as the latter

is passing through its aperture in the former, an inch previous to its termination in the stomach.

In the chest the pleuræ are reflected from the wall to the viscera in the formation of the mediastina. On the lung the pleura is supplied as part of the lung, and on the wall as part of the wall; and it might be supposed that relief might be conducted from one to the other along these reflections. But I am not aware that any part of the pleuræ is occupied by conducting vessels, or supplied with more than it requires for its own nourishment, these being derived from the vessels of the part on which the membrane is lying. The reflection forming the posterior mediastinum is the shorter of the two. It is supplied with nutrient arteries from the thoracic aorta. The pleura forming the anterior mediastinum is supplied from the mammary arteries. It then passes on to the pericardium, and behind this it joins the pulmonic pleura by a narrow union only, as it envelops the root of the lung. But there are here no conducting vessels that I know of; none beyond those which enter the membrane to nourish it; and even if taking blood from the front of the chest did, by derivation, cause a determination along the pleura of the anterior mediastinum, the relief would be to the pericardium.

The same view applies to the reflection of the serous pericardium on to the great vessels near the heart; and again to the reflections of the arachnoid membrane, near the free margins of the falx cerebri and tentorium cerebelli, around the cerebral veins as they pass across to the longitudinal sinus, and around the nerves at the base.

The reflections, therefore, of the serous membranes cannot be regarded as establishing a medium through which the blood-vessels of the viscera can be relieved.

3. *The Head.*—The question here is, can the vessels of the brain be specially relieved by local blood-letting on the head?

We observe that here also there is a serous cavity between the wall and the contained viscus. The parts internal to this cavity are supplied by the internal carotid and vertebral arteries; whilst those outside, viz. the dura mater, the skull, and the external parts, are supplied by the external carotid. Now the arteries of the brain and pia mater do not communicate with those outside the arachnoid cavity, but it is different with the veins. I do not here refer to any minute anastomosis between the periosteum on the outside, and the dura mater, or internal periosteum, on the inside, whilst the latter at the same time lodges the returning veins from the brain; nor to the venous canals in the diploe, for these chiefly open internally; but I refer to the direct communications, by means of what we term the emissary veins, which occupy the parietal and mastoid foramina. By these veins the longitudinal and lateral sinuses may directly empty themselves into the veins of the scalp. This is more especially the case with the mastoid communication, which may be looked upon as a kind of safety opening by which the lateral sinus

may be relieved; and this sinus, it will be recollected, receives the entire blood from the brain, and also from the orbit.

There is another communication between the veins of the scalp and the cranial sinuses, by means of the ophthalmic vein, which externally communicates freely with the veins of the eyebrow and temple, and internally joins the cavernous sinus; but to afford internal relief by this communication, the natural direction of the current in the ophthalmic vein would require to be reversed, which probably could not be effected by anything less than a free opening in the angular vein.

It would appear, then, from anatomy, that, by leeching the scalp, especially behind the ear, the sinuses of the dura mater and veins of the brain may be relieved, and that as directly, though not so rapidly, as by the method of opening the external jugular vein at the root of the neck.

The common practice of cupping on the back of the neck, with the view of specially relieving the brain or head, is no doubt generally, or in great part, efficacious, simply as a slow or mild general blood-letting; but is at the same time not without anatomical recommendation as a local blood-letting. The mastoid communication from the lateral sinuses is to the occipital veins, and these communicate freely with the veins of the back of the neck; and also the arterial supply to the back of the neck, derived from the deep cervical and occipital arteries, has free communications with the posterior muscular twigs which the vertebral arteries give off during their ascent to the brain.

I do not, therefore, propose to include the brain amongst the internal viscera which cannot be specially relieved by local blood-letting, although at the same time it is not necessarily so from the mere fact of its lying inside opposite to where the treatment is applied externally. Besides, we must recollect that here also there is the uncertainty of diagnosis in many cases, so that the affection we are treating may be situated either partially or entirely outside the arachnoid cavity; and even did no communication whatever exist between the blood-vessels of the internal and external parts here, so that no relief could take place by direct draining, it by no means follows that the practice of applying leeches to the scalp may not afford special relief to the brain, on the principle of derivation, or lessening the arterial currents to the organ within.

Before leaving this part of the subject, it is well to remark, that in these considerations I have not overlooked the question, as to the non-variation of the quantity of blood, or at least of fluid, within the cranium. It would be out of place for me to enter on this question here, and I shall, therefore, merely remark, that, whilst I am one of those who cannot understand how the quantity of fluid within the cranium can vary, until it can be proved that the brain can be compressed, *i. e.* diminished in bulk, by any force that the heart is capable

of exerting, I at the same time hold, that we ought not to allow this question to influence our practice. Whichever view be correct, we have still the same principles to contend with in practice; we have in both increased or diminished *pressure*, and in both increased or diminished *rapidity* of the circulation; and it therefore appears to me that those are in error who suppose that this question should influence our practice or our reasoning thereon.

It appears, then, that local blood-letting cannot affect the viscera in the abdomen or chest, except through the general circulation; and that, therefore, these blood-lettings are general and not local, according to the ordinary sense in which the term local blood-letting is employed.

But, although it be granted that leeching the wall cannot draw blood directly from the affected viscus, some may be disposed to hold that it may still afford to it special relief by what is called revulsion or *derivation*, this being the principle, or supposed principle, which guides those continental practitioners who apply the leeches not over or close to any affected part, but at a distance from it. By these terms they mean to express the view, that the result of local abstraction is to draw more blood to the bleeding part, and thus diminish the supply elsewhere.

Without meaning to dispute the correctness of this view within certain limits, it may be remarked, on the *modus operandi* of all local blood-lettings, that they may act in two ways. 1. By the direct abstraction of blood through the anastomosing vessels between the affected and the bleeding part, and it matters not whether this be by the veins or the communicating arteries; this we may call *draining*. And, 2. By lessening the quantity *going to* the affected part, by creating an increased flow or determination elsewhere. This is the revulsion or derivation of authors, and we may call it *counter-draining*.

Now it is evident that to be of local and special benefit, this practice must not be applied too far from the affected part; the farther it is the more it will lose its effect, and degenerate, so to speak, into a general blood-letting. It is probable that in most true local blood-lettings there is relief in both of these ways; but can it be so in the case of the abdominal and thoracic viscera?

That there can be no draining I have already shown; and it appears to me, that neither can there be any derivation or counter-draining from these viscera. The blood taken from the abdominal and thoracic walls comes from the iliac and femoral, or subclavian and axillary arteries, or from the back part of the aorta. Now this cannot lessen the circulation into the visceral arteries more than into those of the leg or arm. Nor can the effect which it has on the general current within the aorta bear more upon one viscus than another.

It must therefore be held, that the abstraction of blood from the

wall of the abdomen or chest cannot in any way, or according to any principle, be regarded, as far as specially influencing the contained viscera or any one of them is concerned, in any other light than as a general blood-letting.

I next proceed to inquire whether these considerations should lead us to alter the common practice of local blood-letting in affections of the internal viscera?

Now, notwithstanding what has been said, it is nevertheless not difficult to show, that the prudent and proper practice still is to apply the treatment over the affected part. This is because we cannot be sure of the exact situation and limits of the disease. There is pain as if in the liver. Is the affection one of the liver itself, or of one of the serous layers of the diaphragm or of the parietal peritoneum? There is pain as if in the kidney. Is it in the kidney, or in some part of the wall, or in both? We cannot be sure, and therefore we must treat the affection as a regional one—as an affection of, or in, the right hypochondriac region in the one case, and of the lumbar region in the other. If the viscus alone be affected, the blood drawn from over it will at least relieve it *as much* as a similar quantity taken from some other part would do; and, if the wall also should be affected, or be the entire seat of the disease, the patient, without running the risk of missing a better practice, has had the benefit of the doubt.

The same remarks apply to the chest. Local bloodletting must have the same direct effect in pleuritis that it has in peritonitis, the membrane in either case forming part of the wall; and can the practitioner be sure that the parietal pleura is not involved as well as the lung? The pericardium, again, receives part of its vascular supply from the internal mammary arteries, and it is therefore in our power to derivate from this part by leeching the front of the chest.

We must therefore come to the conclusion, that, in treating affections of the viscera of the abdomen and chest, it is best to take the blood from over the painful or affected part, grounding the localisation of our practice, not on the vague belief that we can thereby specially relieve the viscus, but on the uncertainty of diagnosis, which leaves us unable to say whether the wall also is affected or not.

But whilst this may be laid down as the general rule, which may be safely followed by all in all cases, it is at the same time to be remarked, that cases will frequently occur in which it is the object of the practitioner simply to lessen the quantity of blood in a congested or over-active viscus; and after he has duly exercised his judgment in the matter, I think enough has been said to prove that *then* he may cup or leech on any part he chooses, or instead, take an equal quantity from the arm, or from a vein in some other situation, resting assured that it is only on the principle of a general bloodletting that these internal viscera can be acted upon.

It may be urged that experiments of this kind are not warrantable;—first, Because, on my own showing, the blood-letting from over the painful part will prove at least equally beneficial with one from any other part, whilst the rule is simple as a general guide to the practitioner, besides being more likely to ensure the confidence of the patient; and, second, Because experience has led to the adoption of the local practice. To this it may be answered, that experience has by no means proved the superiority of the local over the general bloodletting in affections of the internal viscera. We can readily understand how experience may have established the superiority of the local blood-letting in affections of the chest and abdomen generally, because we know that in a large number of them the pleura and peritoneum are involved to our certain knowledge; whilst in those cases where we with equal certainty know the viscera to be affected, we do not know but that the affection may also in part involve the wall. But experience has not yet declared that two or four ounces of blood taken from over the kidney, or liver, or lung, will afford more relief to these viscera than two or four ounces taken from the hand or foot, or by venesection from the bend of the arm. All that experience can be held to have declared is, that a certain quantity of blood taken there is productive of a certain amount of benefit; but experience has yet to declare, whether similar benefit may not be derived by drawing the same quantity of blood from some other part.

The advantages that would attend this alteration in practice in such cases, would be the substitution of a more simple and less expensive procedure, as by taking the blood from a small opening in some vein, as, for instance, on the back of the hand or foot; or, if leeching or cupping be still followed, it could be done without the lengthened exposure of the chest and abdomen, during which the patient is exposed to the influence of cold.

I say, therefore, with reference to the abdominal and thoracic viscera, in a case in which the practitioner feels assured that the affection is *limited to the organ*, it would appear that it matters not where or how he bleeds, according to circumstances he may take his choice both of the part and the mode, feeling assured, that from whatever part he takes the blood, whether by the leech, the cupping-glass, or the lancet, he will reach the affected organ only by having withdrawn so much blood from the general circulation.

The same line of argument, it is evident, may be pursued with regard to the other parts of local treatment in affections of the chest and abdomen, more especially to counter-irritation, only, however, in as far as this and other remedial means have their action through the blood-vessels and the circulation. But the farther discussion of this matter would lead me beyond my purpose, which was simply to demonstrate from anatomy the conclusions which I have already drawn.

Fig. 1.

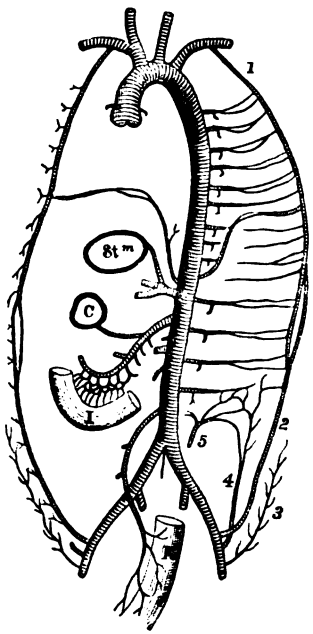


Fig. 2.

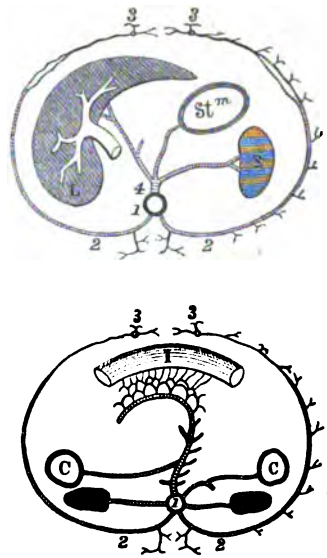


Fig 3

Fig. 1.—Longitudinal plan of the arteries of the trunk. The left side shows both the longitudinal and transverse anastomosing arteries of the wall; the right side only the longitudinal and diaphragmatic anastomoses belonging to the wall, and the various visceral arteries springing from the aorta.

Left Side.—1, Internal mammary; and 2, deep epigastric; connected behind to aorta by series of intercostal, lumbar, and diaphragmatic arteries. 3, Superficial epigastric. 4, Circumflex iliac. 5, Ilio-lumbar from internal iliac.

Right Side.—Branches of abdominal aorta, from above downwards, diaphragmatic, cœliac axis, superior mesenteric, right supra-renal and renal, right spermatic or ovarian, and inferior mesenteric.

Fig. 2.—Transverse plan of the arteries of the abdomen opposite to liver, spleen, and stomach. 1, Aorta, giving off from its back part the arteries (2, 2) for the wall, which unite in front with branches of the internal mammary arteries (3, 3). 4, Cœliac axis, coming from front of aorta, and supplying spleen, stomach, and liver, the latter also receiving the vena portæ.

Fig. 3.—The same, lower down, showing portion of small intestine and sections of ascending and descending colon and of kidneys. 1, Aorta, giving off behind the lumbar arteries (2, 2), which join in front with branches of the deep epigastric (3, 3). In front, as if coming from the aorta at the same part, are shown the superior and inferior mesenteric, and at the sides the renal arteries. Fig. 1 shows the order in which these visceral arteries come off at different stages of the aorta, and their relative size.

The anatomist will notice that it is chiefly the anastomosing branches of the arteries of the wall which are shown, and also that these are represented proportionally larger than natural.

ARTICLE IV.—*The Theory of Menstruation in early Pregnancy, Superfœtation, and the Site of Insertion of the Ovum.* By J. MATTHEWS DUNCAN, A.M., M.D., F.R.C.P.E., Lecturer on Midwifery, Edinburgh, etc., etc.

SINCE the time of the Hunters, physiologists have taught that one of the earliest results of successful impregnation, is the formation or deposition of a new membrane in the cavity of the uterus. This membrane, the caduca of Harvey, and to which William Hunter gave the name of decidua, consists of two chief parts, entitled respectively decidua vera and decidua reflexa; the former lining the cavity of the uterus internally, and at the edge of the placenta uniting with the latter, which covers the chorion externally. The reflexa has generally been described as produced by the intrusion of the ovum from the fallopian tube upon the vera, which had previously been deposited over the whole inner surface of the uterus, closing up the tubes.

Such were the generally received opinions for a long time after the Hunters. But the numerous varieties of statement and hypothesis on the part of authors, might have been sufficient to show that they were not well-founded, for scarcely a single point can be discovered in the history of these parts upon which authors are agreed. For instance, this membrane has been successively described as being vascular and not vascular; as having no openings, or three openings, or only one corresponding to the cervix; as being simple or composed of two, three, or more layers; as being an empty sac or containing a fluid, called by Breschet, hydroperione.

John Hunter described this membrane as resembling a layer of coagulable lymph, and originated several other like fanciful and erroneous notions in regard to it. These have frequently been attributed also to his brother William, and this error has been confirmed by the comments of the nephew of the latter, Matthew Baillie, who undertook the office of his posthumous editor. On examining the writings, etc., of William Hunter in regard to the decidua, it becomes evident that he had difficulties in regard to it which he did not neglect to state. But all that he does say is correct in every particular, although far from being so explicit or full as might have been, or as he himself evidently desiderated. The remarks of Dr Rigby[1] on this subject I have been able to confirm by the perusal of a volume of MS. notes of W. Hunter's lectures preserved in the library of the Royal College of Physicians; and the examination of plate 34th in his great work,[2] affords the amplest evidence to the same effect.

[1] See Dr Rigby's edition of "An Anatomical Description of the Human Gravid Uterus," etc., by W. Hunter, M.D., etc., p. 48.

[2] Plates of the Anatomy of the Human Gravid Uterus.

We find him always describing the decidua as the inner layer of the uterus, as having three openings, corresponding to the tubes and os uteri, which are still pervious after the descent of the ovum (*vide* fig. 7, tab. xxxiv. of his plates) as having an abundant vascularity, and its inner surface perforated like Brussels lace. In accordance also with the plate referred to, he describes the ovum as being "not in the cavity of the uterus, but only between the membranes."[1] Unfortunately these correct descriptions of W. Hunter were neglected, and supplanted by the fanciful doctrines that have been everywhere received. And when we consider the names which have lent authority to these views, as well as the rarity of opportunities of examining the parts in question *in situ* in healthy subjects; and, further, that most observations of the kind have been made on abortions, or, in other words, on abnormal productions, we shall not be astonished at their long maintaining a position in science.

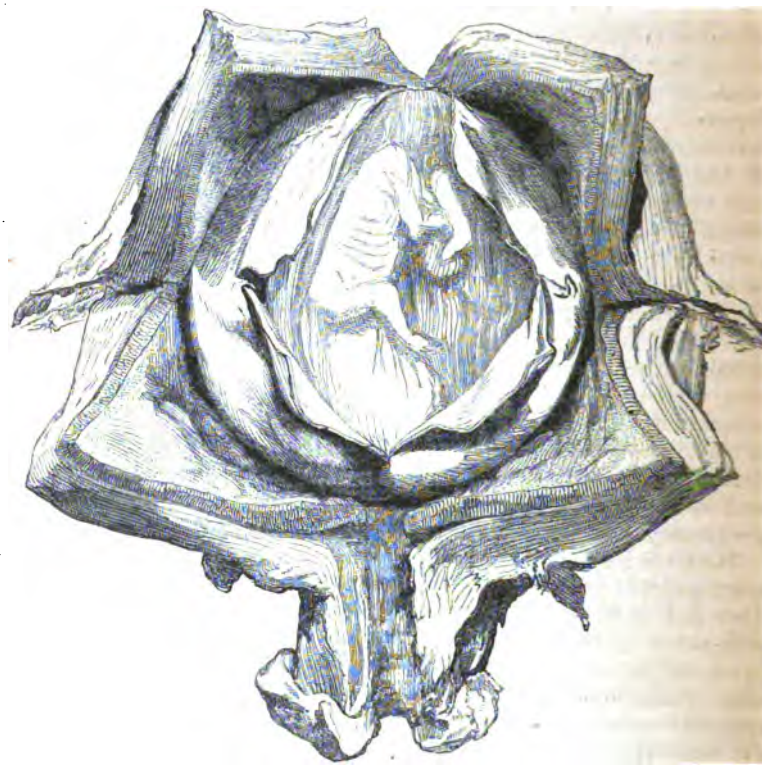
Seiler, C. Weber, and other continental physiologists, were the first to lead the way to more correct views of the nature of the decidua. They opposed the opinion of John Hunter and his numerous followers, that it resembled coagulable lymph in its formation, and described it as a peculiar development of the internal membrane of the uterus. Subsequently, E. Weber, Sharpey, Reid, Goodsir, and others, demonstrated the glandular structure of the decidua, and Dr Sharpey, in particular, threw much light on the whole subject by his investigations into the anatomy of the corresponding parts in the bitch. It is from 1842, however, when M. Coste communicated to the Academy of Sciences the results of his investigations, that we date the complete remodelling of our views on this subject. In 1846 I had an opportunity of examining M. Coste's preparations, and since then have made several observations in regard to this subject.

The preparation (figured in the woodcut) beautifully illustrates several points in regard to the decidua. After describing it briefly, I shall, founding on this and similar observations, proceed to some deductions in regard to points in obstetrics which have not been explained hitherto.

The woman from whom this preparation was taken, died from causes quite unconnected with the uterus or its functions. Judging from the development of the fœtus, she had arrived at least at the eighth week of pregnancy. The mucous membrane lining the uterus or the decidua vera is seen to be of great thickness, that lining the cervix is unchanged. The openings of the cervix uteri, and of the fallopian tube of the left side are easily seen, that of the right tube has been destroyed in dissection. The decidua reflexa completely covers the ovum, being a thin layer without glands springing from the decidua

[1] M.S. Notes of Lectures, in the Library of the Royal College of Physicians. The cavity of the uterus is external to the decidua reflexa,—between it and the decidua vera.

vera. The cavity of the uterus is still not closed, there being ample space all around the ovum between the two parts of the decidua, from the os uteri to the tubes.



NOTE.—In order to have the whole drawing included in the page, the size is considerably reduced from that of the orginal. The decidua reflexa has been opened by a crucial incision, and the four flaps turned aside. The fœtus is seen through the membranes; its superior and inferior extremities are well developed. The cavity of the cervix is filled up with the mucous plug.

On examining the uterus of a woman dying during or soon after menstruation, we find the mucous membrane of its cavity vascular, much injected, red in colour, soft and permeated by its numerous tubular follicles, having the appearance of white lines in a cross section. It is, in fact, not capable of being distinguished, except by the minor degree of its evolution from the decidua of pregnancy. We have thus, at every menstrual period, a nidus prepared for the ovum about to be expelled, and to be made available as a nidus, should the ovum become impregnated. The human female, thus, at every menstrual period, passes through the first or initial processes of pregnancy.

Now, whilst most obstetric authors of repute have with great reason

doubted or denied the occasional presence of real menstruation during the latter months of pregnancy, they have very generally admitted its occurrence, very frequently once, more rarely twice, or thrice after conception. But at the same time they have felt the difficulty involved in admitting its derivation in any case from its ordinary source, seeing that they believed the uterus to become sealed up by the decidua, or otherwise, immediately after conception. The preparation depicted gives anatomical proof of the possibility of this secretion being derived from the lining membrane of the cavity of the uterus, up till the end, at least, of the second month of pregnancy. It would only be necessary to have the mucous plug in the cervix displaced, of which so much notice is made by obstetricians. Now it is quite possible, nay, probable, that the persistence of this plug may sometimes be even injurious, by retaining the sanious or other fluids (hydroperione) which may accumulate above it to an injurious extent; and there is certainly no reason to think that its displacement should involve the ovum in any danger. My dissections of virgin and unimpregnated uteri have frequently, indeed generally, demonstrated the existence of this firm and adherent plug of mucus in the non-menstrual state. From these considerations, it is manifest that the secretion of the menstrual fluid from its ordinary source, and its subsequent discharge, are in no manner impossible in early pregnancy.

Now menstruation proper is a generally received indication that conception is possible. And there is no reason to believe that this does not hold equally good of the menstruation occurring in early pregnancy as at other times. Physiologists have found no difficulty in admitting the possibility of superfœtation during the first few days after conception, or as Velpeau,[1] Allen Thomson,[2] and others, specially point out, till the uterus becomes closed up by the decidua. Of this, the authentic instances of twin-births, where one child has been born black and the other white, in accordance with successive impregnations by black and white males, form incontrovertible illustrations. But the dissection above described shows that the uterus is not closed up till sometime about the third month, and there is no reason to think superfœtation impossible till this takes place.[3] The uterus does not become closed, and repeated impregna-

[1] Traité Complet des Accouch. Bruxelles, 1835. P. 194.

[2] Cyclop. of Anatomy and Physiology. Vol. ii., p. 469.

[3] It is now established, chiefly by the researches of Pouchet, Raciborski, and Bischoff, that a mature ovum is ordinarily expelled after every menstrual period. On the occurrence of pregnancy, the maturation of ova in the Graafian vesicles, and their expulsion, are generally arrested till the end of the period of suckling. But this is not invariably the case. In the human female the sign of ovulation being about to take place is the occurrence of menstruation. The discharge of the menstrual fluid necessarily displaces the mucous plug in the cervix of the pregnant as of the unimpregnated uterus, and thus clears a passage for the conjunction of the ovum of the female with the spermatozoon of the male.

tion is not impossible, till the ovum is so large as to force the decidua reflexa into close and firm opposition to the decidua vera. Up till this time (in the third month) there is a sufficiently free communication between the ovary and the vagina, or between the ova and the semen. In the uterus described, there is at least as much free space for this communication, as exists in the long and very contracted cervical passage of the virgin uterus. Numerous physiologists adduce the plug of viscid cervical mucus as an impediment to impregnation during pregnancy; but frequent examinations have convinced me that this plug is dense and well developed, and apparently impassable in the non-menstruating unimpregnated uterus, and as it does not prevent conception then, so there is no reason to ascribe to it this function in early pregnancy.

Without taking up space in going over the numerous cases of superfœtation which are everywhere recorded, it may be stated that this explanation will account for all the authentic cases. For if we suppose in an instance of this kind, that the first child is born prematurely, but within the limits of viability, we thus gain two months; and if impregnation may take place between two and three months after conception, we have thus four or five months of interval accounted for between the births of successive viable infants.

In the same way it is easy to explain the difference in apparent age and development often observed in twin-births; which may thus be instances not unfrequently of superfœtation with this peculiarity, that both children, though conceived at different times, are born at or near the same time.[1]

The dissection above described, and numerous others, have demonstrated that the decidua is formed by the development of the normal mucous membrane of the uterus, without closing the tubes or cervix uteri. The only new production is the comparatively thin uniform membrane of the decidua reflexa, which springs from the mucous membrane and envelopes the ovum. Burns,[2] Velpeau,[3] Campbell,[4] and others, with their erroneous notions of the origin of the decidua reflexa, have ascribed to it the function of limiting the motion of the ovum on its arrival in the uterus; and in the same way have attempted to account for the various positions of the placental insertion. But such opinions are now untenable.

The mucous membrane of the cavity of the uterus during and for some time after menstruation, or, in other words, the menstrual membrane, may be described as cramming the organ, so much so as frequently to be projected into folds. Now the ovum, propelled by

[1] I may state that all such cases cannot be explained by the supposition of superfœtation. Most conclusive evidence to this effect is afforded by a preparation in my collection of a double monster (anterior duplicity), in which the fœtuses differ considerably in size; and it is difficult to conceive that superfœtation has anything to do with the anomaly of double monstrosity.

[2] Anatomy of the Gravid Uterus, p. 201.

[3] Traité Complet des Accouch., p. 158, etc. [4] System of Midwifery, p. 85.

the vermicular action of the tube, advances into the open cavity of the uterus with its so hypertrophied lining. On its arrival there, it is probably about the size of a pea, and is consequently very soon arrested in its motion, and takes root, as it were, somewhere in the vicinity of its point of ingress.

The ovum, on its arrival in the uterus, will therefore move in the direction of least resistance till it become fixed. Obstetricians have hitherto often been puzzled to explain the insertion of the ovum over the os uteri, or on the side of the uterus opposite to that indicated by the corpus luteum as the side by which it entered, and have suggested to themselves wonderful processes by which the ovum might burrow beneath the decidua to arrive at the distant site, or be otherwise conveyed thither. But our improved knowledge of the decidua dispels these difficulties. It may thus be easily conceived that if on the arrival of the impregnated ovum in the uterus the peculiar hypertrophy of its lining membrane exist only in a small degree, the ovum may enjoy some freedom of motion in the enlarged cavity. Such a state of matters might, *à priori*, be looked for in women having the peculiar erethism of menstruation in a low degree, or in such as have borne families, or in whom, from any cause, the cavity of the womb is slightly enlarged.

At an advanced stage of pregnancy our only guide as to the original site of the ovum is the insertion of the placenta. This is found in every part of the uterine cavity; but its rarest and undoubtedly its most dangerous position, is when it is implanted over the cervix.

Now it is curiously in accordance with the details just given that we find the ovum descending to this position much more frequently in cases of repeated than of first pregnancies. Dr Simpson has pointed out this fact, and suggests that it might afford some clue to the discovery of the cause or causes of placenta prævia. He has shown that, in the Dublin Hospital, while Dr Collins was master, first labours formed thirty per cent. of all the deliveries; and that notwithstanding this large proportion, not one of eleven cases of placenta prævia occurring in that time was a first pregnancy. Of 136 cases of placenta prævia tabulated by Dr Simpson, only eleven were first labours. In fact, a very great majority of the women had had several children previously; and it is to be observed that these figures give far too low an estimate of the relative rarity of placenta prævia in first labours, on account of the great numerical excess of first over second or third labours, and so on.

It may happen, in some cases, that from similar causes an impregnated ovum may not find an appropriate nidus for its development, either from the comparatively atrophied state of the decidual membrane, or from the patency of the uterine cavity admitting of its escape into the cervix below the seat of the proper decidual structure.

[The preceding communication is a condensed statement of part of a tentative lecture delivered before a committee of the Royal College of Physicians, previous to their granting the author their certificate of qualification to teach midwifery, etc. It also formed the subject of a demonstration to the Physiological Society on the 5th of March.]

ARTICLE V.—*Monthly Report on Cases treated in the Regimental Hospital, 79th Highlanders, during the month of January* 1853. By JOHN GRANT, Esq., Surgeon.

Delirium Tremens.

FOUR cases were treated during the month.

It is the opinion of some authorities, that one form of this disease is produced *a potu continuo*, and another variety *a potu intermisso*; the former is said to be directly, the latter indirectly, occasioned by the abuse of intoxicating liquors. It is farther maintained, that where there has been no intermission of the habitual stimulus, the symptoms are characterised by vascular excitement of the membranes of the brain or encephalitis, requiring local depletion to a greater or less extent; but where there has been a suspension of the ordinary indulgence, *true* delirium tremens will be the result, or that form of the affection attended with nervous exhaustion.

These distinctions I have been unable to verify, and have failed to recognise any peculiarity dependent on stimulation continued up to the period of attack, or on its temporary interruption, and am disposed to ascribe the varieties of this complaint to the causes which modify disease in general,—viz., the age, habits, and constitution of the individual.

In military practice, where the patient is generally seen at the commencement of the disorder, its different stages are readily observed; and in the first stage, when the pulse is rather below the natural standard of frequency, and there is as yet no great amount of cerebral disturbance, the remedial agents are of course somewhat different from those indicated in the second stage, which is characterised by symptoms of re-action with decided mental alienation.

The exhibition of large and repeated doses of opium appears to be of more than questionable utility, and may perhaps be superseded by the internal administration of chloroform. Meantime, I have, in the second stage of delirium tremens, relied almost exclusively on small doses of tincture of opium (eight or ten drops every hour) with tartar emetic, as recommended by Dr Graves in cases of fever, and have found this formula equally beneficial in the delirium attending erysipelas capitis, typhus fever, and delirium tremens, in which

affections the pathological character of the cerebral disorder appears not unfrequently to be very much allied.

Although the stomach is generally tolerant to a considerable extent, the antimony in the above mixture has often an emetic effect, which, if attended by the ejection of bile, is often followed by the most beneficial effects; and I can readily understand the utility of emetics, as reported by American practitioners.

Authorities are agreed as to the indispensable necessity of evacuating the accumulated and disordered secretions of the chylopoietic viscera; and I have generally commenced the treatment by the exhibition of calomel, colocynth, and croton oil.

Febris Typhus.

Another case has presented itself, where the infection was received at Glasgow, and where petechiæ were visible on admission.

Wine was administered early, and its use continued for some days, until re-action was evidenced by the disappearance of the petechiæ, and a change in the character of the pulse, which became slower and fuller, and the heat of surface presented a more healthy glow, not easily described, but different from the burning dryness at first observable.

The quantity of wine was then diminished, and finally withdrawn, as amendment proceeded, and the patient became able to receive more nourishment.

On admission he had an emetic, and the same evening two grains of calomel and three of James' powder, which, acting rather freely on the bowels, he has had no other laxative and no other medicine, except Graves' mixture, which seems to have a beneficial influence in procuring sleep and restraining the delirium, which in this case continued to a greater or less degree for fourteen days. In fevers of this nature, where follicular disease of the intestines may be apprehended, and where nature is probably eliminating the *materies morbi* by the kidneys, purgative irritation and derivation by the intestinal canal, may, it is supposed, be in many instances, withheld; and in the present case, the patient, after the first day of admission into hospital, was a week without any alvine evacuations, when a natural motion was procured by enema.

Peritonitis.

This case occurred in a young soldier of weakly constitution, with symptoms of low fever, and great debility, contra-indicating local or general depletion. The disease ran a very rapid course, and during the last twenty-four hours was attended with black vomit, matter of the same character being voided per anum shortly before death, and found post-mortem in the stomach and duodenum. The peritoneum was inflamed throughout, the convolutions of the intestines adherent

from the exudation of recent fibrin, patches of ecchymosis on the mucous membrane of the stomach, with congeries of bloody points from which the melænic matter had been furnished.

Cases of peritonitis with this complication are, I imagine, extremely rare in this country.

EDINBURGH CASTLE, February 1853.

Part Second.

REVIEWS.

An Essay on the Action of Medicines in the System, or "on the Mode in which Therapeutic Agents introduced into the Stomach produce their peculiar Effects on the Animal Economy," being the Prize Essay to which the Medical Society of London awarded the Fothergillian Gold Medal for 1852. By FREDERICK WILLIAM HEADLAND, B.A., M.R.C.S., etc. 8vo. London: Churchill. 1852. —(*Continued from page* 259.)

WE subject ourselves to some self-restraint in refraining from continuing to discuss at full length the remaining propositions of Mr Headland's book. The occasion is tempting, but we are afraid, even with our full persuasion of the importance of the subject, that we may weary our readers. We therefore content ourselves with merely indicating the propositions that remain, restricting our comments to two subjects of much theoretical and practical importance, —tonics and antiphlogistics.

Tonics, as our readers have already learned, are included by Mr Headland among his restorative hæmatics. He describes them in the following passages:—

"ORD. IV. TONICS.

"Under this term I intend to include only the bitter principles of vegetables. The chief medicines of this order are as follows:—

"*Alkaloids and neutral principles.*—Quina, Cinchona, Bebeerine, Narcotine, Salicine, etc.

"*Bitter Drugs.*—Cinchona, Quassia, Cusparia, Gentian, Calumba, Chiretta, Centaurium, Menyanthes, Rhubarb, Hops, Elm and Willow Barks, Tansy, Wormwood, Chamomile, Cascarilla.

"The power by virtue of which these medicines are enabled to act as tonics is due, in each case, to a vegetable alkaloid or neutral principle, having a bitter taste. All of those mentioned above, with the exception of Salicine, contain Nitrogen. The uses of tonics are of a twofold nature. Firstly, they are of use in simple debility and in atonic dyspepsia. Here they give an appetite, increase the muscular strength and powers of digestion, and improve the general health.

In the second place, they have all, more or less, a curative power in ague, and other periodic disorders caused by marsh miasm. Many explanations have been given of this curious and double action of vegetable bitters: and some erroneous theories have been assumed, and false analogies attempted, in the classification of these remedies. The subject is certainly a difficult one, and there are at starting several questions which require a distinct consideration and reply. Upon the answers given to them it must depend whether they should rightly be grouped with blood-medicines or with nerve-medicines; and, if the former, with restoratives or with catalytics.

"Do these medicines act primarily in the blood, or on the nerves? Is their action of a permanent character? Are there any substances in the blood which resemble them? May they remain in the blood, or are they always excreted? If acting in the blood, are they wont to effect a cure by supplying to it a material wanting, or by counteracting in it a morbid process?

"These questions require each a satisfactory reply before it can be proved that I have done rightly in classing tonics among restorative medicines; for they coincide with the minor propositions in which I have defined the action of such remedial agents.

"Quina may be taken as the type of the whole order. It is the most powerful; and the others all more or less resemble it. Perhaps Cusparia bark comes nearest to the Cinchona alkaloids in its anti-periodic action. It is worthy of remark, that some common bitters which owe their efficacy, not to alkaloids, but to neutral principles, as Quassia, Calumba, and Gentian, possess the least control over periodic affections.

"There can be no doubt that these active principles are all absorbed and pass into the blood. They may easily be dissolved out of the vegetable which contains them, if not by simple water, at all events by such an acid liquid as we find in the stomach. We possess positive proof of absorption in the case of Quina, which has been detected by Tiedemann and Gmelin in the blood of a patient to whom it was administered.

"Having them now in the blood, we arrive at the first question,—Do Quina and other vegetable tonics act on the blood or on the nerves? This has been answered at once by many writers, as if it were a thing of great simplicity. But it does not seem to be so. Tonics, as we know, have the power of communicating health and strength in debility produced by various causes, and also of arresting the progress of intermittent fever. Do they effect these things by bettering the condition of the blood, and from it that of the system at large?—or do they at once, and in the first place, influence the nervous system? This is an important question, and it has been variously answered.

"Dr Pereira, in his classification of medicines, ranks them among *Cinetics* (κίνω, to move), which are defined to be medicines exerting a power over the motor system of nerves, and through them on the muscles. But in a subsequent account of Quina, he states that its action is quite inexplicable, and that its use in ague must be ranked with some other special and ill-understood actions, as that of Mercury in syphilis, or of Arsenic in lepra.

"Dr Neligan (in his work *On Medicines*) lays it down that tonics act as stimulants when given to a healthy man. Dr Pereira, on the contrary, states what is more consonant with general experience,—*i. e.*, that a moderate dose of a tonic has little or no effect on a man in perfect health. Dr Neligan admits that their action is permanent, and produced slowly; he also declines any explanation of the action of Quina and others in ague, calling them specifics.

"Some other authors have been still more decided in classing tonics with nerve-medicines. Dr Guy (in his edition of *Dr Hooper's Physicians' Vade Mecum*) considers that stimulants and tonics should rightly be classed together, for that stimulants act as tonics to the weak, and tonics as stimulants to the strong.

"It should be observed, that the irritant action on the stomach of a large dose of a bitter medicine appears to have been the chief foundation of this frequent

opinion of the stimulant action of tonics. The irritable stomachs of nervous persons are more easily affected in this way. Some, too, are met with who bear tonics worse than others, on account of an idiosyncrasy or peculiarity of constitution. But this irritation, and the headache and febrile symptoms which succeed to it, do not surely constitute the proper action of a tonic medicine, which is found to operate most favourably when given in too small a dose to produce anything like a stimulant effect. Nor do I think it proved that any true stimulants are capable of communicating a permanent tone to the system, or to any part of it.

"Dr Ballard also (in *Ballard and Garrod's Materia Medica*) states, that tonics act first upon the nerves, and through them on the muscular system. He opposes them to sedatives, and ranks them with stimulants; quoting Strychnia as an example of a stimulant which gives tone to the muscular system But, in the first place, this alkaloid is quite exceptional among stimulants; and further, the comparison with tonics seems unnatural, inasmuch as the action of strychnia is more or less immediate, not slow and permanent, and it evidently influences the spinal system of nerves in the first place.

"The results of large doses of Quina in producing determination of blood to the head, ringing in the ears, and vomiting, seem to me to mark its action as an irritant poison, and not to be characteristic of tonic medicines. (*V.* p. 79.)

"The action of tonics has been more correctly defined by another able authority. 'Tonics,' says Dr A. Billing, 'are substances which neither immediately nor sensibly call forth actions like stimulants, nor repress them like sedatives, but give power to the nervous system to generate or secrete the nervous influence by which the whole frame is strengthened.'

"This definition I would accept in a modified sense, considering that no permanent alteration in the nervous system can be produced without a primary impression on the blood. Dr Billing further considers, that the supposed stimulant action of tonics in some cases should either be attributed to the operation of the disease, or of some other medicine administered along with them; and that though in large doses they may sometimes produce a quick pulse with headache, and in other instances depression with nausea, yet that these effects should be ascribed merely to their irritating action on the stomach.

"On the whole, it seems to me that those authors who, in defining the action of tonics, have commenced by saying that they act on the nervous system, have started with a mistaken notion; and I am more of the opinion of Dr A. T. Thomson, who classes them as medicines which act on the muscular and sanguiferous systems."—Pp. 117-121.

We have quoted this long passage, with the abstract which it contains of the opinions of various authors on the subject, because, without doing so, we could hardly place in fair contrast to these the ideas entertained by Mr Headland. We take this opportunity of saying, that none of these expositions of the *modus operandi* of tonics exactly agrees with our notions on the subject. We agree more closely with Dr Ballard than with any other of the authorities here cited, our idea being, that tonics act partly directly *upon* the nervous system, but chiefly *through* the nervous system, upon contractile fibres, especially upon those which give tone and elasticity to the blood-vessels, and that thus, by restoring the circulation and nutrition to a more normal state, they produce those effects for which they are valued in general debility, and in many forms of actual disease. Mr Headland, we apprehend, loses much time in fighting with a man of straw when he combats the supposed doctrine of there being neurotic medicines, by endeavouring to point out that

they are not quick and sudden in action, that their effect is not transitory, that they do not show their effects as evidently in healthy as in diseased states, and that they do not act upon the superficial nerves when applied to them. The most intense neurist does not hold them to act after the same fashion as opium or belladonna, when they are administered medicinally. They do not affect the nervous system so visibly as morphia, for example, simply because they are not possessed of such active properties, and the diseases in which they are used do not require them to be given in such doses as to produce a sudden and immediate action. But is Mr Headland right in asserting, that they, physiologically considered, do not act like other neurotics. We apprehend not. He asks:—

"Are they quick and sudden in action? Is their effect transitory? Is it evidenced in health as well as in disease? Do they act on the superficial nerves when applied to them? In each case the answer must be a negative. In all of these particulars the operation of vegetable bitters differs from that of neurotics, and coincides with that of hæmatic medicines."—Pp. 121, 122.

Now, in reply to these questions, and taking as our example quinine, which is the chief subject of his remarks in this section, we venture to reply, in exact opposition to him, affirmatively. We own at once that the quinine cannot be affirmed to act when applied to the superficial nerves, as opium or belladonna will act, because it does not produce either anæsthesia, convulsion, or paralysis, which are the only phenomena that we are capable of readily observing in experiments of this nature. But, in reply to his other questions, we only ask him to swallow ten grains of sulphate of quinine some morning, and to mark its effects. We will venture to say, that it will quickly and suddenly produce a buzzing in his ears, and probably a tremor of his hands; that this effect will be transitory, and that it will occur as readily amidst the health which we devoutly trust he now enjoys, as it would do in the intermission of an ague, from which we hope he may never suffer. It is then, we affirm, capable of affecting the nervous system; and the only reason why, in the practical use of it, we do not see these phenomena produced is, that we do not, in using it medicinally, require to give enough of it at one time to produce them. We hold that a complete fallacy lurks under the argument used by Mr Headland, that these symptoms produced by large doses mark its action as an irritant poison, and are not characteristic of it as a tonic. Assuredly this is quite correct in one sense. They are not characteristic of its action as a tonic; because we can obtain its tonic effects without producing these unpleasant symptoms; but it is one thing to admit this, and quite another thing to infer from this, which we presume is Mr Headland's intention, that it acts by a different *modus operandi* in these two cases. If it be capable of affecting the nervous system in a certain dose—say ten grains—it surely cannot be denied that it must be capable, in a two grain dose, of affecting the nervous system to some

extent also, though, from the smallness of the dose, this may not be very perceptible. It seems a very odd theory, to hold that ten grains will act on the nerves, but that two grains repeated at intervals five times will leave the nerves totally untouched, and confine their action solely to the blood. We have turned back to page 79, to which he refers us, and all we can gather from what is written there is, that he considers that these effects are due to a local irritant action, but we fail to perceive any proof of a local irritant action to which such effects could be referred. If a ten grain dose of quina caused vomiting and purging, or acute pain in the stomach, or any symptom peculiarly referable to that, antecedently to producing vertigo or tinnitus, we could understand this argument, but it does no such thing; and we maintain, that these head symptoms, etc., are simply evidences that quinine can produce an effect on the nervous system which is peculiar to, and characteristic of it. And, moreover, this peculiar effect will follow the frequent administration of small doses, which can produce no local irritant action, as well as one large dose, which might possibly have some such effect. It is, we apprehend, a very unsafe line of argument to maintain, that because the phenomenal manifestations of a large dose differ from those of a small one, therefore the first operates in one way, and the second in another. The superfluous, or, if you choose, poisonous action of a large dose is much more likely to be identical a kind, only greater in degree, than that of the small dose. The death by coma, from opium in poisonous quantity, is but an exaggeration of its hypnotic effect when given in moderate doses. The property by which strychnia kills by tetanus, is only an aggravation of that power by which it restores activity to a paralysed limb. The ringing in the ears, and the so-called febrile symptoms, from a large dose of quinine, are only a higher manifestation of the power by which, in medicinal doses, it produces its curative effects. It is precisely one of the results which unavoidably follows that separating of the physiological from the therapeutical action of remedies which we formerly deprecated, that instead of bringing us nearer to simple intelligible views of the *modus operandi* of a medicine, it leads us to assign to it in slightly varying circumstances, such as that of the mere quantity given, actions which are contradictory or incompatible.

We have failed then to be convinced by Mr Headland's preliminary or physiological argument, that tonics do not operate on or through the nervous system, and must therefore act through the blood; and still less are we convinced by the following sentences, which disclose his argument from pathology.

"The condition of debility, whatever its proximate cause, seems always to be traceable, in the first place, to a want in the blood, which interferes with the due exercise of their functions by the nerves and other organs, by impairing their nutrition. It follows fevers, and accompanies chronic diseases, in both of which cases the blood has been exhausted by continual waste and excretion, without the maintenance of a proper supply. In cancerous, scrofulous,

scorbutic, and dyspeptic habits, the blood may be deteriorated by a fault in the assimilative processes. When, in these instances, there is marked anæmia, iron may be of most service; but when the blood is poor, without any apparent deficiency of red colouring matter, then are bitter tonics needed to improve its condition, and form a valuable adjunct to the special remedies that the case may require. They ought not, as a general rule, to be administered in high fever, or when the pulse is hard, the tongue coated, or the stomach irritable. A loss of appetite, a nervous headache, a soft compressible pulse, a quivering tongue, a flabby condition of the muscles, with general inertia and indisposition to exertion, are indications for their employment. In some cases emetics and antimonials, in others mild purgatives, are of use in preparing the system for their reception.

"It seems, then, that debility is to be attributed generally to the state of the blood, and is to be cured by improving it. By so doing, we may communicate tone to the muscular system, improve the appetite, and increase the nervous force."—Pp. 122, 123.

We cannot say that we have any very definite notions of what that peculiar state of the blood is, when it is poor without any apparent deficiency of red colouring matter, or how, upon the restorative hæmatic principle, a bitter tonic can remedy this state, except by its becoming converted into fibrine or albumen. We should say, that when the blood is poor, the bitter tonic would most likely restore matters to a more normal condition, by enabling the stomach to receive and digest more food, and thus to supply to the system the materials of good blood. This of course, according to our notions, it does by an effect on the nerves and muscular fibres of the stomach. But we do not need to discuss this at length. Our present purpose is to make out whether, by his pathological remarks, Mr Headland produces good evidence, that a bitter tonic operates only by directly restoring something to the blood, or by an action of some other kind in the system; and we must say, that if we had been seeking for proofs that its action is most probably through the nerves, we could not effect our purpose better than by enumerating, as the symptoms which indicate the necessity for its use, "a loss of appetite, a nervous headache, a soft compressible pulse, a quivering tongue, a flabby condition of the muscles, with general inertia and indisposition to exertion." If these symptoms do not point pretty directly to the nervous system, we know not what symptoms do. Mr Headland, after these general pathological remarks on debility, proceeds to show that intermittent fever is also a blood disease; but we must not enter upon the consideration of this point, because the subject of intermittence as a phenomenon of disease is one both too interesting and too extensive to be discussed in a cursory manner. We follow Mr Headland to his next minor proposition, which is involved in the following question. Are there in healthy blood any substances which resemble them? The importance of a distinct answer to this question is obvious. If we cannot find in the blood anything resembling a bitter tonic, we find nothing which the said tonic can supply to the vital fluid, and therefore the restorative hæmatic action will be improved. The great argument for the doc-

trine that iron acts in this way, is the very obvious one, that iron is a normal constituent of blood, and that, when it is deficient, certain morbid states manifest themselves; that when, under these circumstances, iron is administered, these symptoms disappear, and the blood is actually found to have regained its normal proportion of the metal. Now, before we admit the hæmatic restorative action of a bitter tonic such as quinine, we have a right to an equally clear demonstration, that in normal blood there does exist something which resembles a bitter tonic: that in certain diseases this substance is wanting; that these diseases are cured by bitter tonics; and that after their use the said constituent of blood is again found in its normal quantity.

Whether Mr Headland has succeeded in achieving this demonstration, our readers will judge, when they have perused the following extract:—

"Another and a more important suggestion has been made by the same chemist (Liebig). He has pointed out a chemical analogy between certain vegetable compounds and a substance which exists in the bile. He has shown that for the most part the elements of the bile re-enter the blood after passing into the intestine, scarcely more than the colouring matter being finally excreted with the fæces. He has found that if an enema of bile be injected into the rectum, it becomes absorbed there, and does not afterwards pass out into the urine. What the exact function of the bile may be is as yet undecided. Its most important constituents are a non-saponifiable fat, cholesterine; a neutral substance, taurine; and an acid, called choleic, in combination with soda, which latter is present in excess. Bile is bitter to the taste; and ox-gall, or the bile of an ox, has been found, when administered as a medicine, to have an action which resembles that of tonics. It appears that both the bitterness and the tonic action reside in the taurine.

"M. Liebig has pointed out an important chemical resemblance between this taurine and the vegetable alkaloids. Of these he has singled out caffeine, the peculiar principle of tea and coffee, as most analogous to the biliary product. From the analysis of these two substances he has deduced the following formulæ:—

$$\text{Taurine} = C_4\ H_7\ NO_{10}.$$
$$\text{Caffeine} = C_8\ H\ N_2\ O_2.$$

"Then he shows that an atom of caffeine, if added to nine of water and nine of oxygen, will form two atoms of taurine. (*Organic Chemistry*, 1842, p. 180.)

"It should be remarked, that a different formula for taurine has been arrived at by M. Lehmann and others, who assert that it contains sulphur. Choleic acid, and other constituents of this complicated fluid, certainly contain sulphur. But even if sulphur be combined with taurine, it is not clear that it enters with it into the blood. And the above formula for taurine has been confirmed by the analysis of M. Löwig.[1] It seems to be sufficiently correct for our purpose.

"With the supposed function of caffeine as a creator of bile, however probable or interesting, we are not now immediately concerned. We are engaged in inquiring whether there is naturally in the blood any substance which resembles the tonic alkaloids. Liebig has shown that this element of the bile is

"[1] He states that one equivalent of binoxalate of ammonia and four of water contain together the same elements as an atom of taurine (*v. Simon's Animal Chemistry*, vol. i. p. 47. *Syd. Society.*)"

intended for re-absorption into the circulation. And, without theorizing at all on the subject, it may readily be demonstrated that, with the existence in the blood of such a material as taurine, we cannot say that we have not in that fluid an analogue to substances like quina. Taurine is soluble in water, and crystallisable; is chemically analogous to the tonic bitter principles, and contains the same four elements as quina. If necessary, this similarity could be exchanged for identity, and quina could be formed into taurine in the blood.

"If there be one change in the blood to which, more than to any other, the substances which enter it are liable, it is certainly the process of oxidation. Free oxygen is continually entering the blood through the air-cells of the lungs.

"The formula for quina is $C_{20}\ H_{12}\ NO_2$. Adding to this 45 atoms of oxygen, we could make out of it one atom of taurine, together with 16 of carbonic acid and 3 of water.

$$C_{20}\ H_{12}\ NO_2 + 45\ O = C_4\ H_7\ NO_{10} + 16\ CO_2 + 5\ HO.$$

Thus it is conceivable that this and similar alkaloids may, at the same time, supply an important material to the blood, and serve as fuel to support the animal heat, by combining with oxygen, and giving off carbonic acid. I would not of course affirm that this actually does occur; but if it were to take place, we might gain, in this oxidation of quina, some clue to the production of that quickening of the pulse and increase of the general temperature, which are affirmed by some to follow the ingestion of the alkaloid, even in small doses. But it is now sufficient for us if we have ascertained that there is naturally in the system, and in the blood, a substance which resembles quina. We may be allowed to infer, that the presence of the latter substance in that fluid would not be unnatural to it."—Pp. 125–127.

We suspect that very few persons will be inclined to adopt this hypothesis of the action of quina upon the above evidence, and any little hesitation they may entertain as to rejecting it, will, we believe, be removed at once, if they take into consideration the chemical history and properties of taurine. It is possible that some of our readers may not know much about taurine, or perhaps may never have heard of it at all, for we are not aware that it has ever before been considered as playing so important a part in the economy as is here assigned to it. We will therefore inform them shortly regarding it. In the first place, as its name implies, it is essentially a constituent of ox-bile, and it has even been doubted whether it can be obtained from human bile at all; but it seems now to be established that it can be got there, but chiefly in morbid states of the hepatic secretion. Now these are not the circumstances under which we should expect to find a substance whose presence in the human system is so important, that when deficient it leaves the body a prey to ague and other maladies, requiring quina for their cure. If quina had been, *par excellence*, a remedy for the diseases of neat cattle, there might be some ground for supposing that it acted by supplying that taurine in which their bile abounds, and which is here said to resemble quina in constitution. But quina is essentially a human remedy, and taurine is very sparingly a human product, and, moreover, occurs not so much in healthy as in diseased bile; and would therefore appear to be, if it really plays any part in the economy at all, not so much a preservative against disease as a cause of it, and therefore requiring not so much something to restore it to the body,

as something to get it out of the body as fast as possible. But our non-chemical readers will perhaps be astonished to learn that the said taurine probably does not exist as a natural constituent of any bile, either bipedal or quadrupedal. For in fact taurine, or rather its elements, in the natural bile of the ox, constitute, along with a peculiar acid from bile, cholic acid—a conjugate acid—which has been called choleic or taurocholic acid; and it is only when this is boiled for some hours with hydrochloric acid that taurine separates from it. Lehmann, one of the latest writers on this subject, seems to have some doubts as to whether it is a substance naturally existing in bile or not. In one part of his work, translated by Dr Day for the Cavendish Society, he says, "Taurine has never been found isolated in the healthy organism; it appears to be contained preformed in normal bile, and to occur there as an adjunct of the already described cholic acid; at all events, it only occurs in an isolated state in decomposed or morbid bile." But in his chapter on taurocholic acid the same author says:—"We must therefore regard taurocholic acid as containing an adjunct rich in sulphur, which, on its separation from the choleic acid, becomes converted into taurine." This we think, considering how taurine is prepared, is more likely to be true. It is a product, not an educt, from the bile, and has no real existence as a constituent of the healthy secretion of the liver.

But Mr Headland tells us with all the force of words, and numbers to boot, that taurine resembles quinine so much in its properties that, even should the taurine not exist ready formed, the quinine may act, by applying to the system those elements, which the chemist can compel to assume the form of taurine. He tells us that Liebig has assigned to taurine the formula $C_4\ H_7\ NO_{10}$, and he quotes Löwig as confirming this. He tells us it is true that others assert it to contain sulphur, but he throws doubts upon this statement. Now, we think that in common fairness he ought to have stated that ever since the researches of Redtenbacher no chemist doubts that taurine contains twenty-four per cent. of sulphur, its true formula being $C_4\ H_7\ N\ S_2\ O_6$, and, consequently, that Liebig's fanciful analogy of caffeine and taurine, as well as Mr Headland's equation for making taurine out of quinine, are utterly untenable,—the one being a body rich in sulphur, the other not containing a particle of this element. The truth is, to use a homely expression, taurine is no more like quinine than chalk is to cheese. And as to taurine possibly being absorbed without its sulphur, we first desiderate proof that it is absorbed at all, and, secondly, how it could leave its sulphur, which is so mysteriously incorporated with it, and is so difficult to separate from it, as to have escaped for many years the observation of the lynx-eyed Liebig and a host of other chemists, without ceasing to be taurine at all. If taurine could shake off its sulphur as an inconvenient piece of baggage when it goes upon its absorption travels, why might it not just as well leave its nitrogen also?

We have made these remarks, not so much for the sake of demolishing Mr Headland's hypothesis, for at the most he only states it as being conceivable, as for the sake of pointing out one of the prevailing bad practices of the present day—we mean the system of putting upon paper an imposing array of figures in the form of an equation, and applying this to the explanation of questions in physiology, pathology, and therapeutics. Nothing can exceed the value of an equation for setting simply and clearly before us an explanation of something that has been ascertained by actual experimental observation; but an equation, such as Mr Headland's, quoted above, in which, merely because it brings out his sum correctly, he arbitrarily assumes a special number of atoms of oxygen, is no contribution to medical science, whatever it may be to arithmetic. It is said that this does no harm because it is only a hypothesis. It does do harm; it leads men away from proper subjects and methods of inquiry. It dances before their eyes like an *ignis fatuus*, and lands them in such sloughs as that in which Mr Headland has here entangled himself. In fact, by this prevailing style of argument any body may prove anything he likes in any department of science on which chemistry can be brought to bear. Give us a strong battalion of oxygen atoms, such as Mr Headland has brought into the field, a detachment of hydrogens as light troops, and a few heavy ammonias by way of a breaching battery of artillery, and we will undertake to invest, batter, and carry by assault any imaginable position in physiology, pathology, or therapeutics. When we read such an equation as that which Mr Headland has set before us, we are strongly reminded of our dear old friend Jonathan Oldbuck's etymological discussion. "Not a step, not a pace, not a shathmont, as I may say: the meaning of which word has puzzled many that think themselves antiquaries. I am clear we should read *salmon-length* for *shathmont's-length*. . . . Shalthmont—Salmont—you see the close alliance of the sounds; dropping out two *h's* and a *t*, and assuming an *l*, makes all the difference. I wish to Heaven that no antiquarian derivation had demanded heavier concessions."

Our devout aspiration is, would that all therapeutical difficulties could be satisfactorily settled by the simple process of assuming 45 *Os.*, dropping out 16 CO_2 *s.* and 5 *HOs.* Cocker and the therapeutical interest, would then be our cry "to the country."

But to return from the Antiquary to the therapeutist, Mr Headland, in pursuance of his taurino-quinine doctrine, devotes two or three pages to an elaborate but most unsuccessful attempt to connect periodic diseases with impaired hepatic function. The hepatic and splenic congestion which occurs in ague is more frequently an effect than a cause of the attacks; and even were hepatic disorder a constant accompaniment of ague, which it is not, though a very common one, especially in hot climates, it does not help us a bit towards an explanation of intermittence and periodicity as a cha-

racter of disease, or of the action of quinine as the heroic remedy for intermittent and periodic affections. Mr Headland must bear in mind that there are more periodic diseases than ague. We do not presume to speak very confidently with regard to it, because, fortunately for Edinburgh, indigenous agues are very rare here, and the cases of imported ague which we have occasion to treat have very commonly been contracted in the English fens or in foreign climates, and any hepatic derangement which they may have is rather to be connected with the general effects of the tropical climate than with the special disease. In fact, according to our observation, hepatic derangement is rare even in such subjects. But there is another periodic disease, from which our climate does not leave us exempt, with regard to which we can speak most confidently, and which quinine seldom fails of curing very readily—we mean periodic headache; and in this affection we make bold to say that symptoms of hepatic derangement very rarely, if ever, manifest themselves. We must therefore protest against his hepatic pathology of periodic diseases, and still more decidedly against his practice, when he makes the following statements:—

"There is another fact which gives additional probability to such an idea. Another remedy of a different kind has been used in all the diseases in which Quina is admissible, proving in some cases superior, and in other instances second only to it in its beneficial action. This is Mercury; used in remittent and yellow fevers; of the first importance in dysentery; employed by Dr Baillie in ague, and pronounced by him to be in some cases superior even to Quina. In small doses it is frequently of use in cases of debility and scrofula. And Mercury is a Cholagogue; *i. e.* an agent which is known to have the effect of promoting the secretory function of the liver. Thus we may conceive that Mercury, not given in excess, or to salivation, may operate in a different way to produce the same end as Quina. One explanation would suffice for both."—P. 131.

If we were disposed to be savage, we could not wish our worst foe a more hideous punishment than to be placed with a racking hemicrania under a taurine pathologist who should proceed to treat him with blue pill or calomel instead of quinine, as being "in some cases superior, and in other instances second only to it in its beneficial action."

But we must farther remind Mr Headland that even were he more successful than we allow him to be in settling the pathology and therapeutics of periodic diseases, these are not the only cases in which quinine and other tonics are useful. Has he never, for *instance*, treated a case of strumous conjunctivitis; has he not seen that though blistered and collyrium'd with most intense assiduity, it has still remained the same troublesome unimprovable complaint till he put the patient under quinine, and then it began to improve on his hands? Was this complaint, like an ague, due to a deficiency of taurine in the system, or was something else wanting here, and must a new formula be devised to explain the effect of the tonic in the cure of the conjunctivitis. Deficiency of fatty prin-

ciples has been alleged to be the source of the strumous tendency, and cod-liver oil, "in some cases superior, in others second only to quinine," is adduced in proof. Here is some work carved out for the chemical arithmetic, or rather the arithmetical chemistry. The fact must be explained on restorative principles. Nothing more easy. Knock out 6 *C's* and 2 *N's*, and assume 10 *H's*—the thing is done. From two atoms of quinine, we get, or as Mr Headland naively expresses it, "we could make out of it," one atom of margaric acid, the great constituent of animal fats; for

$$2\,C_{20}\,H_{12}\,NO_2 - C_6\,N_2 + H_{10} = C_{34}\,H_{33}\,O_3\,HO.$$

To be sure this is not a very elegant equation, but it brings us in the end the formula for margaric acid; and therefore holding our pathology and restorative therapeutics and equation of strumous ophthalmia to have as much foundation in fact as that for ague propounded by Mr Headland, we flatter ourselves that we have made out that quinine cures opthalmia on restorative principles by forming fat in the system.—Q. E. D.

The truth is that the radical defect of all Mr Headland's reasoning as to the *modus operandi* of tonics lies in the humoralism by which he is held in bondage. Were he only contented to see in the action of quinine and its congeners something of an impression made upon the living solids, instead of something supplied to the secretions and the blood, he would not be obliged, in pursuit of therapeutical truth, to rush into so dark a cavern as the gall-bladder, and bedaub his theories with ox-bile and taurine.

(To be continued.)

Part Third.

CLINICAL REPORTS, LECTURES, ETC.

CLINICAL MEDICINE.—PROFESSOR BENNETT.

FUNCTIONAL AND ORGANIC DISEASES OF THE STOMACH.

CASE IX.—*Cancer of the Stomach.*

Alexander Dunn, aged 55, sawyer, admitted 24th January 1853, states he enjoyed good health until about six months ago, when his appetite began to be very capricious, and he soon lost all relish for food of any kind; he gradually got weaker, and rapidly lost flesh. During the last three months he has been unable to follow any occupation. Ten weeks ago he began to suffer from pain and uneasy sensations after taking food, which was rejected at considerable intervals; but during the last six weeks the vomiting has be-

come much more severe, liquids being rejected almost immediately after they are taken. Says that he has never vomited blood; but that the fœcal matter has been frequently of a very dark colour.

On admission, there is great emaciation, and he has a peculiar sallow appearance, with a haggard worn-out expression of countenance; the tongue is dry and cracked, there is constant troublesome thirst, and the appetite is very much impaired. He has some difficulty in swallowing, and feels as if there were some obstruction to the food when it reaches about the lower part of the sternum. All kinds of food are rejected by the stomach, generally about three hours after it is swallowed; occasionally ten to twelve hours may elapse, at other times not more than half an hour. Liquids of every kind come up almost as soon as they are drank. The bowels, during the last eight or nine weeks, have been very costive, and the stools of a very dark colour and hard consistence. On physical examination of the abdomen, the muscles seem very tense and retracted; the epigastrium and right hypochondrium are prominent, and occupied by a distinct tumour of a roundish but elongated shape, stretching from the cartilages of the false ribs, beneath which it seems to emerge, passing downwards and forwards into the epigastrium and towards the umbilicus. Its surface feels rough, and in the neighbourhood of the umbilicus there are several small isolated lobules or tumours about the size of a hazel-nut, which are hard, somewhat moveable, and resemble enlarged glands. The hepatic dulness begins at the sixth intercostal space, and extends to the level of the umbilicus, where its outer margin can be felt exceedingly hard and distinct; splenic dulness apparently normal. He has generally but little cough, but occasionally it is very severe, occurring in paroxysms, and causing urgent dyspnœa. There is no dulness over the lungs on percussion, but the respiratory sounds are harsh and exaggerated, with prolonged expiration, without increase of the vocal resonance. Pulse 110, small and fluttering; heart's sounds normal; urinary functions normal. ℞. *Sol. M. Morph.* ℥iss.; *Sp. Ammon. Aromat.* ℥ss.; *Mist. Camph.* ℥iiiss. *M. An ounce to be taken every 3 hours. To have 4 oz of brandy.* January 25th.—Had slight vomiting yesterday evening, after which he was much exhausted; pulse 120. He was very quiet during the night, but does not sleep much. To-day, he had for breakfast an egg with a little tea and bread; about three hours afterwards vomiting came on, the ejected matters consisting of the half-digested food, with a large quantity of whitish-brown fluid like weak tea and of very acid re-action. He appears very weak, is much exhausted, and complains greatly of thirst. *To have the following injection every three hours. Beef-tea two ounces, the yolk of one egg, and half-an ounce of brandy.* January 26th.—Was considerably weaker last night although he had had two of the enemata, the greater part of which he retained. He slept better during the night, and took a little tea and biscuit for breakfast. There has been no vomiting since. Pulse exceedingly weak, almost imperceptible. The extremities rather cold. He is evidently sinking fast. *To continue the medicine and enemata as before, and to have ice to allay the thirst.* January 27th.—There was no marked change in the evening, but he became gradually weaker during the night, and died this morning about eight o'clock.

Commentary.—This case is a very valuable one when taken in conjunction with the other instances of stomach disease we have studied. It differs from them in the circumstance that there was evidently a tumour compressing the orifice of the organ, and occasioning the pain and vomiting which occurred after taking food. Considering the man's age, the nodulated character of the tumour, and its rapid progress, we can have little doubt as to its being of a cancerous nature—and probably soft cancer, involving the lymphatic glands in the neighbourhood of the pylorus. It is very possible that the coats of the stomach were also involved, as they exhibited great irritability after fluids had been swallowed, but we have no evidence of ulceration having occurred, as there had been no hematemesis. Unfortunately we could not obtain the con-

sent of the friends to open the body, and hence the extent to which the stomach was involved must remain a matter of speculation. All the symptoms observed in this man may be produced by a tumour outside the stomach pressing on the organ, as well as by disease of the organ itself, and nothing is more difficult, if indeed it be possible at all, than to diagnose these conditions from each other under such circumstances as this case presented. When, on the other hand, in addition to the local swelling, vomiting after taking food, there is hematemesis, and especially if the blood present the appearance of coffee-grounds, showing it has remained in the stomach for a certain time previous to its rejection, then we may, in the majority of cases, confidently predict the existence of cancerous ulceration of the mucous surface.

In many cases the lesion hitherto described as scirrhus of the pylorus or stomach seems to be a simple hypertrophy of the muscular and fibrous coats, which may or may not be associated with cancer of the neighbouring glands. A simple stricture of the pylorus may in this way occur, producing more or less thickening of the organ from the necessity of increased action to overcome the obstruction, or in maintaining chronic vomiting, such as occurs in the intestines generally, bladder, or other hollow viscera. I have recorded four cases of this kind in my work on "Cancerous and Cancroid Growths." (Edinburgh, 1848, p. 46 *et seq.*) In all such cases, it is observable that the same emaciation and cachectic appearances are present as in instances of undoubted cancer, a circumstance which is attributable to the impeded nutrition of the body rather than to a supposed cancerous diathesis. On this account I have long ceased to place any confidence in the so-called "cachectic appearance" as diagnostic of cancer, attributing it either to imperfect nutrition, or to wearing down from excessive pain. It is often present in many other forms of morbid growth, and frequently absent when the disease has been proved to be cancerous by a microscopic examination.

Of all the forms of cancerous disease, that of the stomach is perhaps the most distressing, cutting off as it does the supply through which nourishment enters the system, and inducing, in addition to the wearing-down pain, loss of sleep, and loss of blood, more or less constant vomiting. Still it is our duty to relieve and support the system; and to this end opiates in large doses, nutritive enemata, careful regulation of the diet, and ice allowed to dissolve in the mouth, are our best remedies. Local frigorific mixtures, as recommended by Dr J. Arnott, and the application of a few leeches to the episgastrium, are also occasionally beneficial.

DYSENTERY—GANGRENE OF BOTH LUNGS

Thomas Marshall, æt. 29, chimney-sweep, admitted November 23, 1852. He states, that he has been troubled with slight cough for many years back. Eight weeks ago he experienced rigors without any obvious cause, followed by increase of the cough, looseness of the bowels, severe griping pains in the abdomen, and frequent desire to go to stool, with much straining and tenesmus. He observed a small quantity of blood in the stools, which sometimes consisted only of about a table-spoonful of blood mixed with frothy mucus. The calls to stool were at first so numerous that he cannot state even the probable number in the twenty-four hours. They abated somewhat under treatment at one of the dispensaries, but the diarrhœa has continued more or less ever since. During the last eight weeks the cough has become much aggravated, and the sputa increased in quantity. Two days before admission, he observed that the sputa were of a dirty-red colour, having formerly consisted of thick purulent masses without any tinge. Since the 21st November he has continued to spit this dirty-coloured matter in small quantity, generally during the night, having very little expectoration during the day. He states, that for the last eight or ten weeks he has been losing flesh and strength to a great degree, though he does not present a very emaciated appearance. His diet also, during that period, has been very defective, both in quantity and quality; but pre-

viously he had always been able to procure good food. He is addicted to the immoderate use of spirits, and has several times had delirium tremens.

On admission, the tongue is dry, with slight, dirty-yellow fur; troublesome thirst, but appetite good. No nausea, but frequent inclination to vomit when the cough is severe. No pains in the bowels when at rest, but griping pains are generally present when at stool. There is distinct tenderness on percussion in the right iliac region over a space about two inches. The bowels have been open six times during the last twenty-four hours. The stools are very copious, of thin, fluid consistence, and of a brownish-red colour. They contain numerous blood corpuscles, as determined by the microscope. The urine is passed in small quantity, but otherwise normal. The chest on percussion, appears resonant throughout, except over the upper third of right lung, where very slight dulness can be detected. The respiratory murmur is heard all over the chest, but under the right clavicle it is harsh and exaggerated, and the vocal resonance greatly increased. Posteriorly, there is comparative dulness on percussion, and increased vocal resonance over the whole of right side, but most marked at the apex. Over the lower third on the same side, fine crepitation can be heard during forced inspiration. Sputum in large quantity, consisting of frothy tenacious mucus of a dirty-brown colour, becoming more fluid upon standing. The sputa have a very offensive odour. When examined by the microscope, numerous pus-cells, some blood corpuscles, and large quantities of disintegrated epithelium, can be detected. Has no pain in chest. Breathing is not laboured, but cough is troublesome, especially at night, occurring in paroxysms. Expectoration difficult. There are slight tremors of the hands, but no other symptoms of delirium tremens. Other functions normal. ℞. *Tinct. Opii*, 5ij. *Tinct. Catechu*, 5ss. *Confect. Aromat.* 5ij. *Mist. Cretæ*, 5v. *A table-spoonful every four hours. Habeat Enema, cum Tinct. Opii*, 5j. ℞. *Æth. Sulph. m.* xl., *Mucilaginis*, 5ij.; *Mist. Camph.*, 5j. *Fiat haustus hora somni sumendus. Ordered also* 4 *oz. of wine and nutritious diet.* Nov. 26.—Bowels open nine or ten times during the twenty-four hours; stools watery, and contain distinct traces of blood. Much griping pain when at stool. Cough severe. Expectoration copious. Dec. 1—Diarrhœa continues, notwithstanding he has taken regularly the astringent mixture and opiate enemata. To-day the skin is hot and dry, the appetite bad, with great thirst. *Habt. Pil. Plumbi cum Opio*, 3tia *quâque hora.* Dec. 2—Has had four or five stools since last night. They are feculent, but of very thin consistence, unmixed with blood. He feels very weak, and complains much of pain in chest, especially right side. Dulness on percussion is increased over the whole of the right side posteriorly, and is most marked over the lower third. There is great increase also of the vocal resonance, amounting to pectoriloquy in the supra-scapular region; loud moist râles like gurgling are heard over the right back. On the left side the respiration is harsh and exaggerated, and the vocal resonance is also loud. Breath and expectoration fœtid. *Omit the pil. plumb. opiat., and continue the medicines ordered on the 24th.*

Dec. 6.—Diarhœa abated; but he feels very weak, and cough troublesome; crepitation, with increased vocal resonance, pretty general over the left back. *To have 3 oz. of spirits.* Dec. 9.—Diarrhœa has returned—stools thin and fœcal, containing a good deal of blood. *Injicietur enema amyli c. tinct. opii.* 5iss. Dec. 14.—Diarrhœa has somewhat abated until to-day, when it has again returned as severely as before. Much pain in lower part of abdomen; considerable griping and tenesmus; pulse 100, small and weak, but regular. *The opiate enema has been continued every evening since last report, and he has been taking a chloric æther and morphia mixture to relieve the spasmodic cough. Appl. vesicat* 4 × 3, *abdomini. Increase the wine to* 6 *oz.* Dec. 18.—Has rallied greatly since last report, but to-day the diarrhœa has again returned. The cough is very troublesome—sputa tinged with blood; over the whole of the right back there are harsh gurgling cavernous râles, with a noise approaching to a metallic character when he coughs; mucous rattles heard over left back; the offensive odour of breath and sputa has increased since last noticed.

℞. *sol. mur. morph.* ℥j.; *tinct. catechu* ℥j.; *mixt. cretæ* ℥j.; *spts. ammon. aromat.* ℥ss. *M. sumat hora somni.* Dec. 20.—Symptoms the same, with the exception of increased weakness and emaciation. *Still continues to take the opiate enema and stimulant draught at night, and astringent mixture during the day.* January 1.—Little change has taken place in the symptoms since last report. One day he has appeared to sink, but has rallied again the next. Diarrhœa has continued, averaging about twelve stools in the twenty-four hours. The stools consisted of very small quantities of dark matter, with mucus, and occasionally a little blood. Has continued taking the mixture of ammonia, etc., and the astringents with an opiate enema, which has latterly been administered twice a-day. Died this morning at five A.M.

Sectio-Cadaveris, 31 hours after Death.—Body much emaciated.

Chest.—Heart and pericardium normal, right lung united to thoracic walls by firm and universal pleuritic adhesions. On being removed and bisected, a black gangrenous cavity, five inches long and four broad, was exposed, occupying the upper and middle lobes. It contained an extremely fœtid olive green diffluent matter, having no distinct structure. The walls of the cavity presented a distinct lining membrane of condensed fibrous tissue, internally of pulpy consistence and blackish-brown colour. The surrounding pulmonary tissue, posteriorly and inferiorly, was hepatised and friable, with a few anfractuous cavities also gangrenous. Anteriorly the lung was emphysematous, with here and there portions of collapsed tissue. The left lung adhered to the thoracic walls posteriorly by pleuritic adhesions. On being removed and bisected, the lower lobe and inferior portion of upper lobe congested, and presented a brick-red colour; hepatised and dense in various places, with irregular cavities containing fœtid brown sloughs, varying in size from that of a pea to a pigeon's egg, and communicating more or less with each other.

Abdomen.—Mucous surface of cœcum and ascending colon closely studded with irregular-shaped ulcerations, varying in size from a pin's head to a sixpence. Some of these were evidently chronic, and in process of healing, with a thickened worm-eaten edge and a bluish granular base; others contained a sloughing centre, involving more or less of the muscular coat. Similar ulcers were scattered, but more sparely, through the transverse and descending colon. Other abdominal organs healthy.

Commentary.—In this case we have an example of acute dysentery associated with pneumonia in both lungs, which passed into gangrene, occurring in an individual whose constitution had been impaired by addiction to intoxicating drinks. Dysentery generally prevails during the autumn months in Edinburgh to a greater or less extent; and in the case of Marshall, it came on without any obvious cause, in September, and was ushered in by rigors, followed by bloody and purulent discharges at stool, with tenesmus and abdominal pain. About the same time, also, pectoral symptoms were complained of, although it is not probable that decided pneumonia was then occasioned. Even when he came into the house it was limited in extent, and ran a tolerably acute course subsequently. The febrile symptoms, therefore, which existed previous to his admission, were, most probably, connected with the dysentery. This ran its usual course, producing sloughing ulcers in the mucous membrane of the large intestines to a considerable extent; and, by the irritation and continued discharge they occasioned (which could not be checked), gradually prostrated the patient, and was the chief cause of his death.

It is seldom we have an opportunity of seeing a more illustrative case of gangrene of the lungs, than this man presented,—large and circumscribed on the right side, extended and diffused on the left side. That in both cases, the gangrene was preceded by pneumonia, there can be little doubt, as all the functional signs of this lesion were present during life, whilst after death the gangrenous cavities were everywhere surrounded by distinct pulmonary hepatization. There is every reason to suppose that the same general causes which produced the sloughing sores in the intestines, occasioned the gangrenous

cavities in the lungs. A deficiency of vital power in the organism, prevented these transformations necessary for the absorption of the exudation, and thus it died and underwent putrefaction. Inflammatory gangrene and ulceration both depend on death of the exudation, but in the former case there occur those peculiar chemical changes which induce putrefaction. In the case of the lung the only symptom which indicates this is fœtor of the breath or of the sputum, which was very apparent in Marshall on his admission to the house, and greatly increased afterwards. In a case which entered the ward in 1848, and was the subject of careful examination, I diagnosed a gangrenous cavity in the right lung, and separated it from phthisis, partly on account of the fœtid odour, and partly on account of the situation and limitation of the cavern. The man had swallowed a piece of bone four years previously, which, on his death, was found impacted in the bronchus by Dr James Struthers, then clinical clerk, who opened the body.—(Monthly Journal, November 1852.) In such a case the exudation, though for a long time healthy, must have been continually poured out from the vessels in the neighbourhood of the irritating body and been absorbed, but when, after a time, the vital processes had been worn out, then gangrene occurred. It may be well, however, to say a few words with regard to this occasional termination of inflammation.

PATHOLOGY OF INFLAMMATORY GANGRENE.

Occasionally a very large amount of blood plasma is thrown out, constituting a violent inflammation; a greater or less number of capillaries are also ruptured, and blood corpuscles are more or less mixed up with the *liquor sanguinis* exuded. The exudation thus formed compresses the part, so as to obstruct the blood-vessels, and prevent the continuance of any circulation in it. Under these circumstances, instead of forming a blastema for the production of new structures, it undergoes chemical changes, which induce in it decomposition, and the part is said to be mortified, or to be affected with moist gangrene. This change commences first in the blood extravasated, which becomes of a purple colour more or less deep; the corpuscles break down and become disintegrated; their hematozine dissolves and colours the serum; and, should the exudation have coagulated, it forms brown, rust coloured, purple, or blackish masses. An acid matter is now formed, which, acting on the neighbouring tissues, produces fœtid gases, that are abundantly given off from the affected part. Sulphuretted hydrogen is evolved, which causes the blackish sloughs usually observed in such cases, and discolours silver probes and the preparations of lead. After a time, the elementary tissues surrounding or involved in the exudation become more or less affected. The transverse striæ in the fasciculi of voluntary muscles become first pale, and are then obliterated. Cellular tissue, fat, and other soft substances lose their connection, and fall into an undefined granular mass. The tendons and fibrous tissue retain their characteristic structure for a long time after the other soft parts have been reduced to a softened pulp. The bones resist the action longest, but at length become rough, soft, and commencing externally, are more and more broken down, and reduced to the same pulpy consistence and granular structure as the surrounding parts.

As the tissues thus become broken down and fluid, they are discharged from the system in the form of an ichorous matter, which, examined microscopically, presents numerous granules, imperfect or broken down cells, blood corpuscles, and fragments of filamentous tissue or the other structures involved. If the morbid action be seated in the subcutaneous tissue, the skin soon becomes involved; and an opening is formed, which rapidly increases, and gives vent to the discharge. In a similar manner, gangrene of internal organs, by destroying the intermediate parts, at length enables the discharge to reach the surface, or to find its way into the excretory passages, such as the bronchi, the intestinal canal, the meatus auditorius, etc. In this manner life may be endangered by the destruction of organs necessary for its continuance, by the exhaustion resulting

from the discharge, and sometimes by the absorption of the ichorous matter, which, on entering the circulation, acts as a poison to the economy.

It may be asked, whether inflammation and mortification are similar processes? whether the latter is only a greater intensity of the former? or whether, when mortification follows inflammation, it is dependent on other circumstances, such as a peculiar state of the atmosphere favouring the decomposition of the exudation poured out? In order to answer any of these questions, we must distinguish between mortification arising from a variety of circumstances, and an inflammatory gangrene properly so called, the which is undoubtedly the rarest of all the terminations of inflammation. We frequently see mortification produced by the application of chemical or mechanical agents, which directly destroy the tissues. It also arises from severe and complicated injuries, in which arteries leading to the portions of structure affected have been divided or crushed. In old persons, it follows obstruction in the blood-vessels, or is dependent on circumstances not yet ascertained. In none of these cases is it caused by inflammation. But when stasis of the capillaries is produced to a considerable extent, followed by the exudation of a large quantity of blood-plasma, which, instead of passing into organisation, undergoes the changes previously described, then an inflammatory mortification, properly so called, is produced. We see this take place after burns, a long exposure to frost, and in certain cases of erysipelas. Here the amount of exudation is considerable, the pressure caused by it extreme, the obstruction to the circulation in the neighbouring parts correspondingly great, and these, as well as the exudation itself, die. In this sense, therefore, it may be said to depend on the severity of the inflammation. This, however, is not the case in the sense of those who consider the adhesive, suppurative, and gangrenous inflammations as different stages of one process. Suppuration, as we now know, has no connection with adhesion; it is opposed to it; nor is it in any way related to mortification, which must be considered as a primary alteration of the exudation. The vitality is lost, and instead of passing into organisation, it at once becomes subject to the chemical laws of dead matter, and undergoes putrefaction.

Now, in order that organic substances should enter rapidly into putrefaction, it is necessary that they find sufficient oxygen and water for all their carbon to be transformed into carbonic acid, all their hydrogen into water, and all their azote into ammonia. When these conditions are not completely fulfilled, transition or intermediate substances are formed. When there is not sufficient oxygen, for instance, an excess of carbon is produced in the debris, and hence the black colour observed in mortified parts. There is also often developed a species of contagion, which causes parts undergoing decomposition to excite it in neighbouring ones (eremacausis of Liebig). This does not take place in dry gangrene. Thus a gangrenous stomatitis (Cancrum oris) destroys, in a short time, a large portion of the soft parts of the lips and face; Noma destroys the genitals of young female children. This appears to depend upon the quantity of destructive fluid or mixture generated in the process. A dry gangrenous foot, on the other hand, often requires several weeks before it has produced sufficient decomposition to be detached, and reached all the tissues to the bone.

But there are sometimes external causes which seem to produce mortification, independent of the amount of exudation or the rapidity with which it is thrown out. During the summer of 1836, I watched with great care the progress of a sloughing gangrene, prevalent, not only in the Infirmary of Edinburgh, but throughout the city generally. All kinds of sores and wounds were affected by it, even those of a specific nature, such as chancres, etc. Neither youth nor age was exempted from it. It affected not only those who were debilitated from disease by intemperance or by diet, but those also in the most robust health. Thus a servant girl, aged 16, who had never suffered from illness, and of a robust constitution, fell down upon some glass bottles, and slightly cut her left thumb. A week after, she entered the Infirmary with an ulcer the size of a shilling, filled with a brownish-black slough, discharg-

ing a fœtid and sanguineous fluid. In this, as well as other cases which occurred, it became impossible to attribute the gangrene to the violence of the injury, the amount of exudation, a state of cachexia, or indeed to any circumstances connected with the individual. It could not arise from contagion, as it originated simultaneously in different parts of the city in individuals who had no communication with each other, was not confined to the infirmary, and the system of dressing wounds there precludes the possibility of this explanation. We are, therefore, compelled to ascribe the cause to something without.

Most writers have noticed the connection between a certain state of the atmosphere and the prevalence of hospital gangrene and of dysentery. Its more frequent occurrence in summer and autumn—that is, at a period of the year when increased temperature favours the decomposition of animal matter. The good effects which result from change of air, when every kind of treatment fails, still further point out its origin from changes occurring in the atmosphere. These probably depend upon some electrical state not yet explained, which powerfully influences the chemical combinations of the diseased part, and prevents cell growth. At least such is what we may reasonably suppose from all the facts with which we are acquainted on this head. It is similar to blight among vegetables, the potato disease, and so on.

TREATMENT OF RHEUMATISM BY LEMON-JUICE.

CASE I.—Abigail Rankin, a servant, æt. 39, admitted 15th December 1852. Had rigors on the 7th, followed by febrile symptoms and acute pain in all the joints. On admission, pulse 100, full and strong; heart sounds normal; considerable febrile symptoms; acute pains and swelling in all the joints increased on motion; much sweating at night. Other functions healthy. *Habeat. Succ. Limonis*, ℥ij. *ter indies.* On the 17th, she was ordered ℈j. of Dover's powder. December 20th.—The pains have continued as acute as ever till to-day, although she has taken ℥vj. of lemon-juice daily. At present she can move the joints slightly without so much suffering. *Habeat* ℥iij. *Succ. Limonis ter indies.* December 22d.—There was great sweating last night, and to-day is much better. *Habeat* ℥j. *Succ. Limonis ter indies.* Some swelling of the left wrist joint remained until the 23d, on which day all pain had left her. Dismissed well January 6th, 1853.

CASE II.—Catherine Rooke, æt. 21, married, admitted December 23d, 1852. Had rigors on the 14th, followed by febrile symptoms, and excessive pain, at first, in the knees and ankles, but subsequently in every joint of the body. On admission, pulse 84, of moderate strength; heart's sounds and impulse normal; the joints are more or less swollen, painful on pressure and on motion; skin bathed with perspiration; febrile symptoms with the exception of increased pulse well marked; a considerable deposit of lithates in the urine. Other symptoms normal. ℞. *Pulv. Dover, gr. x. statim sumend.* ℞. *Sol. Mur. Morph.* ℥ss.; *Potassæ Bitant.* ℥ss.; *Sp. Æther Nit.* ℥j.; *Aquæ*, ℥j. *Ft. Haust. hora somni sumendus.* On the 25th, purgatives of calomel and jalap were ordered. December 26th.—The pain and swelling of the joints have somewhat diminished, but are still very acute. *Habeat. Succ. Limon.* ℥j. *ter indies.* January 2, 1853.—The pains have slowly subsided since last report, but there is still considerable soreness and stiffness of the knees. The arthritic swellings have everywhere disappeared. January 4th.—Acute pain has returned in the right arm, which she cannot move. January 5th.—Acute pain has extended to the right arm and back. *Omittantur Succ. Limonis.* ℞. *Potassæ Nitratis* ℥ss. *Aquæ* ℥jv.; *M. Sumat* ℥j. *ex aquæ*, ℥iv. *ter indies.* January 6th.—Pains now disappeared; marked improvement. No critical discharge. Dismissed well January 7th.

CASE III.—Thomas Aitken, æt. 30, blacksmith, admitted December 25th, 1852. Fourteen days ago, after exposure to cold, was attacked by rigors, followed by febrile symptoms and pain in his joints, which have continued up

to this date. On admission, pulse 74, rather weak. A blowing murmur, with the faint sound loudest at the apex, which it seems resulted from a previous attack twelve months ago. Slight swelling only in his right hand and wrist, but there is pain in all the joints, more or less, of an erratic character. Febrile symptoms very slight. Slight bronchitis. *Habeat. Succ. Limonis*, ℥ss. *ter indies*. On the 28th, the dose of lemon-juice was increased to ℥j. On January 2d, he was much better; but on the 4th the pains returned, but not so violently. On the 12th, he was free from pain, having had some diarrhœa, and taken two ℈j. dose of Dover's powder. On the 22d the pains returned, but again subsiding on the 24th, he was dismissed.

Case IV.—James Ollason, æt. 20, clerk, admitted January 4th, 1853, with organic disease of the heart of old standing, and chronic rheumatism of an erratic character, sometimes violently attacking one joint and sometimes another, accompanied with swelling and tenderness. Lemon-juice in ℥j. doses was tried three times a day, for four days; but being evidently of little benefit, was then abandoned for opiates and sedatives.

Commentary.—In no one of these four cases in which lemon-juice was given, although in two six ounces and in one nine ounces were taken daily, did it appear to me that the disease was in any way controlled or alleviated by the remedy. In Case I. six ounces were taken daily without any effects, and then the quantity was increased to nine ounces daily, until the 21st day of the disease, when sweating and resolution of the symptoms followed, more from natural crisis, perhaps, than from the effects of the juice. In Case II. the remedy was continued for two days, and until the 21st day of the disorder was fairly passed. Then nitrate of potash was given with the immediate effects of relieving the symptoms—although here also it is not improbable that a natural crisis of the disease was then established. In any case the inefficacy of the lemon-juice appeared manifest. Cases III. and IV. were cases of sub-acute and erratic rheumatism, which also resisted the lemon-juice: the first for a month, the second for four days. On the whole, this trial of the remedy was in no way favourable, and is strongly contrasted with the good effects of nitrate of potash, which I formerly brought before you.

EFFECT OF POSTURE IN REMOVING PASSIVE CONGESTION OF THE LUNGS AND CHRONIC PNEUMONIA.

Helen Bulloch, æt. 18, servant, was admitted November 21st, 1853, with acute rheumatism and mitral incompetence, resulting from previous rheumatic attacks. Shortly afterwards, owing to imprudent exposure of her person, she was attacked with pneumonia first on the right and subsequently on the left side. The treatment was conducted according to the principles formerly stated. Bleeding was avoided. Antimonials and opiates were administered, succeeded by stimulants and tonics. Notwithstanding the gravity of the case, and the prostration resulting from rheumatic fever complicated with mitral lesion, the exudation on both sides was slowly absorbed, and excreted in the form of abundant lithates in the urine at various times. Her recovery, however, was very slow, so that in the middle of January an œgophonic resonance, slight cough, crepitation, restlessness at night, wandering of mind, and considerable weakness, were still present, but without pain. In this state she remained stationary for about two weeks—the shrill vocal resonance, dull look, want of appetite, weak pulse, and continued crepitation, exciting considerable apprehension. At this time, it occurred to me that probably there was much passive congestion at the back of the lungs, caused by continued decubitus; and I ordered her to lie on the chest and abdomen at night and during several hours of the day. On the following day there was a marked change for the better in the physical signs, and in two days more all crepitation had disappeared. From this time she slowly recovered, although she was not able to be dismissed until March 4th, 1853.

INAUGURAL ADDRESS [1]

TO THE MEDICO-CHIRURGICAL SOCIETY;

ON THE MODERN ADVANCEMENTS IN PRACTICAL MEDICINE AND SURGERY.

BY PROFESSOR SIMPSON, PRESIDENT OF THE SOCIETY.

My first duty, in taking the Chair as President of the Medico-Chirurgical Society, is a pleasant duty. For necessarily it consists in returning you my heartfelt thanks for the honour which you have bestowed upon me in electing me to this distinguished office. Believe me that I feel in no small degree flattered by this, as I have been in many previous instances, by the constant courtesy and unvarying kindness of you, my fellow practitioners, towards me. And any Scotchman may, indeed, well feel proud at being elected to preside over this, at once the largest and the most active medical society in this country.

On the utility of such Societies as the Medico-Chirurgical, it is quite unnecessary to dwell. Their advantages have been described to you by some of your former Presidents in terms far more clear and emphatic than any I could hope to employ. And I believe that at the present time there exists but one opinion in relation to the fact, that the study of medicine, like the study of other departments of practical science and art, has been vastly promoted by associations like our own. For all are ready to acknowledge, that the stimulus of mind upon mind, like the friction of iron upon iron, tends, in associations like this, to sharpen and strengthen all those intellectual faculties that are best calculated to promote our professional knowledge, and to develope the higher types of professional character; inasmuch as our mutual intercourse, criticisms, and discussions, form at once a school and an ordeal, teaching all of us constantly to become more zealous watchers of the medical phenomena or facts occurring in our practice—more careful in our observation and classification of these facts—and more rigid in drawing correct deductions and generalisations from them.

None of us, I fear, can plead guiltless to the occasional influence of that besetting sin—indolence; and all will further, I believe, willingly confess, that sometimes its best cure has consisted in visits to such Societies as this, where the mere spectacle of other members actively at work has often stimulated us onwards to renewed efforts, and perhaps aroused within us a commendable desire to imitate the professional industry and professional zeal of our neighbours. Few of us have ever on any occasion attended at this Society without hearing some new professional fact, or some new professional idea, calculated to arrest our attention, and destined perhaps to give us a new and increased interest in some particular disease, or in some particular part in pathology or therapeutics. Societies, in short, like this, stimulate their members onwards by a kind of intellectual contagion; and as the human mind is swayed powerfully by imitation, we feel ourselves, in such associations as the present, often infected by the love of work, and the desire of extending professional knowledge, by having a constantly recurring opportunity of watching and witnessing these qualities manifested in the acts and ardour of others.

Such associations as this Society, banded together for the mutual cultivation of professional knowledge amongst its members, and the general advancement of medical science and medical art, have principally sprung up here and elsewhere within the course of the present century. Numbers of the most valuable professional essays, and even larger works, that have been published within the last fifty years, have originated in communications to some one or other of our medical societies. Many of these essays and works would never have been

[1] See report of the Society in Monthly Journal of Medical Science for February, p. 180. The following observations are extended from the manuscript notes used on the occasion referred to in the report.

written but for this simple origin. And, doubtless, the progress that the healing art has made, during that period, has been in no small degree owing to the stimulating and regulating influence of medical association. In making this observation, I take it for granted that medical science *has* advanced greatly during the last half century,—that is to say, within the period that the Medico-Chirurgical Societies of London, Edinburgh, and similar institutions elsewhere, have been at work. Ever and anon we hear it doubted, by men both out and in the profession, whether medicine has made any marked advances at all during the period that I speak of. Many of us have heard it, I believe broadly insinuated, that, while other departments of science and art have during the last fifty or sixty years been marching forwards at a rate previously unprecedented in their history, the art of healing has remained comparatively stationary. The late numerous and marvellous strides made in practical science are generally dated as commencing with the time at which our illustrious countryman WATT discovered, or rather re-discovered, the steam-engine; or since he and others taught mankind to subdue and harness that mighty but docile monster-power to different kinds of machinery, and trained it to accomplish for the human race works which seemed formerly beyond all human powers and human hope. The resultant wonders of the steamboat and of the locomotive are works of the last thirty or forty years. And whilst, with the swift rush of the iron horse, our inert bodies are now carried from place to place with the speed of the bird, modern science has, of late years, achieved a still greater feat; for it has given us the means of sending even our very thoughts along that still more wondrous railway—the railway of the mind—the electric telegraph. The calculus of the mathematician, and the telescope of the astronomer, have lately proved to mankind that worlds, hitherto undreamt of, circle round our sun. And amidst the thousand marvels and revolutions accomplished during the last half-century by the science of chemistry, no application of that science is perhaps more strange than the fact, that the chemist has taught the artist to convert that sun himself into a mighty and matchless painter, who can elaborate the most difficult portraits, and the most complex landscapes and figure pictures, with a precision and minuteness unattainable by human art,—and can both begin and finish them, too, in a briefer space of time than is occupied by one single dip and dash of the brush of a mortal artist.

But within the same wondrous period of progress during which these brilliant discoveries have been taking place in other departments of art, medicine, we can confidently maintain, has been by no means stationary. On the contrary, it has in various ways advanced by strides as marked and as great as those belonging to any other department of art taken individually. During the period I allude to, its progress has been in various directions and under various forms. To prove this statement, let me endeavour to recal to your recollection some of the principal changes that have taken place since the commencement of the present century, or since the era of Watt's discovery. Of course, I must allude only to the principal amongst them in such a limited address as the present. And for the same reason the subjects I select for remark must be selected arbitrarily, rather than systematically.

Some departments of medical science which were previously comparatively unknown, have in a manner arisen during the last fifty or sixty years, or at least have been practically systematised during it. At the present day we can probably scarcely appreciate the vast importance of some of these, and the advantage which a knowledge of them gives us as practitioners over the cultivators of medicine half a century ago. Nor, perhaps, would it be possible to see and appreciate them in their proper value, unless we were actually again re-deprived of their aid in pathology, diagnosis, and practice, and unless all the knowledge and advantages springing from them were suddenly obliterated and blotted out.

MORBID ANATOMY, for example, belongs in a great measure to this category. It is quite true that pathological anatomy was known, and in some degree appre-

ciated, before the time I speak of, in the same way that the steam-engine was (as I have already hinted) known before the time of Watt. But it is equally certain that it is only during the last forty or fifty years that pathological anatomy has been so studied as to assume the importance of a leading and indispensable branch of medical knowledge.

In such a Society as this, it is, I believe, most unnecessary to dwell upon the advances made in medicine and surgery by the cultivation of pathological or necroscopic anatomy. And its uses have been various. By its light new diseases have been discovered, which were formerly all but unknown; such as emphysema of the lung, œdema glottidis, phlebitis, ramollisement of the brain, endocarditis, melanosis, etc. In other instances, it has enabled us to separate into specific diseases some affections which were formerly constantly confounded together, both in diagnosis and treatment, as the different species of inflammation of the lungs,—pleurisy, pneumonia, and bronchitis;—the different diseases of the heart;—the different forms of tumours, etc. In another range of its investigations, it has been the happy means of enabling us to study and trace accurately the general courses which inflammatory, tubercular, carcinomatous, and other such diseases are liable to follow, as well as the varieties which they are liable to present in different organs of the body. It has changed and corrected our ideas of the nature of some diseases, by showing us, for example, that acute hydrocephalus, which was formerly supposed to be an instance of pure dropsy, is generally the result of inflammatory and acute tubercular disease; that the old nosological dropsies of the chest are almost all instances of disease of the heart; that the so-called fungus of the testicle is an inflammatory and curable and not a malignant and incurable malady; that the gangrena senelis is a result of arterial inflammation and obstruction, and not of weakness, etc.

Pathological anatomy has further been of vast use negatively, by demonstrating to physicians that some affections that were formerly deemed inflammatory or organic are not so. How many lives has it saved, by proving that delirium tremens is not to be confounded, as it was at the commencement of the present century, with inflammation of the brain, and not to be treated on the principles of encephalic inflammation. It has already saved not a few limbs by showing, that in hysterical subjects you may have affections simulating disease of the synovial membrane of the joints, without any such disease being present; and that many cases of local pain,—as in the irritable mamma or testicle, etc.,—are cases not of local inflammation or organic derangement, but of local neuralgia.

These discoveries in pathological anatomy have been revealed by means of the scalpel. But it has latterly acquired other means to aid it in its researches, especially the microscope.

Histological Anatomy is of too late origin,—as applied at least to the investigation of morbid structures and effusions,—to allow the medical profession to judge definitely of the actual extent of usefulness that will be ultimately derived from the prosecution of it. But it has already by itself effected various interesting discoveries in relation to disease. It has, for example, revealed to us one or two affections, the very existence of which was previously unknown, as leucocythemia and sarcina ventriculi. It has disclosed to us the true nature of some diseases, and entirely changed and corrected our notions in respect to them, as by proving the actual cryptogamic structure of various eruptions upon the cutaneous and mucous surfaces of the body. Further, the microscope enables the pathologist to distinguish and determine, with additional accuracy, inflammatory products in the brain and other diseased parts, when their presence might be otherwise a matter of doubt. It affords us also a new element for ascertaining the malignant character of some tumours and discharges. And above all, it has eminently cleared up, and facilitated our knowledge of the nature, as well as of the diagnosis, of the different morbid states of the urine.

Pathological Chemistry forms another department in which medicine has advanced much within the last half century, and in which it is, I believe, yet destined to advance perhaps infinitely more than in any other. Within the last few years organic chemistry has made enormous steps; but they are still too evidently the unsteady, tottering steps of a young giant. And a herculean amount of work and research remains ere we can hope to attain all that we may confidently expect to derive in theoretical and practical medicine from the advances of pathological chemistry. The fluids of the body are reckoned by physiologists, to be in relation to the solids, as ten to one; and in the eclecticism which marks the present era in medicine, physicians are evidently looking every year more and more to an extension in the knowledge of the nature and treatment of diseases, by new and important advances in humorism, rather than by any prospective advances in solidism. And there seems every reason to believe, that if we knew the pathological chemistry of the blood and fluids, as well as we know the pathological anatomy of the solids of the body—a knowledge I repeat that is to be longed for and worked for, but which has been only yet most slightly acquired—then medicine, both as a science and as an art, would probably make a forward march of the greatest moment. But even within the last quarter of a century, it has already progressed in no small degree by a cultivation of this department of study.

Perhaps the greatest individual discovery hitherto made in pathological chemistry and pathological anatomy, taken together, during the period of which I am speaking, has been the discovery, by Dr Bright, of the great frequency and great importance of *albumen* in the urine, as connected often, though certainly not always, with organic changes in the kidney, but indicating always to us some important change in the pathological chemistry of the blood itself. For this discovery, however, a very rude chemistry only was required—the testing, namely of the urine with heat or nitric acid. A higher and more refined organic chemistry may yet enable the physician to trace other special changes of perhaps equal importance in the urine, and in the other fluids of the body in other diseased states. Reasoning, indeed, from other circumstances than the evidence of any actual chemical proof, many physicians at the present time have little doubt that the origin and essence of most specific forms of febrile affections will yet be found lurking in specific morbid states of the fluids of the body, rather than in any morbid states of the solids; to a pathological toxicology, rather than to a pathological anatomy. And let me add, that probably most local inflammations will yet also be traced back a step further, viz. to blood-poisoning—varying in its nature, in different types and species of these inflammations. It is most probably a special form of toxæmia or blood-poisoning for example, which produces the characteristic inflammations of the skin, and different parts of the mucous membrane seen in small-pox, in scarlatina, and in measles; and in each of these diseases there is doubtlessly a specific poison present. It is a form of blood-poison which in all probability gives rise to the numerous serous and other inflammations which are apt to spring up in patients suffering under albuminuria. It is, in all likelihood, some specific form of blood-poisoning that originates the specific inflammations of gout and rheumatism. Blood-poisoning of another variety seems also to originate and beget the internal inflammatory complications seen so constantly in puerperal and in surgical fevers. A higher chemistry may, I repeat, yet enable us to detect the presence of special poisonous or morbid states of the blood, as the causes of these and of other diseases; and may perhaps enable us also to neutralise them in the system; or to eject them from it.

Morbid anatomy, histological anatomy, and pathological chemistry, the departments to which I have already adverted, have thus gained for us, during the last fifty years, much new and precious information regarding the actual character and nature of the diseases of the body. That information, however, would have been in a great degree sterile and barren, had physicians, at the same time, not discovered means for facilitating the detection of the presence of these diversified morbid states in the living body. Medical science has gained these requisite means, by

having, within the last twenty or thirty years, immensely extended and improved the whole subject of

PHYSICAL DIAGNOSIS.—When we contrast medical practice at the present day with what medical practice was half a century ago, in nothing truly will we find it differing more than in the increased use which the modern physician makes in most cases of disease, of the different means of physical diagnosis. By its aid he is enabled, in many diseases, successfully to trace, in the different organs of the yet living patient, the exact pathological anatomy of these organs at the time of his examination. He tries by it, in other words, to discover by physical signs during life, that morbid anatomy of different organs which is revealed by the scalpel of the anatomist after death.

For ages the surgeon has enjoyed over the physician greatly superior diagnostic accuracy in the diseases belonging to his special department of the profession; because he could generally apply to the discrimination of them direct tactile examination, and even visual inspection of the affected part; or, in other words, because he could make complete use of these means of physical diagnosis in forming his opinion with regard to their nature. Latterly the surgeon has even increased his already great diagnostic facilities by the use of anæsthetics, and the adoption of the exploring needle. But in some classes of affections, the physician is now, in relation to physical diagnosis, almost on a par with the surgeon. In affections, for example, of the lungs and of the heart, the physician can trace accurately enough for practical purposes, the various changes that may be taking place in the pathological anatomy of these organs in the living subject, by the ear; or through the medium of auscultation and percussion. Analogous means of physical diagnosis have been, as you are aware, more or less successfully extended to other organs of the body, besides those contained within the chest; as to the throat and uterus. And as the precision of our knowledge of the maladies of these parts has been advanced by physical diagnosis, so also, in a corresponding degree, have the precision and the success of our treatment of them. More, however, is probably yet to be expected from an extension of the same important branch of study. For if the subject of pathological anatomy is, as some suppose, nearly exhausted, there is still, it must be confessed, much room for the advancement of the physical diagnosis during life of the pathological anatomy of some classes of organs as ascertained after death,—as, for example, that of the organs contained within the abdomen. And it may be that particular measures of physical diagnosis may yet be devised for these and other regions,—even though at the present moment the discovery of such measures may seem to our minds as unlikely as the discovery of any accurate means of physical diagnosis for diseases of the thorax appeared improbable and chimerical forty years ago, or before the time of Laennec.

Let me merely add on this matter, that the application of the microscope and of chemical tests to the urine and other fluids of the body, may be truly classed as a new department of physical diagnosis, and one which has been attended with great,—and no doubt will yet be attended with still greater,—results, in determining the nature, and consequently fixing more accurately the treatment, of different diseases. How simple and precise, for example, is now the diagnosis of diabetes, oxaluria, and many morbid states of the urine, as compared with the state of knowledge in these matters only ten or twenty years ago.

MATERIA MEDICA has also effected some most remarkable advances within the last few years. In other words, there has not only been, through the instrumentality of morbid and histological anatomy, and of pathological chemistry, a great increase in our knowledge of the nature of diseases,—and, through physical diagnosis, a great increase in our knowledge of the discrimination of them,—but changes have taken place in the materia medica within the last twenty or thirty years, which have added much to the simplicity as well as the efficacy of the means and weapons by which practical medicine combats the different diseases

that occur in the bodies of the sick. Some new medicinal powers have been added to the list of the Pharmacopœia, as iodine, hydrocyanic acid, gallic acid, cod-liver oil, etc. But the changes I advert to are of a much more important kind than this, though consisting truly, not in additions to our list of materia medica, but in modifications only of some of the more potent medicinal agents previously known and employed by physicians. In making this remark, I allude especially to the modern discovery, under the form of the alkaloids, of the active medicinal principles in our vegetable materia medica. By this discovery our therapeutic means have most undoubtedly been simplified and increased in power and value in a wonderful degree during the last quarter of a century. Only some twenty or thirty years ago there were no means of exhibiting our most powerful and useful vegetable medicines, except in the clumsy form of large powders, or nauseous decoctions, infusions, and tinctures. What an astonishing advance has been effected in this matter by the discovery in most of the principal vegetable remedies employed in medicine of alkaloid principles, as quinine, morphia, etc. Probably, ere many more years have elapsed, all our principal vegetable remedies will have been forced in this way to yield up, at the command of the chemist, their active and characteristic medicinal principles in such a concentrated and intensified form, that, like those I have already alluded to, they may be prescribed by the physician in doses not calculated, as heretofore, to nauseate the poor patient at the mere bare idea of swallowing drugs. Perhaps medical men have been, as a body, far too remiss and careless in following out the indication of the *jucundè* of Celsus in their prescriptions; and on this ground I, for one, believe that a better covering for a pill than we yet possess, or a way of disarming of their disagreeableness the revolting forms of most medicinal fluids and draughts, would in reality prove a more important discovery for the promotion and utility of true practical medicine, than the discovery of matters of apparently far more scientific bearing and moment. And it may be that, following out the same object, we may yet obtain means of fulfilling most of the indications in therapeutics, by administering medicines in other and less repulsive modes than through the stomach, as, by inhalation, inunction, injection, etc.; and that we may yet produce the therapeutic effect desired upon the system with as great certainty, and with infinitely more rapidity, by some of these methods, than by giving medicines of an analogous kind to be swallowed. Medicines, for example, when capable of being inhaled, affect the system with immensely greater rapidity than when the same medicines are taken into the stomach. And perhaps this same principle may yet be advantageously applied to practice in relation to some medicaments. If it is ever, for instance, a matter of importance in some inflammatory or other ailments, to affect the system rapidly and fully with mercury, why may not the chemist discover for us some gaseous and respirable form of mercurial combination, the inhalation of which should salivate in as many hours, as days are now required for the induction of that effect?

Our therapeutic means are most probably destined to have early and important additions made to them through other discoveries in chemistry, particularly in modern organic chemistry. For there can be little doubt that some of those almost innumerable compounds which organic chemistry is daily adding to its stores, will be found to be endowed with marked therapeutic properties, and that we may find in them most powerful and possibly concentrated forms of medicinal agents capable of fulfilling all our principal therapeutic indications,—and perhaps with properties superior to the drugs at present used,—as an antiperiodic, for example, as certain as quinine, or a narcotic, as important as that most useful of all drugs—opium—and yet, without either its direct constipating effects, or its indirect tendency to excite subsequent nausea, vomiting, etc. Perhaps we may even gain new classes of remedies—as one capable of producing the elimination of morbid matters by that most extended of all mucous surfaces—the lining membrane of the lungs. And, let me add, the whole subject and theory of therapeutics—the department perhaps the least advanced at the present moment in medical science,—seems destined, if we may judge from various at-

tendant circumstances, to undergo ere long some great and important revolution by which we will probably acquire a more certain and direct knowledge than we at present possess, of the mode or modes in which different medicines produce their special medicinal effects upon the body, or the different organs of it. Doubtlessly also, if such knowledge comes to be once acquired, it will enable us to apply our remedies with more accuracy and success, in the details of actual practice.

When we turn again from such advances in professional knowledge and professional resources as I have already alluded to, and consider the practical application of that knowledge and these resources in the departments of surgery and of medicine, we find that no small advances indeed have been made during the last half century in these branches of the healing art.

SURGERY, in various important matters, gives strong and undoubted evidence of sound and solid progress in these later times.

At the commencement of the present century, surgery, as a practical art, was perhaps advanced farther than the sister art of medicine. And when we compare its state at the commencement of the 19th century with its state two or three hundred years before that date, every one must confess that there had been ample room and verge for that advancement. For, assuredly, surgical operative practice must have formerly been a dreadful department of the profession, when the art of arresting hemorrhage by the power of a silken thread was still unknown or still unpractised; when in every operating theatre and at every operation the chafing-dish and searing-iron were as indispensable as are now the lint and the ligatures of the modern surgeon; and when the cries and moans of the poor patient were smothered only, if smothered at all, by the hissing of the heated cauteries against the surface of his bleeding wounds. Long, however, as you are well aware, before the commencement of the present century, the ligature had been universally adopted by surgeons in their operations. Indeed, without it, it seems difficult to perceive how practical surgery in later times could have progressed at all. At the same time, is it not possible that surgery, by making one additional step (and finding some more advanced means to do without the irritation of a permanent ligature, and, perhaps, also without the rough and irritating sutures which are used to tie together the lips of the wound), may yet save the sides of the wound from the presence of any foreign body whatever within it, and thus add greatly to the chances of its speedy and general union by adhesive instead of suppurative inflammation?

Since the commencement of the present century, it is scarcely necessary for me to point out to you, that surgery has advanced in various and most important respects; as—

1. In far more clear and correct views than previously existed of the great pathological doctrines of inflammation and hemorrhage; and in the full and successful adaptation in many ways of a knowledge of these doctrines to the details of practice.

2. In many important improvements in individual operations, as in the operations for hernia and in amputations; and particularly in the general principles of operative treatment being now much more simple and rational, and much more safe and certain, than they previously were. As an illustration of this last remark, let me remind you of the surgery of aneurisms, and of the vast difference between the treatment of that disease now, by merely arresting or diminishing the arterial current going to the aneurism at a point above—and where the supplying vessel is itself sound—and the treatment of it sixty or seventy years ago, when the whole aneurismal sac was sometimes laid open, or the diseased limb itself amputated. Another step in advance may yet possibly enable surgeons to cure this disease by some safe simple means or manipulations applied to the aneurismal tumour itself, and capable of producing the direct coagulation of

blood in its interior,—without the risk and annoyance of either compression or ligature of the artery running to the aneurism.

3. Surgeons have lately introduced some altogether new modes of operating, as Tenotomy; and different novel and useful individual operations are now performed on the principle of tenotomy and subcutaneous incision. The extensive modern class of plastic operations belongs to the same category.

4. One or two individual operations, altogether new, have come into practice, and have been already the happy means of saving no small amount of human life; such as the ligature of the carotid, subclavian, and other large arteries; excision of the upper and lower jaws; the perineal incision in aggravated urethral strictures, etc.

5. Surgery, however, has made great advances in other matters. It has become every day more and more the boast and glory of the surgeon, that he is now able to avoid operations which, twenty or thirty years ago, were considered unavoidable: that he can often substitute lesser operations for those of a more grave and dangerous character, as the excision of joints instead of the total and absolute amputation of the limb to which the diseased joint belongs; the removal of the ends of the bones in compound dislocations instead of the removal of the whole extremity; the cure of hydroceles and serous cysts by injections, instead of setons and incisions. In many of the accidents and diseases of the skull, in compound fractures of the extremities, etc., etc., operative measures are far more rarely had recourse to now than they were at the commencement of the present century. In truth, while modern surgery has been, by the aid of anatomy, rendering all its operative procedures more simple and certain, it has been at the same time showing a marked and strong tendency towards the avoidance of all avoidable operations. Its present acknowledged aim is far more than formerly, the medical preservation and cure rather than the surgical amputation and cutting off of diseased parts. In many directions and respects, it is becoming every day more medical and less operative in its character. Witness the treatment of scrofulous bones, joints, etc., by diet, rest, iodine, cod-liver oil, instead of issues, incisions, and amputations. There will always, of course, be lesions, and diseases, and injuries, only capable of being remedied and removed by operative interference; but the perfection of surgery would perhaps be, what it never will be, the power, namely, of removing these lesions, and diseases, and injuries, without the assistance of knife and scalpel.

6. In few matters has surgery more improved than in the dressings and cure of wounds and external ulcers. For how much more simple, and how much more efficacious too, are the light water-dressing and slight medicinal lotions of modern surgery than were the heavy and complex layers of ointments, lints, compresses, plasters, bandages, etc., which were almost universally used by practitioners thirty or forty years ago?

7. Further, it has been principally during the time to which these observations apply, that the attention of the surgeon has been directed to the causes which lead to death after operations, with a view of trying to avoid and avert these causes. No point has been better established in modern times, than the important fact that, after surgical operations, surgical patients do not die of surgical complications, as hemorrhage, mortification, tetanus, etc., but die in from eighty to ninety per cent. from blood-poisoning and internal inflammations, as pleuritis, pneumonia, peritonitis, etc. And I do believe at the present moment, that any individual in the profession, who, in surgery or in midwifery, could point out some means of curing, or some prophylactic means of averting by antecedent treatment the liability to these same identical diseases,—viz., surgical or puerperal fever,—such a fortunate individual would, I say, make, in relation to surgery and midwifery, a greater and more important discovery than could possibly be attained in any other subject of investigation. Nor does such a result seem hopelessly unattainable, particularly when we see the occurrence of the disease in question —namely, surgical and puerperal fever—regulated by some special, constitutional, and other circumstances which we may yet be able to detect and remove. Surgical fever, for instance, sometimes shows no tendency to occur, even after the most

severe forms of operation; whilst, in other individuals, it will occasionally spring up suddenly and fatally after the most trifling use of the knife or ligature. It will occur after almost all operations in one season or one hospital, and not after any operations in other seasons and other hospitals. And if its supervention is thus regulated and modified by various special circumstances, why may medical science not yet detect, and be able to control and regulate, these circumstances? Or if once the blood does become modified and altered, and the disease is actually established, why may we not hope for the discovery of means to eject the existing poison by some excretory organ, as the kidney, skin, lungs, etc.; or find even some direct antidote for the poison as it circulates in the blood itself?

Lastly, As one of the latest improvements in the art of surgery, and as one in which the members of this Society have taken no small interest, permit me to allude to the power which the surgeon now possesses of wrapping the patient in a painless anæsthetic sleep, while his body is being examined for painful injuries and diseases, or is being subjected to the cauteries, heated irons, scalpels, sutures, saws, and pulleys of the operator,—thus almost realising, in this artificial abrogation of pain and suffering, those dreams of the marvellous which the fervid and fresh fancy of our younger days was taught to ascribe to the enchantments of the Arabian Nights, and the spells and charms of bygone ages.

MEDICINE, as a practical art, has advanced during the last half century, perhaps as remarkably, or still more remarkably, than surgery. The modern physician has acquired a far more extended knowledge than his predecessor of the last generation had, of the nature and diagnosis of the various diseases to which the body is liable. And in proportion as his knowledge has become more certain and precise, so also can he apply his medicinal measures and weapons with a greater certainty for the relief and removal of them. Besides, at the present day, the medical man knows, I believe, far better than was ascertained formerly, the wonderful resources of nature in the cure of many diseases, inflammatory as well as febrile; acute as well as chronic. He knows more precisely where he should leave nature to her own unassisted efforts, and where he ought to strike in to aid her with the resources of art.

In illustration of how much practical medicine has latterly progressed, I might again refer to various of the important advances to which I have already alluded under the previous heads of pathological anatomy, physical diagnosis, and materia medica; and I might also dilate, if I had not already occupied your time too long, on many individual improvements in the practice of medicine, in relation to particular diseases,—as to the cure and banishment of scorbutus from our fleets; to the introduction in later times of iodine in the treatment of goitre and other affections; to the great and important changes that have taken place in the detection and treatment of the oxaluric, of the phosphatic, and other diatheses indicated by the state of the urine; to our increased knowledge of the nature, diagnosis, and treatment of the various local diseases of the head, chest, abdomen, and pelvis; to the increased certainty and success with which we can now adapt our curative treatment to the relief or removal of the numerous ailments complicated with, or originating in, typhus fever, scarlatina, albuminuria, rheumatism, gout, and other general diseases; and to the vast additions latterly made to our previous knowledge of the nature and unspeakable importance of various prophylactic, hygienic, and sanitary causes and measures. But let me content myself however with pointing more particularly to one or two special matters only, as illustrative of the fact that practical medicine, during the last fifty or sixty years, has made greater strides than practical surgery, or indeed than any other branch of practical knowledge whatever.

As an illustration, look for a moment at the whole class of infantile diseases. In the latter years of the last century, Dr William Hunter and the other leading English authorities in those days on such affections, believed and taught all infantile diseases to be the results either of fever, and to be treated with anti-febrile means, or the results of weakness, and to be treated with tonics and stimulants.

The pathological investigations, however, of the last half century, have proved that the different organs of the child are liable to nearly as numerous and diversified morbid actions as the same organs are in after life; that the same classes or types of diseased action occur in the system of the child as occur in the system of the adult;—that some maladies, particularly of an inflammatory character, as pneumonia, peritonitis, encephalitis, etc., which were supposed to be very rare in infancy, are now known, on the contrary, to be exceedingly common during that fragile period of life;—and that the medical management of infantile maladies must be varied as in the adult, according to the individual disease present, and cannot be conducted on the belief that all affections in infancy are amenable to one or two modes of general treatment. The hygiene and management of infancy have in an equal degree improved. And the whole changes in these respects have produced great changes in the mortality of the earlier years of life. Formerly, towards the middle of the last century, fifty or sixty out of every hundred children born in London, died before they had reached their fifth year; but the mortality has gradually and steadily diminished, so that now not above thirty or thirty-five in every hundred die at that early period. At the present time there are above 600,000 children born annually in Great Britain. According to the above scale of mortality, above 300,000 of these would have perished formerly before they were five years old, now only about 200,000 die during the first five years of life; thus showing a saving of human life in this item alone, to the extent of at least 100,000 human beings a-year.

Or, with a view of tracing the recent progress of medicine, let us consider for a moment another and very numerous class of ailments, the most afflicting of all that can fall upon man—I mean those included under the general term of insanity. Everywhere some fifty or sixty years ago, up to the end of the last century, and in too many places far, far on in the present century, the unhappy maniac had seldom ought else for his dark lot but chains and a dungeon, starvation and filth; and little or nothing else for his treatment but stripes and scorn, provocations and contumely. At the commencement of the present century, the poor lunatic, when once incarcerated in an asylum, seemed utterly and for ever cut off from the friendship and charity of his fellow-mortals; he was a man buried, as it were, while he was yet alive, a being deemed incapable of human feelings, and almost undeserving of human sympathy. Need I say how changed all this is now; or what the happy results have been since such poor sufferers were duly subjected to proper medical and moral management; and were treated as men still possessing the feelings, and impulses, and affections of man. If any one wishes for a test of the modern march and progress of practical medicine, he would find a strong and startling criterion of it in tracing the internal history of any asylum fifty years ago, and contrasting it with what asylums are now.

In this rapid survey, let us look for a moment at only one other advancement in practical medicine since the days of Watt—and no doubt the greatest of all. I allude, of course, to the discovery of vaccination.

The vast importance of it can only be appreciated when we venture to consider for a moment what would at the present day have been the dreadful state of mortality in our own and in other countries from small-pox, if no such discovery had been made. If small-pox were as fatal now, with our population of twenty millions, as it was during the latter period of the last half century, or before vaccination, it would destroy in Great Britain alone, some 80,000 lives a-year; for with a population of about eight millions, it then yearly produced a mortality amounting to 30,000 or 40,000 deaths. Physicians may proudly point to this single discovery alone, as a victory of medicine over disease and death unequalled in its greatness and importance by any of the wondrous discoveries that have been made through the whole past century in any of the physical arts or practical sciences. By itself alone vaccination has extended already the average sum of man's life upon earth some three or four years. Nor are the marvellously simple means

by which medicine has been able to accomplish this mighty end, less remarkable than the end itself. With an almost invisible speck of matter upon the tip end of his lancet, the medical man can in a great measure defy one of the most fatal, and at the same time one of the most loathsome, diseases that ever afflicted the human race. For even when it spared life, we must remember how often an attack of small-pox in former times spared not the eye-sight and the very features of the escaped patient. And indeed the more deeply any man considers the subject, the more will he venerate the name and discovery of JENNER, to whom was thus, as it were, entrusted the mighty and responsible office of shutting one of the great gates of human death. During the last half century, military science has, it is often averred, advanced greatly and permanently; that is, it has invented during that period much new and dreadful machinery, and many new and horrible missiles for the destruction of man and man's works. During the same period, in the discovery of vaccination alone, medicine has invented a means far more potent to save, than the soldier has invented means potent to destroy. If modern military science can boast of the discovery of wondrous shells and shrapnells, of destructive rockets and rifles, by which the deaths of thousands of men may be easily effected—modern medical science can boast of the discovery of a small vesicle, by the multiplied contents of which, the death of thousands and tens of thousands of men may be easily prevented. For truly, the millions of money expended in the vast military stores of Woolwich and Cherbourg lack the ability to destroy human life to any such degree as one drop of despised cow-pox matter, with its powers of multiplication in the system, has the ability to save it. The lancet of Jenner, has, during the last half century alone, saved in the world more human lives, than during the past or any other century in the history of man, warriors were ever yet successful in destroying. How often do we find the art of war described as a "noble and glorious art!" But when we consider in our hearts the relative effects of war and of medicine upon human life and human happiness, the physician may surely arrogate such a title for the art of medicine on infinitely better grounds than the soldier can claim it for the art of war.

The preceding observations refer to what medicine has done during the last fifty or sixty years,—to some of the principal steps which it has made,—to some of the principal advances which it has effected during that period. And I do think that such a glance at the past, however rapid and imperfect, is calculated to fill us with cheering and bright hopes for the future. Much has been done in medical and surgical science; but no doubt much, very much more, requires to be effected, and will be effected. Let me advert for a moment to one consideration as calculated to inspire our hearts with every confidence, that a continuation of earnest research and labour on the part of the profession will be rewarded with a continuation of success in the alleviation and removal of diseases.

It is now some two hundred years since Sydenham wrote and practised in London. Consider for a moment what this father of English medicine described as the most fatal diseases in England at that time. Six of the most destructive diseases, or classes of diseases, to human life in that age were, according to Short's mortality bill, plague, ague, dysentery, scurvy, child-birth, and small-pox. These maladies, however, enjoy their fatal pre-eminence no longer. We have banished the plague, which in the olden times often destroyed in London more lives than all other maladies counted together. We have rooted out and modified the ague, so that it is rarely a fatal affection now, while every year it destroyed thousands formerly. Scurvy has almost disappeared from our mortality bills. Dysentery, though still sometimes a fatal disease, is infinitely less common, and infinitely more under medical treatment, than it was at the time of Sydenham. At that date, or in the middle of the 17th century, about 1 in every 40 or 50 women delivered in London died of child-birth or its consequences; but gradually, as medical science has advanced, that mortality has decreased, till now not above 1 in 150 or 200 die. We have in Great Britain

about 600,000 deliveries annually, and still above 3000 of the mothers perish in child-birth. If the old mortality, however, of the 17th century yet held good, and this department of practice had not greatly progressed and improved, not less than 11,000 or 12,000 maternal lives would now be lost by the present proportion of annual births;—the advancements of modern science thus effecting in this item alone a saving of the lives of 7000 or 8000 mothers every year. And as for small-pox—another of the great sources of human death in the days of Sydenham—this fearful and most destructive malady has, as we have just stated, been almost entirely destroyed by the discovery of Jenner. These most formidable and fatal diseases of Sydenham's time sustain the pre-eminence of their formidable and fatal character no longer. And may we not hope that a couple of centuries hence, the very same fact may hold true of some of those diseases that are at present most destructive and deadly in their effects upon our population? Does not the history of the past suffice to encourage within us a bold belief that perhaps in half a century or a century hence, our present most fatal diseases may, by the advancement of hygienic and medical means, be our most fatal diseases no longer? At the present day we have, according to the registrars' official returns for England and Wales, consumption producing above 50,000 deaths a-year; convulsions above 22,000 deaths; pneumonia and bronchitis above 30,000 deaths a-year; typhus fever and scarlet fever generally above 20,000 deaths each; and measles, hooping-cough, rheumatism, diarrhœa, hydrocephalus, etc., destroying among us human life in less, but still in fearful, numbers. These several diseases stand at the present time as the highest on the roll of destructive agencies that prevail in British practice. For one, I confess that I cannot but entertain an ardent belief that medical means may yet devise measures, prophylactic perhaps rather than curative, to stay the great destruction of human life prevailing amongst us from the first, for example, of these affections, phthisis. Perhaps a more advanced pathology and chemistry may yet ere long furnish us with more enlightened and practical views of pneumonia and other inflammatory disorders than we yet possess, and arm us with more sure and potent medicinal weapons and resources against them. We have, from the experience of the last few years, every reason to hope that the whole class of zymotic diseases will be greatly subdued betimes in intensity and violence, when the investigation of the physical causes predisposing to them, or even actually exciting them, is more fully expiscated. If medicine has devised means to arrest the ravages of small-pox, may it not yet devise some means also, by inoculation or otherwise, to arrest the ravages of scarlet fever and measles, of hooping-cough, of typhus fever, and perhaps of the whole class of non-recurrent diseases? And even if we fail to arrest them, we may possibly find out for the varying animal poisons producing these diseases, antidotes as certain as quinine is against the poison of marsh fever. Let us at least not sit indolently down and argue ourselves into the belief that it is impossible to attain such results. The conquest of small-pox seemed to our forefathers a hundred years ago as impossible as the conquest of these maladies can look to any one now; and yet we all know that the subjugation of small-pox was effected by the genius of one man and the devotion of one mind to its accomplishment. Some time before Jenner turned his attention to the subject, the learned and accomplished Dr Mead —the first London physician of his day—wrote of the utter hopelessness of the very idea of battling with and vanquishing such a formidable enemy to human life and happiness as small-pox. He speaks of the possibility of a "specific antidote being found against the contagious poison" of small-pox—that is, an antidote "by which it may be so thoroughly destroyed that though it had been received into the body, it may not produce the disease," as an idea as wild and chimerical as that of alchemy; and one, in his opinion, outraging "the principles and elements of things that are so certain and so well established by the permanent laws of nature."[1] These disheartening opinions of Dr Mead

[1] See his Medical Works, vol. ii. p. 131 (1763).

regarding the hopelessness of ever gaining a prophylactic for small-pox, were published in 1747. Before the end of the century, however, Jenner had both discovered, and successfully adapted to practice, the great prophylactic measure that has so justly rendered his name imperishable in the annals of the human race. Thus the happy results of Jenner's labours descend to us stamped with a precious lesson from him of hope and perseverance under alleged impossibilities; and these results preach loudly to the profession in all time to come, that we should never permit any apparent improbabilities to prevent us from going ever onwards in earnest search for possible means to cure and conquer diseases even of the most formidable and fatal kind.

In conclusion, let me add one word more. I have hitherto spoken of associations of the profession, like that of the Medico-Chirurgical Society, as principally calculated to advance medical observation amongst us, and to promote the general extension of medical science. There is, however, another high and important advantage belonging to such associations as those formed by this Society,—another benefit flowing from their construction. For such associations are not only destined to spread professional zeal and information among their constituent members; they are destined also to promote professional goodwill and genuine brotherhood amongst them. They are intended to cultivate the heart, as well as to cultivate the head;—to increase and deepen our feeling of professional fraternity and kindness, as well as increase and extend our stores of professional acquirements and knowledge. And during the time that I may have the pleasure and the honour of occupying the Chair to which you have elected me, I shall ever endeavour to use my influence in it, so that while every freedom is given to professional debate, there shall, if possible, be nothing uttered that shall unnecessarily hurt the feelings of any member of the profession, present or absent. And I feel sure that in endeavouring to attain this end I shall ever meet with your hearty concurrence and aid.

Part Fourth.

PERISCOPE.

APPLICATION OF THE EYE-SPECULUM TO THE DIAGNOSIS OF EYE-DISEASES. BY F. C. DONDERS OF UTRECHT.

(*Translated by the Editor from the Nederlandsch Lancet—Derde Serie—Eerste Jaargang. Bl.* 739, *etc.*)

IN a former Number of this periodical (October 1851) I gave a simple account of a small work by Helmholtz, entitled "Beschreibung eines Augenspiegels,"[1] assuredly without expecting that it would appear a few months afterwards in another language, among the original articles in the "Annales d'Oculistique." Whilst I was waiting for an eye-speculum to be made for me, by Rekos at Königsberg, under the direction of Helmholtz, Epkens, the instrument maker at Amsterdam, guided by my account of the apparatus, endeavoured to construct it in a considerably improved form. I saw his alterations with much interest, but reserved my opinion as to their utility, till I

[1] Description of an Eye-Speculum. An abridged account of Helmholtz's discovery was published by Dr Saunders, in this Journal, No. CXXXIX., 1852, July, p. 40 —Ed. of Med. Journal.

should have an opportunity of comparing Epkens' speculum with one of the original model. When this was done, I found that Epkens' speculum was in many points preferable, and likely, in his hands, to be still further improved. Instead of the parallel glass-plates, inclined at the polarising angle, Epkens employs as a reflector common mirror glass, inclined at a somewhat variable angle, and from the central portion of which the metallic coating is removed, so that a clear circular space of three lines in diameter is left. By this means the reflection is made much more powerful; and as the eye under examination is close to the mirror, the rays are so cast back from the strongly glancing metallic surface of the glass, that they are thrown upon the portion of the retina which we desire to inspect. For, indeed, although the less amount of reflection at the centre of the glass is, at some distance, distinctly appreciable, the eye under examination cannot perceive this at all, when it is situated at the proper distance from the speculum—a proof that the retina, even immediately behind the centre of the mirror, is illuminated in a sufficiently uniform manner by the diffuse-image of the reflected light.

A second alteration consists in combining with the speculum a cylindrical tube of 15 centimetres, = 6 English inches, long, by 3·5 centimetres, = 1·4 English inches, wide. To the outer end of this is fixed the lamp, whose rays are to reach the mirror through the tube; while a large thin black disc is attached to the body of the cylinder, screening the eye both of the observer and individual under examination, from any direct rays from the source of light. In the tube a lens may also be placed to magnify the reflected image.

In the last speculum made by Rekos, there was a disc, having the form of a diaphragm with eccentric axis, carrying near its margin four common lenses of different powers, which, when the disc was turned, might be alternately adapted to the observer's eye. The lens best suited for distinct vision can be thus easily selected, while in operating with the original instrument, it was necessary to fit one lens after another into the eye-piece, in order to make a choice. This improvement likewise has been adopted by Epkens.

Finally, and to this point we attach much importance—the whole apparatus is fixed to a stand, which can be screwed at the requisite height to a table.

We found, then, Epkens' speculum preferable, from its more strongly illuminating the retina; while, in order to conduct an observation, it was not necessary to exclude all daylight from the chamber, the reflected image on the cornea being hardly more an obstacle than when Helmholtz's speculum was used. But, above all, it appeared to us, that the fixing of the whole apparatus, including the lamp, was a great improvement. For we found it excessively difficult to manage Helmholtz's speculum with the hand, and only now and then obtained with it a momentary view of the retina. We willingly attribute this to our own want of dexterity; and, indeed, we have the proof in what Helmholtz has seen, that the difficulties connected with the use of the instrument in its first form, may, by repeated trials, be overcome. But an instrument is certainly preferable, which can be used by those who have less of practice and dexterity; and, in this point of view, Epkens' instrument assuredly excels. It is true that, with the fixed apparatus of Epkens, the movements of the eye necessary for the successive exploration of different parts of the retina cannot be followed; but there is no difficulty whatever, when from looking too much upwards, downwards, or sidewards, the pupil is not placed immediately behind the centre of the reflector, by gently moving the patient's head with the hand, in bringing it into the requisite position. The examination may be continued for the time that may be necessary, without any uneasiness for the eye of observer or patient, for Epkens has conveniently obviated the disagreeable propinquity of the faces (*bouche-à-bouche*) by the interposition of a velvet flap hanging from the cylinder.

We have already examined many eyes with this apparatus, and remarked,

that the brilliancy of the reflection in pupils of like width, varies considerably in different individuals. It is not proposed to enter at present upon a description of all these physiological differences. We wish this communication to be regarded as a preliminary one, written solely with the view of showing that Helmholtz has not indulged too visionary a prospect as to the diagnostic value of his apparatus, and that he has conferred a real service no less upon ophthalmic surgery than upon physiology.

One of my first cases was a man of thirty-six years of age, who had, a short time before, arrived here from Surinam, and who, while there, had three years ago remarked one morning, that straight objects appeared to his right eye crooked. Insensibility of about two-thirds of the outer and lower part of the retina had gradually followed, the central part corresponding to the axis of vision being included in the diseased condition. He could thus only somewhat indirectly distinguish objects placed below and to the outside of the eye, images from this quarter being of course projected upon the sound portion of the retina. As the direction of the axis of vision was somewhat disturbed, there was also this peculiarity in the case, that in the course of three years the patient had not learned to judge of distance quickly and correctly by means of the *left* eye, although it was neither long nor short-sighted, and possessed considerable power of accommodation. If he wished to fill a wine glass, he commonly poured the liquid to its near or far side; and, when shooting, sometimes shouldered his gun at a passing insect, taking it for a bird.

After the application of belladonna, the ordinary mode of examination revealed nothing. I then, in company with my colleague, Van Goudoever, examined the eye with the speculum. We both perceived, with perfect distinctness, at the side of the blood-vessels, one or more dark, almost black, blotches, which, from their somewhat red glance, appeared to be blood-coagula. Further, over the whole diseased portion of retina, the reflection was exceedingly irregular,—here bright white,—and there again so weak, that *little* except the large blood-vessels could be distinguished,—there were besides a number of rough parts, interspersed with unmistakeably lighter and more shadowy points. This examination accordingly quite confirmed our suspicion, that a limited effusion of blood between the choroid and retina, or in the latter membrane itself, had been the cause of the sudden distortion of objects (which must necessarily have been the consequence of displacement of the parts of the retina); and that a chronic inflammation had followed, and extended over two-thirds of the retina.

A second case was occasioned by a wound from a sharp arrow, received many years before, on the inner side of the eyeball, and which had probably penetrated deep enough directly to implicate the retina. Blindness of this eye had, in the course of one or two years, followed; and there only remained some slight power of vision in a small portion of the retina. Here we first found a place where the black pigment was quite uncovered, and surrounded with a white, strongly reflecting margin; then, immediately outside the entrance of the optic nerve, we distinguished an almost black arch, shaped like a positive[1] meniscus, and which might have been regarded as a tear in the retina, had *not* two blood-vessels belonging to the membrane been distinctly seen stretched across the dark space. Helmholtz asserts that he usually distinguished a pale shadow, probably derived from the "plica semilunaris." We have likewise observed something of the sort in sound eyes, but nothing which, in point of darkness, could at all be compared with what was seen in this case. Besides the arch was wanting in the same patient's sound eye. Further, the blood-vessels seemed here and there to end as if cut short; and some of them were alternately wider and narrower, which we never have observed to be the case in sound eyes. Finally, the reflection from the tissue between the larger

[1] A meniscus, as distinguished from a concavo-convex lens.—(Trans.)

vessels was unequal, but not in a degree which could have authorised any conclusion from this circumstance alone, as to a morbid condition.

We could add here various cases, but content ourselves in the meantime with these two,—the most remarkable which we have as yet met with, our sole object at present being to illustrate the practical use which may be made of the eye-speculum.

MODE OF DETERMINING THE OPTICAL POWER OF A MICROSCOPE. BY PROFESSOR HARTING OF UTRECHT.

(*Translated by the Editor from Het Mikroskoop*, vol. i. p. 407.)

THE optical power of a microscope may be said to consist of three qualities, viz., magnifying power, defining power, and penetrating power. Although the first is the quality to which most importance is generally attributed, the practised observer well knows that it is of far less consequence than the second and third. And although there are a variety of simple means by which the magnifying powers of microscopes may be ascertained and compared, it is to be regretted that science has not yet suggested any easy and infallible mode of testing their powers of definition and penetration. *Definition* appears mainly to depend upon the amount of exactitude with which the spherical and chromatic aberrations of various parts of the instrument are balanced and corrected. *Penetration* chiefly depends upon the "angle of aperture" of the object-glass, *i. e.* upon the *angular width* of the luminous pencils passing from each point of the object through the lens. The brightness of the image increases as the square of the diameter of the lens or mirror which produces it; hence it is obvious that of two lenses of equal focal length and refracting power, that which is the wider and can transmit the broader pencil of rays, will give the brighter image. If the effective diameters of two such lenses be = 1 and 3, there will be *nine* times as much light in the image formed by the second as in the image formed by the first. Of course there will be, in the image formed by the second lens, parts clearly visible, which in the image formed by the first lens, were too feebly illuminated to affect the retina of the observer. When the aberrations are perfectly corrected, *i. e.* when the rays passing from the object through the centre and margins of the lens are refracted without chromatic dispersion and to the same foci, the maximum of defining power is attained, and the penetrating power of the lens is likewise a maximum for that angle of aperture. It does not, however, follow that a lens of greater angular aperture is not superior in penetration, nor that a lens of exquisite penetrating power, *i. e.* capable of exhibiting the most feebly illuminated parts of an image, must necessarily be one of good defining power, *i. e.* capable of exhibiting sharp outlines on bodies of extreme minuteness, or of resolving or separating the dots, stripes, etc., of the more difficult test-objects. With these preliminary observations, which may be regarded as an imperfect abridgment of different chapters in Harting's excellent treatise, we proceed to lay before our readers his very ingenious and beautiful method for testing the qualities of definition and penetrating power.—*Editor*.

I conclude by noticing another method of testing the optical power of the instrument, which, although rather troublesome, appears to me among the best, permitting us, as it does, to ascertain with a great degree of accuracy and certainty, the utmost limits of penetrating and separating power possessed by a microscope, and hence easily to express numerically its optical qualities in the most varied circumstances.

This method consists simply in subjecting to observation under the microscope the dioptric images of certain minute objects instead of the objects themselves. These images can be diminished at pleasure by withdrawing to a distance from the lens the object which forms them; and hence we have it in our power to measure the extreme limits at which the object continues to be visible.

For the formation of the dioptric images achromatic object-glasses might be used; but even where those of the shortest focal length are employed, the object whose image it is required to form must be placed at a great distance. This would cause various difficulties, and only be practicable with a microscope placed horizontally—unless, indeed, the object selected were very minute, in which case the accurate determination of its diameter (from which that of its image must be afterwards deduced) would be rendered difficult.

Small air-bells in a fluid are for this purpose far better. I employ by preference a watery solution of powdered gum arabic, which always contains numbers of such air-bells originating in the air entangled among the particles of the powder. The water employed should have stood for a considerable time freely exposed to the air, or been shaken up with air for some time; for when we use water which is not saturated with air, the bubbles in the fluid gradually become smaller, and images formed in them decreasing in magnitude, cause errors in the subsequent measurements, as we shall actually find to be the case.

A drop of the fluid must then be placed on a clean glass object-slide, and covered with a good clear mica plate, a ring-shaped piece of paper being interposed, in order to prevent the flattening of the air-bells by pressure. The object-slide is then placed under the object-glass upon the stage of the microscope, and an air-bell of suitable size for the formation of the images is sought for. All do not give images of the same degree of sharpness; a peculiarity dependent on the fact that some air-bells are in contact with the covering-plate, and consequently have their spherical form disturbed to some extent, or on the presence of small molecules in the fluid above or beneath the air-bell, or even in its interior, causing some haziness of the image, just as defective polish of a glass lens would do. It will, however, be always easy to find some [1] which will form images of the utmost distinctness and purity. This may be ascertained in the first instance by holding between the mirror and stage some easily recognised object, *e. g.* a piece of paper or the like. The image is always formed on the under surface of the air-bell, which must consequently be brought nearer to the object-glass than when it is desired to bring its margins into focus.

The object whose image is to be the subject of examination should be placed upon an apparatus, which can be moved upwards and downwards in the space between the mirror and stage. In some microscopes this can hardly be done, either from the space being too limited, or in consequence of the drum-like form of the foot of the microscope which quite envelops the space. If such microscopes, in place of a mirror, be provided with a reflecting prism, the object may be placed opposite the side external to the microscope. The instruments best adapted for the manipulation which we are describing are, however, those whose illuminating apparatus consists of a mirror and converging lens, which can be shifted up or down. The lens being removed from the ring which supports it, the object is substituted in its place. The relative magnitudes of object and air-bell must be such that the image shall be exceedingly minute when the object is tolerably near to the stage. On afterwards increasing the distance between the object and air-bell, it is not difficult to find the limit at which the image (under a given magnifying power) is barely visible.

Of course it is impossible to measure *directly* the dimensions of this most minute visible image, for our best micrometric methods will here be found of no avail. Yet their size may be estimated with extreme accuracy in the following manner. At the same distance from the air-bell and in place of the object used, substitute another body, such as a piece of card, of 4 to 5 centimetres = 1½ths to 2 inches diameter, which has been exactly measured. Let this be now

[1] The following example will demonstrate this. I brought a printed page of a book to such a distance from an air-bell that the length of the image of the whole page was $\frac{1}{4}$th millimetre = about $\frac{1}{100}$th of an inch, and that of the image of each letter about $\frac{1}{250}$th millim. = $\frac{1}{10000}$th of an inch. In spite of their minuteness, these images, formed by reflected light, possessed such clearness and sharpness, that under a magnifying power of 154 diameters, the whole page was, without difficulty, legible.

again measured (by some of the micrometric methods elsewhere alluded to[1]), just as if it were a real object. By dividing the real diameter by the apparent diameter, the amount of diminution is found; and this is the same for all objects at a like distance from the air-bell. We have, consequently, nothing to do, in order to find the amount of diminution of the image of the more minute object, but to divide its *true* diameter by the figure expressing the diminishing power.

For example, let the true diameter of the greater object be 5 centimetres = to 1·969 English inches, and the diameter of its image = 32·2 micromillimetres,[2] = ·00127 English inches, then the figure expressing the amount of diminution will be $\frac{1\cdot969}{\cdot00127} = 1553$ very nearly. If now the smaller object have a diameter of 175 micromillimeters = ·00689 English inches, then must its image at the limit of vision be in diameter = $\frac{\cdot00689}{1553} = \cdot0000044$, or about $\frac{1}{226.000}$th of an English inch. When exact micrometric methods are employed, it is easy in this way to estimate the diameter of an image even to millionth parts of a millimetre, *i. e.* to 25,400,000th parts of an inch.

As for the object suitable for these investigations, it is plain that we have an extensive choice. To find the limit of vision for bodies of a round or long thread-like form, grains of pearl sago, or vegetable bodies, such as mustard-seed or the pollen-granules of many plants, hairs of animals, metallic wires, etc., may be employed. Small round openings and chinks may serve for the determination of the visibility of positive images of light. In the last case care must of course be taken, by means of suitable screens, to shut off all light except what passes through the aperture. To determine the defining power, metallic wire-gauze is a suitable object, or two holes placed near each other in a black metallic plate. The images of such objects resemble exactly a double star viewed through a telescope (kijker). The bodies may likewise be placed in different circumstances in order to ascertain the influence of these upon the limits of vision. Thus we may use as an object a very thin glass capillary-tube placed in water, and compare it with tender organic-tubes and vessels, which may also be seen in water, but whose limit of visibility is of course far more circumscribed than that of absolutely opaque objects.

In fact this method admits of innumerable variations, and is consequently of most extensive application. Besides, when proper precautions are taken, it gives results perfectly sure and comparable. Especial care is however requisite in the mode of illumination. For it is certain, that when the field has a clear white ground, the contrast causes minute opaque bodies (*i. e.* objects which are dark by transmitted light) to continue visible, which against a grayish or light-blue back-ground could not be seen. Hence it is by no means indifferent to receive on the mirror light from a white cloud, from a dull overcast, or clear blue sky. Artificial light cannot be used in these experiments, for the image of the flame becomes diminished like the object, and hence a clear field of view is not to be obtained. The observations must consequently be made by daylight; and whenever comparable results are sought for, the mirror should always be directed to the clear, blue, cloudless sky—this being a distinct atmospheric condition to which others in similar circumstances may refer in conducting the same experiment. The mode of ascertaining the limit of vision, with a given amount of illumination, may be gathered from different examples in the body of this work.[3] It will likewise be found that for all such observations, even when the highest magnifying powers are employed, the *flat* mirror is perfectly sufficient, since in the image in the field of view formed by the air-bell, all the rays proceeding from the mirror are united and constitute an object of considerable luminous intensity.

[1] See Translation from *Het Mikroskoop* in Monthly Journal of Medical Science, June 1852, p. 453, *et seq.*

[2] The micromillimetre is equal $\frac{1}{10000}$ millimetres = ·0000394 English inches. See *Monthly Journal*, June 1852, p. 456.

[3] It is unnecessary to introduce any examples here, the author's description of his method being both full and suggestive.—EDITOR.

Part Fifth.

MEDICAL NEWS.

EDINBURGH MEDICO-CHIRURGICAL SOCIETY.

SESSION XXXII.

MEETING V.—*Wednesday, 2d February*, 1853.—Dr SIMPSON, P., in the Chair.

Dr Bennett read a communication on

THE CURATIVE TREATMENT OF PHTHISIS PULMONALIS, AND THE *modus operandi* OF COD-LIVER OIL.

Dr Bennett commenced by reading the remarks of Dr Christison, at a *previous* meeting of the Society (Meeting V.), upon the treatment of phthisis pulmonalis; and said that he thought these remarks did not do justice to the efforts that had been made in this city since 1841, to place the treatment of this affection on a *rational* foundation, by the introduction of new principles and new resources. He was satisfied, notwithstanding the statement of Dr Christison to the contrary, that the treatment of phthisis in the Royal Infirmary, and in Edinburgh *generally*, was not conducted, previously to the introduction of cod-liver oil, on the principle of treating the digestive rather than the pulmonary system, and introducing proper elements of nutrition; much less were practitioners in the habit of *looking* to the true pathology of the disease, and of regarding the introduction of oil into the system as one of the primary indications of cure. He had looked into the journals of the Infirmary relating to the period in question, and found that in comparatively few cases was a nourishing diet ordered. On the contrary, the general treatment consisted of a few leeches frequently applied to the chest, antimonials, cough mixtures, and opiates, sulphuric acid to relieve the sweating, occasionally counter-irritants, and towards the termination of the disease wine and stimulants. The introduction of the tonic regimen in phthisis had been ascribed to Dr Stewart, of Erskine; but he had good grounds for stating that Dr Stewart's treatment had not been generally recognised by the profession, and that it was founded, not on any pathological principles, but upon an empirical *basis*. Dr Bennett then entered at considerable length into an exposition of those facts which, in his opinion, proved the curability of tubercle. He considered its pathology to be, first, a derangement of nutrition, dependent on an excess of the albuminous, and a deficiency of the oily constituents of the chyle; secondly, an impaired constitution of the blood; so that, thirdly, when exudations occurred, they assumed the form of tubercle. He then stated that, in conformity with these views, he considered that the treatment of phthisis should be directed exclusively to the general system, and particularly to the correction of the impaired digestion and assimilation, while the local disease of the lung should in most cases be left to itself. The rule of practice he thought ought to be, an endeavour so to act on the digestive system as to improve the quality of the blood. This was to be accomplished, not only by giving animal oil (which, however, as the principle of nutrition that was deficient was directly indicated), but by attention to those circumstances which correct acidity in the alimentary canal, and stimulate all the nutritive processes, such as exercise, bathing, proper diet, mental occupa-

tions, etc, etc. He considered the ordinary palliative medicines as useless, and even injurious, because they frequently interfered with the main objects of cure, —the correction of the acidity in the intestinal canal, and the introduction into the system of the normal products of assimilation. Beef-steaks and mutton-chops were all very well so far as they went, but experience had proved that in the majority of cases they could not be digested. By giving cod-liver oil we saved the stomach, as it were, the trouble of extracting and reducing fluid fat from the food, and, although a considerable quantity of it was not assimilated, a sufficiency was absorbed to increase the molecular basis of the chyle, and to give a stimulus to the better elaboration of blood. It was with this view that he administered cod-liver oil; and since its introduction into British practice, he had had the satisfaction to see it extensively adopted by practitioners in this country, some of whom, as, for instance, Dr C. J. B. Williams, of London, had published theories as to the action of this remedy, identical in all essential particulars with the one advanced by him (Dr B.). He then criticised at some length the remedies in common use, and pointed out that such as were beneficial operated by stimulating the nutritive functions. The good effects of change of climate he thought were to be entirely explained in this way, and not to any supposed action on the lungs. He concluded by announcing his intention of shortly laying his views and the result of his experience more at length before the profession in a separate publication.

Previously to the discussion of Dr Bennett's paper,

Dr Simpson read a communication "On the Possible Prevention of Consumption and Scrofula by Oil-Inunction and Inhalation." This communication was founded chiefly on evidence collected by Dr Simpson from various parts of the country, to the effect, that the children and young persons employed in wool factories (in which large quantities of oil are daily used), were unusually exempt from scrofulous diseases and from pulmonary consumption. Dr S. considered this immunity to be probably owing to the oily particles which, floating in the atmosphere, or attached to the raw material, entered the system by the lungs and the skin.

A discussion followed these two communications, in which

Mr Syme remarked that, in regard to the production of scrofula, however much or little might be due to the influence of a hereditary taint, there could be no doubt as to the fact, that individual weakness of constitution, aggravated by defective diet, clothing, or climate, was sufficient, not only to increase, but to cause, the symptoms; that, therefore, the great object of protection and curative treatment should be invigoration of the system; and that as cod-liver oil is the most powerful means hitherto employed for this purpose, great credit was due to Dr Bennett for its introduction into use; but that Dr B. could not claim the establishment of any new principle of practice in this respect.

Dr Christison observed, that among the many important topics embraced by Dr Bennett's paper, there were three primary and distinct questions which might be separately discussed by the Society. One related to Dr Bennett's theory of phthisis; another to the degree of credit due to him for the introduction of cod-liver oil in the treatment of that disease; and the third to the knowledge possessed generally by the medical profession in Edinburgh, prior to his writings, as to the treatment of phthisis by the tonic or invigorating method of cure. Dr Christison stated, that he should confine himself entirely to the last of these questions. Dr Bennett's theory of the disease was a very pretty theory as it stands; and it was to be hoped, for Dr Bennett's sake, that it would stand; but, for his own part, he believed that the practical good resulting from it would remain long after the theory itself was forgotten. As for the first introduction of cod-liver oil into general practice in this country, he had been anticipated in a few observations he had intended to make by the remarks of Professor Syme; so that he had only to express his concurrence with that gentleman's statements, and to add the indignation with which he had often perused accounts in the English medical journals of the applications of cod-liver oil to the treatment of

phthisis and other scrofulous diseases, without any acknowledgment of the obligations due to Dr Bennett for having first introduced it.

But the general tonic or invigorating treatment of phthisis—namely, by generous diet, animal food, wine, malt liquors, and exercise in the open air—was quite a different matter from the treatment by cod-liver oil. And if Dr Bennett still claimed, as he had done a few weeks ago in this Society, the general introduction of that method of cure into medical practice in Edinburgh, he claimed what was not due to him. He was unfortunately prevented, by the infidelity of a railway train, from being present at the commencement of the paper, when that topic was adverted to. But if the claim was still maintained, it was necessary for him, in justice to the medical profession of Edinburgh, to show that they had not been so ignorant of the proper treatment of pulmonary consumption as such a claim would imply.

Now, so far from the tonic treatment of phthisis being new in Edinburgh, it was taught even in the latter years of the late Dr Gregory of this University, as he could prove by the following extracts from notes of that eminent professor's lectures, taken by himself when a pupil of the class in 1817. "I am convinced," said Dr Gregory, "that the common mode of dieting patients who are consumptive is inefficacious; I mean the restricting of them to cool, spare diet, and denying them the use of animal food and fermented liquors. The old practice of limiting them to a milk diet has been sufficiently tried, in order to prove that phthisis cannot be cured by it. . . . . It has been found that when a very low diet has been urged, the patient becomes worse, instead of better, his debility becomes greater, and the hectic fever is aggravated. A deviation from the spare regimen has been tried with success. I have tried that plan several times. One of my patients recovered, beyond all expectation, when we changed his milk diet for animal food. I have tried it often in my public practice in the hospital, often with benefit, seldom so as to effect a cure. It very generally retards the progress of the symptoms." These sentiments of Dr Gregory were very possibly dictated by what he had observed of the effects of the system of cure followed for some years previously by Dr Stewart; for it is to that gentleman that we are indebted, in Scotland at least, for the general adoption of the invigorating treatment of phthisis.

Dr Christison went on to state, that he had been familiar with Dr Stewart's system before he was even a medical student, as it made a great noise in general society, and was often the subject of conversation between himself and his father, who was well acquainted with Dr Stewart. In consequence, his attention was earnestly turned to it afterwards, both during his medical studies, and after graduation. He knew that it was put in force by many practitioners in Edinburgh soon after 1821 at latest. And he certainly had laboured until now under the firm conviction, that the invigorating treatment by generous diet and exercise in the open air was the accredited treatment in Edinburgh from that time. In proof that this was no new idea of his, he begged to be allowed to read an extract from his lectures on Materia Medica, as they were delivered first in November 1832, and as they have been annually delivered, with some trifling deviations, ever since.

Under the head of Spare Diet, in the branch of Dietetics, his original lecture of 1832 goes on thus:—"Doubts have existed as to the proper dietetic treatment of phthisis; and it is only in recent times that grave errors have been rectified.

"The greater part of physicians in this country, down nearly to the close of last century, treated consumption on the supposition that it was an inflammatory disorder; and, as a matter of course, resorted to spare diet as one of the remedial measures. I need not say that their efforts were hardly ever crowned with success. Some empirical practitioners, therefore, began to try an opposite system. Instead of confining the patient to the house, and to meagre diet, invigorating animal food was given; commonly wine also; and exercise in the open air was enjoined, to a great extent, especially exercise by gestation. Under this

method of treatment cures were reported. And it was in particular brought into notoriety by the alleged success of one of the first persons who practised it,—a clergyman of the Scottish Church, who had taken a medical degree when a student, and who obtained much reputation by curing, as was believed, first a member of his own family, and subsequently several individuals of rank, but especially a daughter of the then Duke of Northumberland. This gentleman, Dr Stewart of Luss (and afterwards of Erskine). soon acquired such celebrity by his apparent cures, that he was consulted from all quarters of Great Britain, even from the south of England.

"Dr Stewart's system was soon adopted by others, and at first seemed equally successful in their hands as in his. But when at length it came to be extensively tried, the proportion of failures proved much greater than at first; gradually it appeared to fail as often as any other remedy; and now this method of cure, like other novelties in the treatment of consumption, has sunk into oblivion. It has not disappeared, however, without leaving behind a salutary change in the treatment of this melancholy disorder. Physicians are now fully aware of the necessity of abating greatly the rigour of the former antiphlogistic method of treating consumption.

"In the present time we have made such advances in the knowledge of the pathology and symptoms of diseases of the chest—compared with what was possessed when Dr Stewart's practice came into vogue—that we are able to account for its apparent success. In the early stage of pulmonary consumption, it is impossible to form a trustworthy opinion as to the real existence of the disease; because the symptoms are in themselves vague and obscure, and, in respect of other diseases, are undistinguishable from some forms of chlorosis or of protracted catarrh. But above all, pulmonary consumption, in its most advanced stage and most characteristic form, is undistinguishable from some severe cases of chronic catarrh, except by the method of diagnosis with which the physician has been furnished only in recent times by percussion and the stethoscope. I have myself met with several instances of recovery from pulmonary catarrh, and every physician who extensively uses the stethoscope must meet with the like, where the general symptoms were precisely those of the worst forms of pulmonary consumption, and which would to a certainty have been considered recoveries from that disease in its confirmed form by any one who, like Dr Stewart and his contemporaries, was unacquainted with the improved diagnosis introduced into medical practice by Laennec. As incidents of the kind must have occurred much more frequently before the stethoscope was known, I have no doubt that many of the supposed cures of consumption were really cures of pulmonary catarrh.

"But while I make this statement, let it not be supposed I am inclined to maintain that consumption never ends in recovery, or has not been occasionally cured by the system recommended by Dr Stewart. The researches of many late pathologists have discovered cicatrices in the lungs; and I have myself met with instances of the kind. This clearly proves that cavities had once existed, and been filled up. The natural inference therefore, is, that in some instances pulmonary consumption admits of cure.

"As to the proper means, so far as regards diet," etc.

Dr Christison concluded with observing, that he here laid down, not his own opinion merely, but what he understood to be the prevailing practice in Edinburgh for some time before that lecture was written. And it surely must be allowed that he could scarcely be mistaken as to this, when at that time he lived in constant professional and friendly intercourse with almost every medical practitioner of note in the city. Dr Bennett had endeavoured to prove the reverse, by reference to the journals of the Infirmary, in which he found that, with the exception of the journals of Dr Christison himself, there was no mention made of generous diet in the prescriptions for phthisical patients. But the Infirmary reports were not the best authority on this subject. The hospital case-books were good for what was contained in them, but not for what was not contained

in them. It was well known—and he spoke under correction of many gentlemen now present, who had acted both as resident and clinical clerks—that it was a very general practice, not to insert the diets in the reports of the cases, but in a separate diet-table.

Much more might be said as to the introduction of the tonic method of cure of phthisis. But he wished to restrict himself absolutely to the question raised not long ago in this Society, and to which the paper of Dr Bennett was understood to be owing—namely, whether he had introduced the treatment in question into medical practice in Edinburgh. Upon this point he hoped no doubt could now exist.

Dr Macaulay read a letter from Dr Stewart, dated June 1818, in which he gave directions for the treatment of a consumptive patient, who had been previously seen by him (Dr M.). The case was too far advanced to admit of the tonic plan being pushed to any great extent, but it was evidently urged as far as possible in the advice given by Dr Stewart. Dr Macaulay stated that his own practice at the time would have been very little different from that suggested in this case; and as Dr Christison had been taught by Dr Gregory on similar principles in 1817, the Society would probably be inclined to think that the tonic plan for the treatment of phthisis originated a great deal earlier than 1841, as contended for by Dr Bennett. Dr Macaulay mentioned another case which had occurred about the same time, 1818, of a gentleman who, having lost all his brothers and sisters in early life by consumption, resolved to allow himself a more generous diet and regimen than they had been subjected to; and this he did with complete success, having attained to a good old age.

Dr Warburton Begbie referred to the plan of treatment adopted in phthisis, and the views entertained of its nature by some of the older writers, particularly Borelli and Christopher Bennet, who flourished in the seventeenth century. These physicians advocated and practised the tonic system of treatment, and so did many others. (Passages in corroboration were read from both authors.) He wished the Society to know that the original introduction of this plan of treatment was not claimed for his father by Dr A. P. Stewart. The late Dr Stewart, of Erskine, was well acquainted with the writings and opinions of the older authors; and what he had done, and deserved credit for, was carrying out their principles, with some minor modifications, of which, as of the treatment itself, he had been an able advocate.

Dr Seller said, with reference to Dr Young's work, from which Dr W. Begbie had produced the views of Dr Christopher Bennet, in the seventeenth century, that that book was a complete index to the historical part of tubercular phthisis; that if Dr Thomas Young had not bestowed much of the genius which he so undeniably possessed, on his original profession, he had laid that profession under signal obligations, by this and other monuments of his great industry. If Dr Begbie had read that book through, he was sure he would admit there were sufficient proofs that Dr Bennett was right when he said that the rule of treatment and regimen in phthisis was in past times antiphlogistic; and that Dr Christopher Bennet's views, with those of some others, were rare exceptions to that rule. The rule as to food in phthisis was in past times non-stimulating diet. Milk was its type—women's milk, goats' milk, asses' milk—from Hippocrates downwards. It could not be pretended that cod-liver oil was superior to milk as diet; it was in truth not diet at all, but a remedy, by the help of which, in cases of limited tuberculosis, a diet sufficiently nutritive sometimes to suspend or stop short the disease could be introduced. Dr Bennett's merit was having brought into use that remedy in the treatment of phthisis in this country.

Dr W. Begbie could not agree with Dr Seller in regarding the views of Dr Christopher Bennet as singular or exceptional.

Dr Taylor said he would not trouble the Society at that late hour with any remarks of his own, but would confine himself to reading one or two extracts. Before doing so, however, he begged to state he perfectly concurred in all that had been said by Dr Christison and others, in regard to the merit due to Dr

Bennett for his introduction of cod-liver oil into practice in this country; and for this full justice had not perhaps been done him. The first extract he had to read was from the preface to Dr Thomas Reid's work on "Phthisis," published in 1798. "In this momentous period," he says, "of general innovation and change, or in the insidious cant of the times of reformation, several novelties have been attempted in the cure of phthisis pulmonalis; but it will be shown in the progress of this work, that wine, bark, and animal food—part of the new discoveries—have been the common practice since the time of Morton, and probably long before." The next extract was from the work of Beddoes, published in 1799. "In cases, therefore," he says, "where habitual weakness, or the history of the family gives reason to apprehend consumption, one of the most indispensable rules of preservation is to use animal food freely. There seems no limit to the quantity, but the indications furnished by the palate, and the power of the digestive organs." In regard to a remark made by Dr Bennett, that the pectoral symptoms should be neglected, and the treatment entirely directed to the improvement of the general health, he begged to say this was recognised by Fuller in his "Medicina Gymnastica," published in 1705. After talking of people being intent on the cure of that which is most troublesome,—viz., the cough, when they should lay the axe to the root of the tree, and be more intent on the cure of the habit of body, he says, "I am confident legions of the dead might have been above ground, if they had but conceived the fallacy of these means (balsamicks, tinctures, etc.), if they had but stuck close to the proper quantity of any one good alterative, they might have plunged out of their several maladies; but by placing all their hopes in things directed to the cough, they have fared like the dog, which bites at the stone that is thrown at him, instead of biting him which threw it." Dr Taylor begged to state in explanation, that the only object he had in view in reading the extracts he had done, was to show that the nutritive treatment of phthisis was by no means a novelty. The antiphlogistic mode of treatment was very much owing, he believed, to the influence of Broussais and his school; and as that had declined, the others had again became prevalent.

Professor Miller wished to take the opportunity of adding his testimony as to the value of cod-liver oil as a remedial agent; all the more as, like others, he had at first been quite sceptical as to its alleged virtues. He had taken every occasion to state his change of opinion, and to give all credit to Dr Bennett. He protested against the opinion implied in the speech of Mr Syme that scrofula was not hereditary. He thought it highly dangerous that such a statement should go forth uncontradicted; and believed that scrofula was sometimes implanted in the child by hereditary taint, while it was sometimes acquired by children of healthy progenitors. He considered that Dr Bennett had pushed his *stomach* theory and practice in regard to phthisis rather too far. He would by all means treat the stomach; but to "abandon the lungs," was a great deal too strong a phrase. The inunction of oil habitually, as proposed by Dr Simpson, had one very great objection—it was an essentially dirty practice, unless indeed it were followed by rubbing dry, and "polishing" the patient. He (Professor Miller) thought that some of the good effects ascribed to that rampant quackery of the day, the "rubbing" system, might be due to the great quantity of fat and grease which was rubbed in. Some good usually resulted from every quackery and imposture; and oil-inunction might be an atom of good in the general evil of indiscriminate rubbing.

Dr W. T. Gairdner agreed in a great measure with Dr Taylor in thinking that during the last century, at least up to its latter third, the treatment of phthisis had been conducted commonly more or less upon the tonic plan. The opinion of Morton, whose justly celebrated work was published towards the end of the preceding (17th) century, had to a great extent contributed to fix the principles of modern practice in this country, and he (Dr G.) had expected that it would have been referred to sooner in the course of that debate. The regimen recommended by Morton in almost all the numerous varieties of con-

sumption which he describes, was emphatically tonic in every sense; and in its therapeutical list, amid a great number of superfluous remedies, those of most constant occurrence were found to be bark and chalybeates, with balsamic, antiscorbutic, and stomachic medicines. Many of his detailed cases were treated in a manner which would be entirely approved at the present day, and apparently with marked success, as in the instance of his only son, who recovered from a very distinct attack of pulmonary and intestinal phthisis under "a wholesome air, the use of a milk diet, and afterwards of the Peruvian bark, and of a plentiful nourishment, which afforded a good juice." In the special form of *pulmonary* consumption, Morton recommended a great variety of remedies, addressed both to the chest symptoms and to the general system; a warm, dry air, free from smoke; moderate exercise, daily rubbing, and a generous diet "by which as few excrementitious particles as possible may be heaped up in the blood; such as mutton, veal, mountain birds, fish that are easily broke into pieces, craw-fish, etc." The patient was to avoid a surfeit or a debauch, and to "eat flesh only at dinner, contenting himself with panado, water-gruel, broths, poached eggs, etc., at night." For drink he was to have chiefly beer impregnated with pectoral herbs. Nor was the introduction of oily matters into the system neglected, as is proved by the following passage. "For lubricating medicines, let the patient plentifully use new butter that has not been salted in all his spoon-meat; buttered ale, and, if he bears it well, common sweet-oil or rather oil of sweet almonds, but especially linseed-oil fresh drawn without fire, of which let him take *a spoonful every hour*, unless there be a looseness or some other symptom that forbids it." This was certainly a sufficiently near approach to the treatment by cod-liver oil, and the quantities given were, it would be observed, much larger than is now usual with Dr Bennett's remedy.

Dr Gairdner farther remarked that he had referred in detail to the treatment of Morton, not as an exceptional case, but as exhibiting the accredited principles of treatment at the time; which in one form or other had continued to guide the practice of English physicians until the progress of anatomical inquiry brought with it an increasing tendency to solidism in theory, and an undue bias towards local and antiphlogistic treatment in all diseases. He could not but think that Dr Bennett deprived himself of a most important kind of evidence in favour of the tonic treatment of phthisis by putting it forward as novel, and disregarding the opinions and statements of the older physicians in its favour. It would be found that a treatment of phthisis not very different from that now commonly employed by judicious practitioners, was advocated hundreds (it might be said thousands) of years ago by the early Greek and Roman physicians. In the fragments of Aretæus, which, as is well known, contain probably the first really extended description of phthisis pulmonalis, air, exercise, and good food, form the three chief indications of cure alluded to; and the milk diet, so much insisted on throughout antiquity, was undoubtedly used, in general, as part of a tonic, not of an antiphlogistic system. Moreover, numerous precedents might be found among several of the Greek and Roman writers for the use of oily and fatty substances in phthisis, both internally, as recommended by Dr Bennett, and externally by inunctions, as proposed by Dr Simpson. In particular, both Aretæus and Cœlius Aurelianus make prominent mention of frictions with oil as an important part of the treatment of the disease.

In reference to a remark by Dr Simpson respecting the slight allusion made to phthisis pulmonalis by the Latin classical writers, Dr Gairdner said that he did not think any inference could be drawn from this circumstance as to the rarity of the disease in Rome under the empire, at which time inunction was chiefly practised. The descriptions by Celsus and by Cœlius Aurelianus were amply sufficient to show that it was not neglected or overlooked by medical writers; and any obscurity or confusion to be found in these descriptions existed to a far greater extent in those of the older Greek authors, who, like the Romans, not unnaturally confounded some cases of phthisis with empyema,

peripneumony, and catarrh. The description of the disease by Aretæus was, however, unexceptionable; and Galen, who practised at one time in Rome, knew and described it well as it existed there.

Dr Gairdner said, in conclusion, that he had limited his remarks to the historical question as to the origin and progress of the treatment of phthisis by constitutional and tonic remedies and regimen. The pathological theory of the disease specially put forth by Dr Bennett, and the treatment by cod-liver oil, introduced by him in 1841, were subjects on which he would gladly have had an opportunity of offering some observations, had the time of the Society and the course of the debate permitted it.

Dr Simpson made a few remarks in reference to the asserted want of cleanliness attending the proposed practice of inunction. He believed this difficulty would be got over, and the practice would be adopted. He considered that not a little impulse was given to the antiphlogistic treatment of consumption by a paper read at the Medico-Chirurgical Society by Dr Alison, and included in the first volume of its Transactions; the object of which was to show the inflammatory origin of tubercle.

Dr Macaulay remarked, that whatever glimpses or anticipations of the tonic treatment of phthisis might be found in old writers, they ought not to detract from the merits of Stewart or any others who had introduced another system; for undoubtedly, at the beginning of this century, the almost invariable treatment of consumption was antiphlogistic and non-nutritive. Such was the practice taught by Dr Gregory, when Dr M. attended his lectures in 1805; such was his own practice in 1814. He had not of late seen much of phthisis, but approved entirely of an opposite plan, and joined in the commendation of cod-liver oil, for the introduction of which mankind and the profession owed so much to Dr Bennett.

Dr Bennett said that he considered the communication of Dr Simpson a very valuable one. There was every reason to believe, as had been stated in his monograph on Cod Liver Oil, that inunction might be very beneficial. In this way the fatty matter was introduced into the lymphatics, and served to elaborate blood through the agency of the mesenteric glands. He could not imagine, however, that its introduction by the lungs could be serviceable, as the object was not to mix free oil with the blood, as would then occur, but to improve the blood itself. This could only be successfully accomplished by first operating on the chyle.

With regard to the question of priority, Dr B. observed that the establishment of all important improvements in medical theory or practice, might be divided into two stages. In the first stage, the truth of the innovation was attacked; in the second, its originality. Now the truth of his views seemed to have been tacitly admitted, although the honour of establishing them had been claimed by Mr Syme for the late Dr Sanders; by Drs Christison and Macaulay for Dr Stewart; by J. W. Begbie for Christopher Bennet; by Dr Taylor for Drs Fuller and Morton; by Dr Gairdner for the Roman and Greek Physicians, who had even given oil in Phthisis, as it seems did the father of medicine himself. Thus to use the words of a recent anonymous author when alluding to a new operation introduced by Mr Syme "Tout çela etait bein connu d' Hippocrate." But when he remembered the great doubts which existed in 1841, as to the probable benefit of cod-liver oil, and the difficulty he had experienced in persuading his medical friends to give it a trial, he was at a loss to understand how this should have happened, if all that he had been speaking of was at that time universally believed. Mr Syme had distinctly stated that at first he was wholly sceptical as to the advantage of cod liver oil, though now satisfied of its utility—other speakers had said the same thing. But surely it was rather inconsistent to be sceptical of the advantages of this substance, at a time when it was contended that the principles on which it was administered were thoroughly understood. So far from disregarding the opinions and statements of the older physicians, as Dr Gairdner imagined, he accepted all the facts they had adduced in support of his views and practice. All kinds of remedies and modes of treat-

ment had been tried in Phthisis, and no doubt some of them must have been useful. But it was dangerous to the progress of medicine to attempt extract from the hypothetical and random statements of the older physicians, a clue for originating a particular treatment, because this or that substance had been administered, or because they had casually remarked the good effects of a particular practice. By so doing, the greatest quack of modern times might in future be hailed as the most eminent man of his age. If, on the other hand, it could be shown that such a treatment as was most in accordance with the principles he had been advocating, had been more successful than any other (which had not yet been done) then, and then only, it would constitute another argument in favour of the doctrines he had laid before the Society.

MEDICAL REFORM BILL.—DEPUTATION TO LORD PALMERSTON.

On Friday the 18th of March, a deputation of the Provincial Medical and Surgical Association waited, by appointment, upon Lord Palmerston, at his residence in Carlton Gardens, to present to his Lordship the bill that they have prepared for the regulation of the medical profession, and to request him to introduce it into parliament as a Government measure.

The deputation consisted of Sir Charles Hastings, M.D., D.C.L., President of the Council; A. Robertson, M.D., of Northampton; George Webster, M.D., of Dulwich; Mr Noble, of Manchester; Mr Southam, of Manchester; Mr Cartwright, of Oswestry; Mr Bree, of Stowmarket; Mr Bottomley, of Croydon; Mr Stedman, of Guilford; Mr Walsh, of Worcester; Mr Nunneley, of Leeds; Mr Hastings, barrister—being the committee of the Association who have charge of the bill; John Forbes, M.D., President of the Metropolitan Counties' Branch; J. C. Williams, M.D., Nottingham, President of the Midland Counties' Branch; C. Chadwick, M.D., Leeds, President of the Yorkshire Branch. J. Bedingfield, M.D., Needham Market, President of the Suffolk Branch; J. Heygate, M.D., Derby; Mr Norman, Bath; F. R. Horner, M.D., Hull; G. S. Jenks, M.D., Brighton, Vice-Presidents of the Association; J. Conolly, M.D., Hanwell; C. Cowan, M.D., Reading; W. P. Brooks, M.D., Cheltenham; Mr Nunn, Colchester; Mr Clements, Shrewsbury; H. Johnson, M.D., Shrewsbury; E. V. Mainwaring, M.D., Bournemouth, Members of the Council; Dr Renton, late President of the College of Physicians of Edinburgh; Dr Combe, President of the College of Surgeons, Edinburgh; Sir John Liddell, of the medical department of the navy; and Mr Wakley, late M.P. for Finsbury.

The following members of parliament accompanied the deputation:—The Right Hon. T. B. Macaulay; the Right Hon. M. T. Baines; the Right Hon. T. M. Gibson; Lord Alfred Hervey; Lord Elmley; Lord Hotham; Mr O. Ricardo; Mr Walter; Mr Brotherton; Sir G. Pechell; Sir T. Winnington; Captain Rushout; Mr Laslett; Mr B. Westhead; Sir H. Willoughby; Mr W. Knight; Mr Craufurd; Hon. R. H. Clive; Mr Cobden; Sir G. Goodman; Mr Bright; Hon. Captain Duncombe; Mr Alcock; Mr Pigott; Mr Phinn; Mr Langton; Mr Raikes Currie; Mr Booker; Mr Aglionby; Mr Tomline; Mr Whalley; Mr C. Berkeley; Mr G. Berkeley; Mr Brown; Mr Martin; Mr Price; Mr Headlam; Mr Cobbett; Mr Bass; Mr Drummond; Mr Mangles; Mr Bell; Mr Stafford; Viscount Newark; Mr Barrow; Mr Vernon; etc.

The deputation having been courteously received by Lord Palmerston,

Sir Charles Hastings addressed his lordship on their behalf, and said that the Provincial Medical and Surgical Association had existed for twenty-one years, and numbered nearly two thousand members, residing in all parts of the kingdom. It was established for the advancement of medical science; but, finding many obstacles from the incongruous state of the profession, they had found that it was absolutely necessary to improve the organisation of the profession.

They had, from the first, laid down broad principles of medical reform, and had never swerved from them. These were, uniformity of qualification, equal right to practise throughout the United Kingdom, and the adoption of the representative principle in the formation of the councils or other governing bodies. They had from time to time presented petitions to parliament, and memorials to government on the subject; and, within the last three years, Sir George Grey had expressed a resolution to give his sanction to any measure that might be generally approved of by the medical profession. Since that time a committee of the Association had framed a bill with the assistance of Mr Hastings, a barrister, which had been repeatedly submitted to the profession, through the medical societies, and the district branches; and it had been altered and improved to meet various suggestions. As the result of their labours, he might now state that the bill had received an unparalleled amount of support. The London College of Physicians had given it their approval, and declared that it was in harmony with their proposed new charter. A deputation from the Edinburgh Colleges was present to express their approval: and it was warmly supported by a vast majority of medical practitioners throughout the kingdom. In fact, no opposition had been made to its principles, though some exceptions might have been taken to its details. The bill embraced the establishment of a Medical Council, of a Board of Examiners, before whom every candidate for a right to practise must go, and of a system of registration. Sir Charles then expressed the wish of the Committee, that, should any objections be made to the measure, his Lordship would give them an opportunity of replying through their Secretary, Mr Hastings; and in placing the bill in his Lordship's hands, he tursted that he would think fit to pass it into law, and thus to confer a great benefit, not only on the medical profession, but on the sick and suffering of the whole community.

Dr Renton, and Dr Combe, on behalf of the Medical Colleges of Edinburgh, then expressed their perfect approval of the bill.

Sir John Liddell pointed out the need of better education for medical officers in the navy; at present they had men well versed in surgery, but ignorant of pharmacy, and *vice versa*. The establishment of a common Board of Examination would remedy this evil.

Mr Wakley then addressed his Lordship, and said that he had been engaged for twenty-nine years in the work of medical reform, but never till now had he seen much hope of a successful result. Various measures had been introduced into Parliament by Sir James Graham, and by himself, but they had all failed. Now, however, there was a very great approach to unanimity on the subject; and if Lord Palmerston took up the bill as a Government measure, which he implored him to do, it would receive the most powerful support, it would pass into law without any serious opposition, and would be productive of incalculable good.

Dr Webster, as an old medical reformer, and one supposed to take rather extreme views, begged to express his entire concurrence in the bill.

Mr Brady, M.P., and several other gentlemen having spoken to the same effect,

Lord Palmerston said, that he was deeply impressed with the subject which had been brought before his notice by the deputation, affecting, as it did, not only the interests of the public, but those of a most highly educated and important profession. Many attempts had been made to legislate on it, but without success. When out of office, some years since, he had been requested to take up the subject, but had declined to do so, on account of the discordant opinions then entertained by the medical profession. Now, however, a different feeling seemed to prevail, and a great approach towards unanimity had evidently been made, as was manifest from the importance and varied character of the deputation. He should give his best attention to the measure that had been laid before him; and if there was a good prospect of bringing it to a

satisfactory settlement—and he thought he saw, now, such an opportunity—he should feel it his duty to bring it before Parliament, in conjunction with his colleagues, as a Government measure, and to carry it out with energy. His Lordship added that he should take an early opportunity of communicating with the Secretary, Mr Hastings, on the subject.

Sir Charles Hastings thanked his Lordship, on behalf of the deputation, for his personal courtesy, and for his promise to take the bill into his favourable consideration.

The deputation then withdrew.

POISONING WITH TARTAR EMETIC

SUCCESSFULLY TREATED WITH GREEN TEA AND TANNIN.

BY S. A. M'CREERY, M.D., U.S.N.

On the morning of the 28th of September, 1852, Dr ———, feeling a little indisposed, took twelve grains of blue mass. At half-past two o'clock P.M., some hours later, he took, on an empty stomach, what he believed to be (and what he had ordered) half an ounce of Rochelle salts, with forty grains of bicarbonate of soda, and as much tartaric acid. Immediately afterwards he dined sparingly on ham, the breast of chicken, and tomatoes, and after dinner ate two ripe figs. In about thirty-five or forty minutes after taking the medicine, he experienced some nausea, but attributing it to his indiscretion in having eaten so soon after taking the powder, he resisted the disposition to vomit, which every moment became more urgent. In the course of four or five minutes, however, he was obliged to yield to it, and vomited twice very freely, after which he felt relieved. In two or three minutes the nausea and vomiting returned, and he then began to suspect that he had received the wrong medicine. Instant inquiry was made, and it was discovered that the person who had put it up had mistaken the *antimonii et potassæ tartras* for the *sodæ et potassæ tartras*. Dr Thomas Williamson, of the navy, saw the patient at twenty-five minutes past three P.M., and immediately ordered for him copious draughts of green tea and large doses of tannin. Albumen, the infusion of flax-seed, and of the slippery elm, and iced water, were also freely administered. The vomiting, which was very distressing, continued with little intermission until nine or ten o'clock in the evening. There was also very severe purging, with most violent cramps of the legs, and slighter ones of the wrists. The first evacuation from the bowels was purely serous; those which followed were of a bilious character, but very loose. There were no cramps of the stomach. When it was thought that the stomach and bowels had been cleared of the poison, an injection containing tincture of opium was given, and repeated in a few moments. The injections not being retained, a full dose of the acetate of opium was administered by the mouth. Brandy mint julep was also freely given, as the patient was very much prostrated. A large sinapism was applied over the epigastric region, and frictions were used to the extremities during the paroxysms of cramp. Iced tea and iced mucilaginous drinks were continued through the night. The patient passed a better night than could have been expected; was still nauseated, and complained of great thirst, and had some headache; the tongue was moist; there was no abdominal pain or soreness, or any burning sensation about the stomach; the bowels were twice moved. Iced tea, and iced milk, and arrow-root diet were directed. 30th.—There was no vomiting or purging; no thirst; diet and drinks were continued as before.

October 1st.—Rested badly the past night; had headache and nausea; and the tongue was coated with white fur, but there was no febrile excitement. The iced drinks were continued, and toast and coffee, chicken-

broth, and oyster liquor were allowed the patient. 2d.—There was some slight irritation of the mucous membrane of the throat, and pain on pressure in the upper and right side of the breast; no fever. A sinapism was applied over the seat of pain, and the slippery elm bark and gum Arabic were used to allay the irritation of the throat. The diet and drinks were continued. 4th.—There was still some slight irritation of the throat, and some cough, but no pain in the chest nor fever. The patient was allowed full diet. From this date he continued to improve, and on the 13th, was able to resume his ordinary occupations. —*Amer. Jour.*, quoted in *Dublin Medical Press*, March 23, 1853, p. 184.

JURY TRIAL.

MULLAR AGAINST ROBERTSON AND OTHERS,

Conductors, Publishers, Printers, &c., of the Monthly Journal.

Our readers must be perfectly aware of the circumstances out of which this trial originated. It was decided by Jury in the Second Division of the Court of Session on the th of March—the Lord Justice-Clerk presiding. The Issue was as follows:—

"It being admitted that the defenders are responsible for the Monthly Journal of Medical Science, No. 128, dated August 1851, No. 20 New Series, and No. of Process:

"It being also admitted that, under the title 'Medical News,' p. 198, the following article appears in the said Journal:—

"'THE LONDON MEDICAL GAZETTE.

"'One of the Conductors of this Journal lately felt it necessary to address a letter of remonstrance to the editor of the London Medical Gazette, who published it in an imperfect form, under the pretext that the matter excluded would have been subject to the English law of libel. Two results have followed. In the first place, the letter is rendered meaningless; and, secondly, the author is made to appear to have used libellous language. In order that our readers may judge how far this conduct was warranted, we now place before them the letter in its original form, the omitted portion being enclosed within brackets:—

"'Edin., June 26th, 1851.

"'Sir,—I have only to-day happened to see your Journal of May 16th, which contains some statements that certainly should not have remained so long unnoticed if they had been known to me sooner.

"'You say a fierce paper war has arisen between the two Edinburgh professors, Syme and Lizars; but you must, or at least ought, to know that I have not addressed a single word upon the subject in question to the so-called 'professor,' [regarding him as long placed beyond the pale of professional respect and courtesy.

"'In estimating the value of my operation, you proceed upon the supposition that the allegations of Mr Lizars, and his assistant, Dr Mullar, are well founded; but in fairness to your readers, if not to myself, should have mentioned that the statements of these persons, in so far as they attribute bad effects to the operations which I have performed for the remedy of stricture by division, have been declared by me to be all utterly devoid of truth.]

"'Within the last eight months I have performed this operation nine times in the Royal Infirmary of Edinburgh, in presence of the largest class of surgical clinical students in her Majesty's dominions. These gentlemen can testify that in no instance has there been bleeding, extravasation of urine, or any other unpleasant consequence, and that all the patients speedily and com-

pletely obtained the relief which they desired. As you say, that 'something more than the guarantee of Mr Syme's reputation is wanting, to assure the surgeon that he would be justified in having recourse to the proposed operation,' I beg to inquire, if you think the evidence thus afforded sufficient, and if not, what further proof you deem requisite to establish the safety and efficiency of my operation.—I am, &c.

"(Signed) 'JAMES SYME.'

"Whether the statements contained in the foregoing article of the said Journal are of and concerning the pursuer, and falsely, calumniously, and injuriously represent the pursuer as the assistant and associate of a person who is described as having been long excluded from, and unworthy of, the respect and courtesy of the medical profession generally, and of his fellow-practitioners therein, and as being united with the said person in joint medical publications, and in the culpable propagation of false statements regarding diseases and medical operations of great importance to the health and lives of individuals,—to the loss, injury, and damage of the pursuer?

"Damages laid at £1000."

The Jury, after brief deliberation, returned a verdict for the defenders.

PUBLICATIONS RECEIVED.

The Local Application of a Benumbing or Congealing Temperature. By James Arnott, M.D. London: Churchill. 1852.

Permanent Cure of Reducible Hernia. By George Hayward, M.D. Philadelphia: Collins. 1852.

The Right of Marischal College and University, Aberdeen, to Confer Degrees, Vindicated. By one of the Professors. Aberdeen: Lewis Smith. 1853.

On Near Sight, Aged Sight, Impaired Vision, and the Means of Assisting Sight. By William White Cooper, F.R.C.S., Esq. Second Edition. London: Churchill. 1853.

Lectures on the Nature and Treatment of Fever. By D. J. Corrigan, M.D., etc. Dublin: Fannin & Co. London: Longmans & Co. 1853.

On Hysterical and Nervous Affections. By William John Anderson, F.R.C.S. London: Churchill. 1853.

An Account of Yellow Fever as it Occurred on board the R.M.S. ship "La Plata." By John Wiblin, Esq., F.R.C.S., and Alexander Harvey, M.D. London: (Reprint from Lancet.) 1853.

Sanitary Measures and their Results. By Thos Shapter, M.D. London: Churchill. Exeter: Adam Holden. 1853.

On the Construction and Use of the Microscope. By Adolphe Hannover, M.D. of Copenhagen. Edited by John Goodsir, F.R.S.E. Edinburgh: Sutherland and Knox. 1853.

A Text-Book of Physiology. By Dr G. Valentin. Translated and Edited by William Brinton, M.D., &c. London: Renshaw. 1853. Part I.

On Lithotomy and Lithotrity. By William Coulson, Surgeon to St Mary's Hospital, &c. London: Churchill. 1853.

Urinary Deposits: their Diagnosis, Pathology, and Therapeutical Indications. By Golding Bird, A.M. and M.D., &c. Fourth Edition. London: Churchill. 1853.

A Dictionary of Medical Science. By Robert Dunglison, M.D., &c., of Philadelphia. London: Sampson Low, Son, and Co. 1853.

Homœopathy: Its Tenets and Tendencies, Theoretical, Theological, and Therapeutical. By James Y. Simpson, M.D., F.R.S.E., &c. Third Edition. Edinburgh: Sutherland and Knox. 1853.

Syllabus of a Couse of Lectures on Diseases of the Eye, at the New York Ophthalmic Hospital. By Mark Stephenson, M.D. Philadelphia: Fanshaw. 1852.

First Report of the Surgeons of the New York Ophthalmic Hospital, with the Address of the Hon. C. S. Woodhull, for the Year 1852. New York: Fanshaw. 1852.

Journal of the Statistical Society of London. Vol. XVI. Part I. March, 1853. London: Parker & Son.

Bulletin de l'Académie Royale de Medecine de Belgique, 1851-52 et 1852-53. Dix Numeros. Bruxelles.

Memoires des Concours et des Savants Etrangers. (Second Fascicule du Tome II.) 4to Bruxelles. 1852.

El Porvenir: Periodico de los Intereseo Materiales, Morales y Cientificos de las Clases Mèdicas. Madrid. 1853. (Three Numbers.) [We must decline the proposed exchange.]

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On Malformation of the Aortic Valves as a Cause of Disease.* By THOMAS B. PEACOCK, M.D., Assistant-Physician to St Thomas's Hospital, and Physician to the City of London Hospital for Diseases of the Chest.

PATHOLOGISTS are familiar with the frequent occurrence of malformation of the aortic semilunar valves; I think, however, that the importance of this condition as a cause of valvular disease has not been hitherto adequately appreciated. In the following communication, I propose to point out the various modes in which disease originates in the malformed valves, and to relate some cases in illustration.

When the aortic valves are irregularly developed their number may be in excess or deficiency. In the former case there are four segments, which may either be of nearly equal size, or, while three of the curtains have their normal dimensions, a small supernumerary segment may be interposed between two of them. It is, however, much less common to find excess in the number of the aortic valves than of the pulmonic; and the anomaly does not appear materially to interfere with the function which they have to perform, so as to predispose to disease. It is not, therefore, necessary to occupy more time with the consideration of excessive development; and I shall proceed to speak of the other condition, that in which the number of the segments is defective. Elsewhere,[1] I have described two forms of defect in the number of the aortic valves. First, that which is by far the most common, in which two of the curtains are found blended into one, by the fusion of their contiguous sides, and the atrophy of the intervening septum so produced. Generally the valve formed by the union of the two segments is much larger than the other, and the former point of division is indicated on the upper surface by a frænum, or, to use the expression of John Hunter, *a cross bar*, extending from the free edge of the valve to the side of the aorta, and on the lower side by a more or less deep sulcus or groove. In these cases the relative size of the two valves may be regarded as indicating the period at which the

[1] Pathological Transactions, vol. iii.

union of the segments occurred. When they are nearly of equal size, and the frænum and sulcus are not very apparent, the fusion probably took place at an early period of fœtal life; but where the united valves form a curtain much larger than the other, and proofs of the former division are conspicuous, the adhesion most likely occurred only a short time before birth. The second form of defect is very rarely seen, and consists in the interposition of a very small segment between the extremities of two unusually large valves. This seems to depend on the growth of the smaller valve having from some cause been arrested, so that it has not become developed in proportion to the other two. In addition, however, to these forms of defective development, by which the number of the aortic valves is reduced to two, it occasionally, though so far as my experience serves me, very rarely happens that the whole of the three segments are united together, so that they form a kind of membranous diaphragm extended across the orifice. This membrane is perforated in the centre by a somewhat triangular aperture, and its upper surface presents the remains of three distinct frænа, precisely as is the case so frequently with the pulmonic valves.

All these anomalous forms may be regarded as strictly malformations; that is, as dependent on disease during intra-uterine life. It is true that the union of the valves has been ascribed to the occurrence of inflammation after birth; but, in opposition to this, it may be shown that the valves are not unfrequently found in this state in infants which have survived only a few hours, and in children who have died at early periods of life without having presented any signs of disease. I possess the heart of an infant, which lived only six days, in which, in addition to other malformations, two of the aortic valves are completely united. I have also two other preparations exhibiting this condition, in young boys, who met with accidental deaths, and were not ascertained to have suffered from any serious illness during their lives. In the "Pathological Transactions," a case of malformation of the heart, in a child fourteen months old, is described by Dr Hare,[1] in which, in addition to other irregularities, the curtains of two of the semilunar valves are stated to have been united. Very recently my friend, Dr R. Quain, allowed me to examine the heart of a child, six months old, in which a similar condition existed. And Mr Canton[2] exhibited, at the Pathological Society, the heart of a child, which had only lived two days, in which the aortic orifice was entirely obliterated, in consequence of the valves having become united. It is therefore evident that the adhesion does take place during fœtal life. Similar changes in the pulmonic semilunar valves are well known very frequently to occur in cases in which the co-existent malformations clearly indicate the disease to have occurred during fœtal existence; and though the valves may become adherent in after life, there can rarely be any difficulty in

[1] Pathological Transactions, vol. i. p. 208. [2] Ibid., vol. ii. p. 38.

distinguishing cases of this description from those to which I have referred. It has also been suggested, that the union of the segments may result from the accidental tearing down of one of the angles of attachment; but accidents of this kind are most probably very rare,[1] and could scarcely fail to be attended by well-marked symptoms, and to be readily detectable on examination after death. For either the valves must be torn from the fibrous zone, or the valves and zone still attached, must be separated from the middle coat of the aorta; whereas I have ascertained in several cases, that, though the remains of the angle of attachment of the united curtains was at a lower level than the other angles, the connections of the parts were unaltered.

The defective condition of the semilunar valves, to which allusion has been made, is not necessarily productive of any important consequences. The two segments may expand with the progress of growth, so as to adapt themselves to the increased size of the aortic orifice, and effectually prevent any regurgitation from the aorta into the ventricle. Frequently, however, they undergo changes which render them sources of obstruction, or interfere with their competency, and these effects may result either from their becoming the seat of inflammation, from their ceasing to expand with the increasing capacity of the orifice, or from the giving way of their angles of attachment or folds under the pressure of the column of blood in the aorta.

The imperfect development of the aortic semilunar valves peculiarly predisposes them to suffer from inflammation, whether primary or secondary, as during rheumatic attacks, from any slight strain or other injury, or probably even from violent mental excitement; and thus, as has before been frequently remarked, where the segments are deficient in number, they very generally present evidences of having been the seat of inflammatory action. In this respect they obey the general law, that parts which have once suffered from inflammation (for I regard the adhesion of the valves as due to inflammation, or at least exudation, during fœtal life) are ever after prone to take on the same diseased action. When inflamed, the disease may either be acute, subacute, or chronic. Acute and subacute inflammation give rise to the effusion of fibrine and to softening, in consequence of which the valves may give way so as to allow of regurgitation, leading to a more or less rapidly fatal result. Under chronic inflammation, on the contrary, the valves become indurated and thickened, and so occasion obstruction, which may only be manifested after a long interval. As, however, acute and subacute endocarditis are not modified by the defective condition of the valvular apparatus, and their effects have already been fully investigated by other writers, I shall not further allude to them, but confine myself to the consideration of the results of the more chronic forms of disease in the previously malformed valves.

[1] See paper by the author on "Injuries of the Valves from Violent Muscular Efforts," in the 15th Vol. of this Journal, N. S., Vol. VI.

I. *Obstructive Disease as a result of Malformation of the Aortic Valves.*

When the malformed valves are attacked by chronic inflammation, the disease may be limited to the free margin or crescentic edge of the valves, to their line of attachment or to the septum, which marks the former point of division of the united valves, or the whole fold of the valve may be almost equally affected. The morbid change also may amount only to a slight degree of thickening and opacity of the membrane, or it may be so extensive that the valves may become very rough, dense, and unyielding, so as to form serious impediments to the flow of blood from the ventricle. In this extreme degree of morbid change, the valves may either be extended in the plane of the aperture, so as almost entirely to close it, or they may be protruded forwards, making a funnel-shaped opening into the aorta. The former condition is most generally seen where the valves are two in number, the fusion being limited to the contiguous sides of two of the segments, and in this case the aperture is a mere slit, of a somewhat crescentic form, extending from one side of the orifice to the other. The two valves are generally of unequal size, the margin of the smaller segment being convex, while that of the larger is concave; and, on the aortic side of the larger segment, there are the remains of the septum, indicating the original division between the two valves of which it is composed. In some specimens, however, where the valves are very extensively diseased, and have undergone the fibro-cartilaginous and osseous transformations, they are too much distorted for their original conformation to be readily distinguished; but it is easy in different specimens to trace the diseased changes from the slighter to the more marked conditions, and thus to demonstrate that the latter are only the advanced degrees of the former. Where the united valves are protruded forward, so as to form a funnel-shaped aperture, it is most usual to find the whole of the three segments adherent together. In this case the opening into the vessel has generally a somewhat triangular shape, and, on the aortic side of the adherent valves, three septa or frænа are usually apparent. When the valves are much indurated and thickened, the segments generally admit of being only very slightly separated in the first form, and may leave a permanently patent opening in the second; but the more aggravated examples of obstructive disease resulting from malformation of the valves are, I believe, only seen in persons in advanced life.

When, from the condition of the valves, great obstruction is created to the flow of blood from the ventricle, there is frequently a tendency to excavation of the ventricular parieties near the origin of the aorta, and occasionally a complete passage is formed, extending behind the valves into that vessel; or the undefended space at the base of the left ventricle may be dilated, and the membrane may ultimately give way, making an opening from the left into the right ventricle, or into the right auricle. In other instances in which there is a

communication between the aorta and the left ventricle, behind the adherent valves, the canal seems to have been produced by the gradual dilatation and extension, towards the ventricle, of one or other of the sinuses of Valsalva. Dr Bristowe recently showed me a specimen in which, with a slight amount of obstructive disease, caused by the septum imperfectly separating two adherent valves, there was an aneurismal dilatation of the right sinus of Valsalva, forming a tumour, which pressed upon the orifice of the pulmonary artery.

In either of the modes which have just been mentioned, obstructive disease may be complicated with regurgitation; and, indeed, in the form in which the valves are extended across in the plane of the orifice, as well as where they are protruded forwards, there may be a permanently patent aperture by which the blood may regurgitate from the aorta into the ventricle. But such is not ordinarily the case; for the changes which occasion a great amount of obstruction, can only be of very slow production, and are incompatible with the existence of material regurgitation for any considerable length of time before death. In the forms of disease now to be described, incompetency of the valve is the primary morbid change, and though it may be combined with some degree of obstruction, it is throughout the most important condition.

II. *Regurgitant Disease as a result of Malformation of the Aortic Valves.*

1. Though with the progress of growth, and the enlargement of the aortic orifice, the valves may increase in size, it very often happens that the fold does not grow equally in all parts; but, in consequence of the induration of the septum, which indicates the point of fusion of the two curtains, that portion of the united segment does not continue adequately to expand. The margin of the curtain at that point is thus, as it were, held back, so as to leave a somewhat triangular shaped aperture between the two valves. When this is the case, a slight flow of blood from the aorta into the ventricle takes place during the diastole of the heart, and generally the edge of the defective valve soon becomes retroverted, and free regurgitation is established.

2. In other cases, the united fold expands fully, but, in consequence of the pressure of the blood on its surface being imperfectly resisted from the atrophy of the angle of attachment in its centre, the united curtain is allowed to drop below the level of the other. This change takes place the more readily, from the gradual yielding of the margin of the valve and of the angles of attachment, under the undue pressure to which they are subjected; and in this, as in the former case, when once a regurgitant current is established, it usually rapidly augments, as the lower edge of the valve generally soon becomes retroverted.

3. In yet other cases, regurgitation takes place through the gradual giving way of the curtain of the malformed valve, the fibrous

tissue becomes thinner and more separated as the fold expands, till at length the serous membrane is left without support, and an aperture is produced, through which the blood flows back into the ventricle.

4. It is quite possible, also, that when the valves are imperfectly supported, in consequence of the congenital defect, a strain or other slight injury may occasion a yielding or laceration of their angles of attachment, so as to allow the larger curtain to fall below its proper level. Trivial accidents of this kind would, however, not be immediately or even rapidly fatal; but as in such cases endocarditis would necessarily result, it is difficult to say whether, when evidences of inflammatory action are detected, they should be referred to accident or to idiopathic disease.

The various changes now described are aggravated by alterations in the capacity of the left ventricle, the aortic orifice, and the aorta, by which the power of resistance in the valves is lessened, or the pressure which they have to sustain is augmented. It should also be observed, that in cases of malformation of the valves, the evidences of subsequent disease are not entirely limited to the united segments, but that as the naturally formed valve is usually much expanded, so it is liable to undergo changes similar to those which affect the malformed valve. Occasionally, also, the former is found extensively diseased, while the latter is but little altered. Thus the naturally formed valve may be generally thickened and indurated, or its edge may be retroverted or depressed, or its fold perforated, when the united segment is entire. I recently observed a specimen of the latter description in the museum of St George's Hospital.

In the plate attached to the paper, I have represented drawings of specimens exhibiting marked examples of most of these changes in the malformed valves.

Figs. 1 and 2 are drawings, taken from above and below, of the aortic valves in a woman who died at the age of 76, after having undergone the operation for strangulated hernia. It was ascertained that, with the exception of a severe cold, under which she suffered two months before, she had enjoyed remarkably good health throughout her life, and was an unusually robust person for her age. She had never presented any evidences of disease of the heart, and was able to ascend to the top of the house without her breathing being embarrassed. The aperture from the ventricle into the aorta was extremely small, being a mere chink ten lines in length. There were only two valves, and one of these was evidently formed by the congenital union of the anterior and left segment, for there remained evidence of its former division, as seen in Fig. 1. The valves were thick, rough, fibro-cartilaginous, and in some places osseous, and the segments did not admit of separation to the extent of more than three or four lines. Notwithstanding the great amount of valvular disease in this instance, and the length of time during which it must therefore have been in progress, the heart was not materially hypertrophied, its weight being only ten ounces and

Fig. 6.

Fig. 1

Fig. 2

Fig. 3

Fig. 5

Fig. 4.

Tuffen West, ad nat del et lith

Ford & West Imp 54, Harrison

three-fourths. The sinuses of Valsalva were much dilated, and the coats of the aorta somewhat atheromatous.

Fig. 3, sketched from above, represents a specimen in which, with fusion of two of the segments into one, the valves are protruded forwards, forming a funnel-shaped aperture, and are greatly thickened, indurated, and ossified. The preparation from which the drawing is taken, is contained in the Museum of the Royal College of Surgeons, and, though the age of the subject is not named in the catalogue, there can be no doubt that it was a person in advanced life.

Fig. 4 represents the condition of the aortic valves in Case 3, and illustrates the imperfect expansion of the portion of the united segments into which the frænum is inserted, by which the edge is, as it were, held back, so as to leave a triangular space between that valve and the other, through which regurgitation takes place. In this instance, the general yielding of the united curtain from the imperfect support which it receives in its centre, is also well seen.

Fig. 5, drawn from a preparation in the Museum of the Royal College of Surgeons, which has also been engraved by Dr Baillie, exhibits the aperture thus formed, as seen from below. In both this and the former case, notwithstanding the existence of free regurgitation, the edges of the valves were not retroverted.

Fig. 6 represents a specimen exhibited at the Pathological Society by Mr Obré, through whose kindness I am enabled to give the drawing. It affords a good example of retroversion of the edge of the malformed segment, in consequence of the imperfect support which it receives from the atrophied angle in its centre. It shows also the effects of endocarditis, as usual not limited to the malformed valves alone, but involving the naturally formed segment. It will further be seen to have affected, more or less extensively, the endocardium at the base of the ventricle, and especially in those situations exposed to the column of blood flowing from the aorta during the diastole of the heart. In this specimen the retroverted portion of the malformed valves near the frænum displays one of those small dilatations which have been termed aneurisms, and when the specimen was recent, the retroverted portion of the valve was adherent to the endocardium below it.

The following cases I have selected as affording examples of several of the different forms of disease above described.

CASE I.—*Malformation of the Aortic Valves—Sub-Acute Endocarditis, leading to destruction of the united segment, originating in violent mental excitement.*

J. L., æt. 18, was admitted a patient of the City of London Hospital for Diseases of the Chest, on the 27th of November 1849. It was stated that he had been ill nine months, having been seized the previous February. He was looking at the remains of a house which had been burnt, and where two persons had lost their lives, when his feelings were so much worked upon by the description of

the occurrence, that he was thrown into a fit, which left him paralysed in the left arm and leg. Soon after, he began to suffer from symptoms of disease of the heart. He, however, admitted that previously his breathing became hurried on exertion, and he was very readily fatigued, but he had never presented any signs of cyonosis, and had never had any serious disease. His head was very large, and his face tumid and puffy. The fits continued to recur at intervals. He had partial loss of power over the left arm, and the fingers were contracted; the left leg was also weak. The left side of the chest was fuller than the right, and the prominence was most marked in the precordial space. The cardiac dulness commenced three fingers' breadth above the nipple, and extended laterally from the right side of the sternum to two fingers' breadth beyond the line of the left nipple. A very distinct purring tremour was felt over the whole precordia, but it was most obvious in the interspaces between the second and third, and the first and second cartilages, immediately on the right side of the sternum. It was also feebly perceived over the carotid and subclavian arteries. The pulsation of the apex of the heart was felt in the sixth intercostal space. A loud systolic murmur was heard over the whole præcordia, but it was most distinct at the base, along the upper part and right side of the sternum, and at the top of that bone. At about the union of the upper and middle thirds of the sternum, the sound in the right intercostal spaces had a distinctly gurgling character. A feeble diastolic murmur was heard at the base of the heart, and a double murmur was audible to the left of the spine, in the interscapular region.

The general symptoms under which he laboured were those of cardiac asthma. He complained especially of severe pain in the region of the heart, extending to the back and down the left arm. He had great difficulty of breathing, but could ordinarily lie down if his chest and head were elevated. On any exertion, however, the dyspnœa and pain became extreme, and he could only obtain relief by pulling upon his arms, by resting his head between his knees, or by hanging his arms and head over the side of the bed, so as to compress the chest. He never presented any marked degree of lividity, but, during the paroxysms of difficulty of breathing, the head and face became hot and red, and the heart and arteries of the neck pulsated violently. The fits were of an epileptic character—he foamed at the mouth and became comatose, and the left arm was always violently convulsed. He continued to attend at the hospital at intervals, till his death. He had occasional fits, and was jaundiced for several days on two occasions. The difficulty of breathing, and pain in the region of the heart, were continued sources of suffering, but he obtained some relief from the use of alterative medicines, diffusible stimulants, and anodynes. Frictions in the precordial region, with embrocations containing the tincture of aconite, and soap and camphor liniments, prepared according to Dr Fleming's formula, afforded him great relief. He used them

at first in the proportion of one-fourth of the aconite, then of one-third, and lastly, of one-half; but latterly even this had lost its effect. He was never anasarcous; and the urine, though repeatedly examined, never presented any trace of albumen. He died suddenly, when raising himself in bed, on the morning of 12th October 1852. The *post-mortem* examination took place the same evening, twelve hours after death.

The body was considerably emaciated; the pleuræ were free from adhesions; the lungs were voluminous, but very much congested, and imperfectly crepitant. The heart occupied a large portion of the front of the chest. The pericardium was not adherent, but there was a white patch on the serous covering of the heart in front. The smaller branches of the coronary arteries were ossified. The right auricle was large, and contained a firm but not decolorised coagulum. The right ventricle was somewhat large, and its walls hypertrophied. The pulmonic valves were healthy. The left ventricle was greatly dilated and hypertrophied; the walls near the base being fully half an inch in thickness. The lining membrane was opaque, and the inner portions of the muscular substance were pale and dense. At the apex there was a very marked bulging of the ventricular parieties, and the walls were here thin, and had undergone the fibro-cartilaginous transformation. In one portion of the cavity thus formed, there was embedded a mass of bone. The aortic valves presented an aggravated example of recent disease superinduced upon an original malformation. The right and posterior segments were so completely blended that there remained only a thin band of fibrous tissue connecting the centre of the fold with the coats of the aorta. The other valve was also adherent at its angles, and its curtain was much thickened. The right portion of the united segment was completely broken down, and its edges were studded with recent vegetations and nodules of cretaceous deposit; and there was also a large opening in the left portion of the united valve. The aperture was thus not only reduced in size, only admitting the passage of a cylinder thirty lines in circumference, but when the valves were extended so as to close the orifice, two apertures existed in the malformed valve, through which blood could regurgitate into the ventricle. The ascending aorta was greatly dilated. The mitral valve was somewhat opaque but not otherwise diseased. The heart weighed twenty-eight ounces avoirdupois.

The thymus gland was still of large size. The liver, spleen, and kidneys were large and congested.

Permission could not be obtained to examine the head.

In this case, though the aortic valves presented a congenital defect, it is not probable that any material imperfection existed till after the severe mental impression described. At this time some inflammatory action probably originated in the valves, and hence the destruction of the right valve which was found after death.

CASE II. *Malformation of the Aortic Vales, giving rise to Obstructive Disease—Small Aneurism at the base of the Ventricle—Slight Rheumatic Symptoms—Double Pneumonia.*

Isaac Locke, æt. 40, an iron-founder, admitted into St Thomas' Hospital, from Dartford, July 27, 1851.

At the time of his admission he stated that he had been a strong man, and had enjoyed good health till eleven weeks before, when he suffered from indigestion, and had pain and swelling in the right leg. The latter symptoms soon subsided; but he continued to labour under indigestion, and was compelled to relinquish his employment and apply for relief at the hospital. He complained chiefly of debility, and a sense of weight and tightness across the epigastrium, which was increased on taking food, and occasionally affected his breathing so as to prevent his walking. He distinctly denied having ever suffered from rheumatism, inflammation of the chest, or any severe accident or strain, prior to the commencement of his illness, and he stated that he had never before been subject to difficulty of breathing, and this was confirmed after his death by a shopmate who had long known him. His habits, however, had been intemperate. His face was somewhat tumid and sallow; the pulse feeble, a loud systolic murmur was heard in the precordia; and it was distinct at the level of the left nipple, between that body and the sternum, and over the upper part of the sternum; but was only feebly audible below the mamma. The second sound could not be detected. The liver was enlarged; the urine high coloured, of the specific gravity of 1015, and not albuminous. He was directed to take a combination of blue pill, colchicum, and alkalies, with aperients.

On the 2d of August the following notes were taken. He is losing strength, and the slightest exercise induces perspiration. His ankles swell during the day, when he is sitting up, and are at all times somewhat tumid, though he does not experience any pain or tenderness on pressure. The face is puffy. During the last three nights he has suffered so severely from pain extending across the epigastrium and right hypochondrium, that he has been compelled to sit upright in bed. The pulse is 100, and somewhat unequal in force. On percussion the chest possesses its usual resonance, and the respiratory sounds are natural. The dull space in the region of the heart is not materially enlarged. A loud and rough systolic murmur is audible over the whole precordia, feebly at the apex, but very distinctly at the base along the upper part of the sternum, and especially on its right side in the neck, and at the upper part of the chest behind. No second sound can be perceived. There is a general fulness of the neck, but no jugular pulsation or distension. The impulse of the heart precedes the pulse at the wrist by a distinct interval, and is disproportionate to it in power. He was directed to have a blister applied between the shoulders; the mercurial was

continued, and a diuretic mixture with colchicum and hyoscyamus was prescribed. From these means he obtained some relief; but on the 13th he was found sitting up in bed, and complaining of the severity of the epigastric pain, and he stated that he had not lain down for several nights. The notes taken were as follows:—The difficulty of breathing is very great; he has a severe cough and expectorates a small quantity of mucus of an adhesive quality and russet colour, and containing small air-bells. The abdomen is tumid, and there is tenderness on pressure in the right hypochondrium. The face is puffy, and the tongue covered with a whitey-brown fur. The pulse is feeble—120 in the minute. The breathing is short and hurried. The right dorsal region, below the level of the scapula, sounds dull on percussion, and the respiration is somewhat tubular. In the corresponding situation on the left side, small crepitation is heard. The urine is scanty, and has a specific gravity of 1012. It is dark-coloured, and when tested by heat and nitric acid, becomes opaque. The gums are affected with the mercury. He was ordered small dozes of the potassio-tartrate of antimony, and a blister was applied to the chest. The disease, however, continued to progress; the right dorsal region became entirely dull on percussion, and loud bronchial respiration was there heard; and similar signs, though less marked, existed on the left side. The sputum contained a considerable intermixture of bright blood; the pulse became very quick and feeble, and the prostration of strength increased. Ammonia, with decoction of senega and wine, were exhibited, but he continued to sink, and died on the 19th.

The *post-mortem examination* was not performed till the 21st, and the body was then much decomposed. The brain was healthy; the right lung much engorged and solidified in its posterior and inferior portions; the left lung was also much congested, especially posteriorly and inferiorly.

The heart was larger than natural, weighing about twelve ounces avoirdupois. The right auricle was dilated, and the right ventricle was also somewhat large, and its walls unusually thick. The auriculo-ventricular and pulmonic valves were healthy. The left auricle was enlarged, and its wall much thickened, and in the appendage there were two small sacculi formed by the protrusion of the lining membrane through the muscular fibres, the largest of which would admit the end of the fore-finger. The left ventricle was more capacious than natural, and its walls were moderately hypertrophied. The mitral valve was thickened and opaque, and some small shreds of lymph adhered to the ventricular surface of its free fold. The aortic valves were very much diseased; the angles of attachment and contiguous sides of the several segments being adherent, so that the capacity of the orifice was very much diminished. The adhesions between the right and posterior curtains were most extensive, and the septum so produced was considerably thickened, and so indurated and ossified as to form an unyielding mass

projecting into the aortic orifice. At the base, and somewhat to the left side of the attached margin of the posterior valve, there was a small aperture which led into a cavity situated in the substance of the septum of the ventricles. A second and larger opening, situated in the adhesion between the right and posterior valves, led into the same cavity, and from it there also existed a communication with the right sinus of Valsalva, but it was doubtful whether this had not been the result of the changes which had taken place after death. The cavity was of sufficient size to lodge a hazel nut. It contained decolorised coagula, and was lined by a distinct membrane, and had firm and resistant walls; and the commencement of the right coronary artery crossed its upper surface. The sac was embedded in the septum of the ventricles, and the muscular substance by which it was separated from the cavity of the right ventricle, or, more properly, from the right auriculo-ventricular aperture, was of a deep leaden hue and much softened. The ascending portion of the aorta was considerably dilated, but its coats were free from atheromatous deposits.

The liver was much enlarged and indurated. The spleen was also large and somewhat solid. The kidneys contained a small proportion of opaque yellow-coloured deposit, in the form of somewhat larger granules than usual.

This case illustrates the origin of obstructive disease in malformation of the valves. Notwithstanding the absence of any symptoms of disease of the heart till so short a period before death, there can be no doubt that the changes in the valves had been long in progress, and that the cavity in the septum of the ventricles had also been of slow formation. At the time when the slight rheumatic symptoms occurred, eleven weeks before his admission into the hospital, the malformed and thickened valves probably became slightly inflamed, and to this superinduced disease the fatal result may be more immediately referred. It is not likely that in this case there was regurgitation from the aorta into the ventricle, for the opening from the small sac into the sinus of Valsalva might have originated from decomposition after death, and, if not so caused, but the result of previous disease, it would only have created an indirect channel which could have transmitted but little blood. Had life been much prolonged, a communication would have been formed from the left ventricle into the right ventricle or auricle, by the gradual enlargement of the small aneurismal sac.

CASE III.—*Malformation of the Aortic Valves—Imperfect Expansion of the united fold at the point of fusion—Former attacks of Rheumatism—Regurgitation from the Aorta into the Ventricle—Defective condition of the Mitral Valve.*

H. D., æt. 40, a porter, but formerly, during ten years, a sailor, was admitted an out-patient of the Royal General Dispensary,

Aldersgate Street, March 1, 1845. He stated, that eleven years before he had had a severe attack of rheumatic fever, with which he was confined to bed eleven weeks, and he had ever after been subject to rheumatic pains. Though he had committed occasional excesses, he asserted that he had never been very intemperate. For the last four years he had been liable to affections of the chest during winter, and this he ascribed to working in a warehouse over a damp cellar, from which a vapour constantly arose. About three months before, his health became more impaired; he suffered from cough, difficulty of breathing, and palpitation of the heart, and more recently his face and extremities had become swollen. The following notes were taken at the time of his admission. His face is now more puffy than it has been, and the lower extremities are very œdematous. His breathing is very difficult, and, for the last two days, he has expectorated much blood. He complains of palpitation of the heart, and is incapable of lying down, but has no pain. He thinks he passed yesterday fully three pints of urine, but occasionally he makes very little. The urine has a specific gravity of 1010, and is not albuminous. The pulse is 87, and intermits every twenty to thirty-five beats; it is large and sharp, and stronger at the left than at the right wrist. The respirations are thirty in the minute. There is very evident pulsation of both carotid arteries, and also of the right jugular vein. The chest is throughout less resonant than natural, and especially so low down, and on the left side. The cardiac dulness commences on the right margin of the sternum at the level of the third cartilage, and thence extends downwards, and laterally to a little beyond the left nipple. The apex of the heart beats between the sixth and seventh left ribs, an inch and a-half below the nipple, and slightly outside a line drawn from that body. In front, over the right side, the respiratory murmur is distinctly heard, and on the left side it is attended with some sonorous rhonchus, and with distinct crepitation; respiratory sounds are inaudible in the dorsal regions. At the level of the nipple, and mid-way between that body and the sternum, a slight bellows murmur is heard with the impulse of the heart, and this is followed by a short rasping bruit. Towards the apex, the first murmur increases in intensity; but it is also loudly heard and is of a rough character, in the course of the aorta and in the carotid arteries. The pulse at the wrist follows the impulse of the heart by a very distinct interval. He was directed to take alterative and diuretic medicines.

March 12th.—The difficulty of breathing is much greater, and in the night amounts to complete orthopnœa. His face, arms, and feet, are more swollen, and he continues to expectorate mucus deeply tinged with blood. The dulness on percussion on both sides of the chest is more extensive, and subcrepitant and mucus rhonchi are heard in all parts. The double murmur is heard as before in the præcordia, but is more distinct. The urine is scanty, and has a specific gravity of 1015, and exhibits a slight trace of

albumen, with the usual tests. In addition to the former medicines, an antispasmodic and anodyne mixture was prescribed.

March 19*th*.—All his symptoms were greatly aggravated; he had passed a very disturbed night, being slightly delirious, and moaning incessantly, and labouring under extreme difficulty of breathing. On the afternoon of the 20th, I found him in the agonies of death. His face was much swollen and livid. He was partially insensible, though capable of being aroused. The difficulty of breathing was extreme, and he expectorated a large quantity of muco-purulent secretion intermixed with dark blood. The pulse was scarcely perceptible at the wrists, and his hands were cold and livid. He had not made any water during the whole day. He died at five P.M.

The *post-mortem* examination took place at one P.M. on the 21st. The right pleural cavity contained a considerable amount of fluid, and the membranes adhered slightly by old attachments at the posterior and upper part. The right lung was voluminous, and its anterior margin somewhat emphysematous. It was less crepitant than usual, being much loaded with serum. The left lung was universally and very firmly adherent by old cellular membrane. It was of small size, sparingly crepitant throughout from the quantity of serum which it contained, and much congested posteriorly and inferiorly. On section much thin whitish coloured secretion escaped from the divided extremities of the smaller bronchial tubes of both lungs. The right bronchus and its branches were greatly dilated, the left somewhat larger than usual. The mucous membrane was throughout of a deep purple hue, and the tubes contained much secretion of a pale yellowish-white colour.

The pericardium was universally adherent by old and firm cellular attachments. At the base and upon the auricles, the adhesions seemed more recent, and several small ecchymoses, or extravasations of blood, were found on tearing the membranes apart. The heart was of very large size, weighing sixteen ounces and a-half avoirdupois. The right auricle was dilated, and its walls considerably thicker than usual. The right ventricle was also large. Its walls were much hypertrophied, being three eighths of an inch in width, and did not collapse on section. The tricuspid aperture was greatly expanded, allowing of the passage of a cylinder five inches in circumference, and the valves were evidently incompetent to close the opening. The pulmonary orifice had a capacity of three inches and six lines, and the valves were healthy. The left auricle was large, and much increased in thickness, and the left ventricle was very greatly dilated and hypertrophied, the walls measuring near the mid point seven-eighths of an inch in width. The mitral aperture was three inches and six lines in circumference, and the valves were opaque and much thickened, especially at their free edges. The cordæ tendineæ were thick and somewhat short. The aortic aperture was three inches and nine lines in circumference.

The right and posterior similunar segments were united by their contiguous sides so as to form one valve of much larger size than the other. The union between the two segments was evidently congenital, and the point at which it had occurred was marked on the ventricular side by a furrow running from the attached margin to the free edge of the valve, and on the aortic side by a distinct septum or frænum having a similar course. The part of the malformed valve at which the fusion had taken place, had not expanded to an equal degree with the rest of the fold, so that the margin of the valve displayed a concavity in that situation; and when the valves were closed, there remained a triangular aperture through which a column of water, poured into the aorta, freely regurgitated into the ventricle, and this defect was increased by the general yielding of the curtain and angles of attachment of the valve, so as to allow it to fall below the proper level. The two valves were also opaque and considerably thickened, the thickening being most marked at the free and attached margins. Fig. 4 in the plate represents the state of the valves in this case after the aperture was laid open, and Fig. 5, though drawn from another preparation, well indicates the appearance of the valves before division, with the exception, however, that the opening there represented, is larger than that which existed in this case. The aorta was dilated throughout its thoracic portion, as were also the branches given off from the arch. The coats of the vessels were throughout thin and slightly atheromatous. The liver and spleen were large, firm, and much congested. The kidneys were large and solid, and, on section, the striated portions had a mottled and coarsely granular appearance. A small quantity of serum was contained in the abdomen.

The case which has just been related affords a good example of imperfection both of the aortic and mitral valves; the former being predominant, and originating in congenital adhesion of the contiguous sides of two of the segments, by which the valves were reduced to two in number. This malformation, however, appears not to have interfered with the function of the valves so long as they were free from disease, and the orifice retained its natural size. For, though the compound segment had not equally expanded in all parts, but was much narrowed in the situation where the fusion had taken place, the two valves were yet sufficiently large to close the orifice. During the attack of rheumatic fever which occurred eleven years before death, the valves probably became indurated and thickened, and this must materially have prevented their adapting themselves to any subsequent change in the capacity of the orifice. The immediate occurrence of regurgitation must, however, be ascribed to the enlargement of the orifice, and this probably took place only a short period before the patient first came under observation. The valves being prevented by their thickened and unyielding state from undergoing further enlargement, an aperture was then left between the edge of the less expanded part of the larger valve and the other

segment. Notwithstanding, however, the free regurgitation which had been allowed to take place, the edge of the valve was not retroverted, the great thickening of the valves having probably been sufficient to prevent the retroversion. The united fold had also yielded generally so as to fall below the level of the other valve. The mitral valve was somewhat incompetent, for while the orifice was increased in size, the valve retained its natural dimensions, and the physical signs observed during life pointed to incompetency, both of the mitral and aortic valves.

The three cases which have just been related afford examples of the predisposition in the valves, when malformed, to take on diseased action, and the changes which, under such circumstances, they undergo. In the first case, the disorganisation of the valves was probably due to the occurrence of subacute endocarditis, caused by violent mental excitement. In the second case, the great obstruction to the flow of blood from the ventricle resulted from slow thickening and induration, and the immediately fatal result was occasioned by more acute inflammation developed during a slight rheumatic attack. In the third, regurgitation was established by the dilatation of the orifice after the imperfectly expanded valve had become thickened from inflammation, probably during a severe attack of rheumatism. The cases afford, therefore, examples of several of the forms of disease to which the aortic valves, when malformed, are exposed. They do not, however, indicate the frequency with which malformation acts as a predisponent to valvular disease. The importance of this cause will, however, be seen when it is stated that of thirty of aortic valvular diseases, of which I possess full notes, not less than five may be regarded as having so originated; and during the last and present sessions of the Pathological Society, specimens of disease arising from malformation, have been exhibited by Dr Bennett, Mr E. Pye Smith, and Mr Obré. I believe, indeed, that a careful investigation would prove this cause to operate more frequently than is generally supposed; and that where symptoms of valvular disease present themselves in persons at early periods of life, who have not suffered from severe rheumatic attacks, or experienced any strain or injury, they will generally be found to depend on malformation of the valves.

ARTICLE II.—*On the Pathology of the Bronchio-Pulmonary Mucous Membrane.* By C. BLACK, M.D., Chesterfield, Bachelor of Medicine, and formerly Medical Scholar in Physiology and Comparative Anatomy in the University of London; Fellow of the Royal College of Surgeons of England, etc. etc.—(*Continued from page* 300.)

Chronic Bronchitis.

BUT one pathological condition of the bronchio-pulmonary membrane is characteristic of the chronic form of bronchitis; and this

condition corresponds with that which has already been described as the second of the more acute forms of the disease. It, in fact, is a continuance, in a minor degree, of the second pathological condition of the acute. It has already been shown, that such pathological condition is characterised by partial epithelial desquamation of the bronchial membrane, increased mucus-cell growth, and by the occurrence, into the submucous tissue and upon the denuded surfaces of the basement membrane, of an exudation, which, in virtue of its inherent vitality, is capable of undergoing a development into cells. Certain phenomena, having particular reference to the physical condition of the pulmonary tissue and to the respiratory function, were also shown to have their immediate dependence on the particular pathological state of the membrane described.

Now, in chronic bronchitis, this condition of the membrane and its attendant phenomena still continue; but the inflammation has, in a great measure, lost its severity, whilst the phenomena are proportionately modified or influenced by such an occurrence, and by the degree of structural effect which a continuance of inflammation has already produced in the bronchial tissues. Owing to the continuance of capillary engorgement, and consequently to a persistent deficiency in the vital tonicity of the bronchial vessels, an increased exudation is being constantly poured forth both into the submucous tissue and upon the denuded surface of the basement membrane. This exudation, as regards the submucous tissue, is not entirely removed by the process of cell-development and of cell-decay before described; but, on the contrary, a part of it, after having acquired the plastic-cell development, is assimilated to the surrounding tissues, which are thereby slowly and gradually hypertrophied. In like manner, the exudation into the substance of the basement membrane is not entirely dissipated by the increased cell-growth which is continually taking place on its free surface, in consequence of which a gradual thickening of this structure occurs, and thereby leads to a diminution in the calibre of the affected tubes. With this hypertrophy of the basement and other bronchial structures, the increased flow of nutritive blastema towards the free surface of the former conjoins increased activity of that portion of the epithelium which is yet entire, and maintains a constantly morbific action in those parts which remain destitute of their epithelium. Hence, at the free surface of the bronchial membrane, there are an increased production of mucus-cells, an occasional hypertrophy of the epithelium—to be afterwards considered—and the constant growth, in the exudation-plasma, of the particular structures which characterise the presence of inflammation. The appearance of the membrane on inspection accords well with the pathological condition described, and exhibits throughout the affected part an increased coloration, which, however, is of a deeper hue where the basement structure is denuded than in parts yet protected by epithelium. The membrane, therefore, seems, to

the naked eye, to have suffered in isolated parts of its surface, and as it were, in patches; but this deceptive appearance is, as I have before said, entirely owing to the permanent shedding of epithelium in one, and to its due maintenance in another, part of the bronchial structure. Those parts which are destitute of their epithelium, sometimes present a fine, smooth, glazed surface, which is covered by a thin film or layer of a grayish-white, tremulous substance, beset with numerous minute points. This substance consists entirely of exudation, which is moulded to the form of the surface on which it is poured out, and which might readily be mistaken for a superficial layer of basement structure itself by any one unaccustomed to note the difference. This difference consists in the slightly irregular, free surface which the exudation presents; but more particularly in the great number of its nuclear points, in the irregular distribution and frequent crowding together of these, and in the disposition which they manifest to the characteristic exudation-cell development. Other parts of the bronchial membrane, also destitute of their protecting epithelium, present a slightly rough, irregular surface of their denuded basement structure, which is owing to the existence of a molecular coagulation of exudation-plasma. Each of these attached molecules is seen, under the microscope, to be the nucleus of an exudation-corpuscle; and hence the origin of such cells in an isolated manner. Such isolated molecules have generally a firmer attachment to the basement membrane than has a mass of coagulated exudation; therefore, as the former retain their position, and consequently maintain the necessary conditions of growth longer than the latter, from them plastic and pus-cells are more particularly produced. Other parts, again, of the bronchial membrane, have not only lost their epithelium, but also a portion of their subjacent basement structure, which latter may either consist of its most superficial layer or of its whole thickness (Fig. 11.) Such basement patches are distinguished by the general evenness and smoothness of their epithelial surface; but more especially by the relative position of their nuclei, which are placed at nearly equal distances from each other. When, therefore, a structure having the above characteristics is observed in the sputum, it may be safely regarded as a basement patch; but when a body, having a similar configuration, but with nuclei crowded more or less together over its whole surface, presents itself, we are not thence to conclude that it is not of basement origin; inasmuch as it is certain, that the excess of nutritive blastema, with which the basement membrane is surcharged during bronchial inflammation, by coagulating in innumerable points on the free surface of the latter, destroys, as it were, the particular arrangement of its nuclei, and endows it with all the physical appearances, when detached, of as much coagulated exudation.

Under the former aspect, then, it is easy to distinguish a basement patch; but under the latter, it is a thing quite impossible.

The presence of basement patches in the sputum is indicative of

basement ulceration; but their absence is by no means diagnostic of the integrity of that structure, inasmuch as the latter may become so charged with inflammatory exudation, that when such exudation undergoes cell-development, it may thereby break up the basement membrane in the same manner as tuberculous deposits will hereafter be shown to break up the pulmonary tissue, and that, too, into such minute portions that the original structure admits no longer of recognition. Hence, from the above facts, the advantage to be derived, in reference to diagnosis and prognosis, from microscopic examination of bronchitic sputa.

Besides the above-mentioned conditions of the bronchial membrane, we find that the latter, in consequence of the hypertrophied condition of its tissues and of its continued vascular engorgement, presents a generally puffy, swollen appearance, throughout the extent of surface involved—that the submucous tissue is thickened by the deposition and organisation of plastic exudation—and that, in many instances, the fibrous and cellular coats of the bronchi partake of the general hypertrophy. These appearances, however, will be modified by the extent and the degree of inflammation which have existed from the first, by the period of its duration, and by the degree of constitutional power which exists. When the extent of surface involved is great—when the inflammation inclines to the acute character—and when it has existed for several months—most of the above-described conditions of the bronchial tissues are observed; but when the inflammation is of slower progress, and when, instead of several, it has existed many months, the above appearances of the bronchial membrane give place to a minor degree of coloration and of puffiness, yet to a manifest thickening, consolidation, and to more or less firmness and rigidity of the bronchial tissues generally. This last-mentioned result is due to the progressive organisation, for a time, of the exudation into the submucous tissue and the structures exterior to it, during which process of organisation the exudation acquires greater firmness and solidity, thereby contracting its bulk, and thus exerting a degree of pressure on the walls of the vessels, by which their diameter is lessened, and the quantity of blood in them is diminished in comparison with that of the earlier stage of the disease.

Chronic bronchitis, allowed to take its own course, always manifests a tendency to extend; therefore, this tendency being maintained, exudation, which at once becomes more or less organised, ultimately accumulates to such an extent in the submucous and other exterior tissues, that on its perfect organisation taking place, a complete amalgamation and matting together of such structures occur, so that further exudation into these parts is impossible. But as the exudation into the basement structure of the bronchial membrane is being continually removed by cell-growth and by the escape of such exudation from its free surface, the products of inflammation find an outlet in this direction, and thus prevent complete compression and consequent

obliteration of the capillaries, which would otherwise occur from their complete immersion in exudation-plasma, and the pressure exerted during its subsequent organisation and contraction. But, in all such cases, the quantity of exudation received by the basement structure exceeds that of the discharge from its free surface, in consequence of which an actual encroachment, although in some instances a very gradual one indeed, is made upon the diameter of the affected tubes. This tendency to occlusion of the bronchi is more particularly manifested in cases of chronic capillary bronchitis attended by a fair amount of constitutional vigour; and the period of life which, from numerous cases, appears to be the most favourable to such an occurrence, extends from the forty-fifth to the sixtieth year. During this period, chronic bronchitis, in persons otherwise healthy, manifests a decided and progressive, yet gradual, extension towards the pulmonary cells, the epithelium and basement membrane of which sooner or later become implicated, thereby modifying, in proportion to the degree and extent of pulmonary tissue invaded, the respiratory function, and, through it, the nutrition and functional activity of the body generally.

Bronchitis becoming chronic after the latter of the above periods, rarely indeed leads to structural hypertrophy of the tissues involved. On the contrary, owing to the natural disposition to decay which now, in obedience to the immutable laws of nature, pervades every tissue—to the consequent excess of molecular disintegration as compared with molecular nutrition—and to the looseness and laxity of tissue thereby occasioned, it assumes more particularly the character of atonic inflammation of the bronchi; whilst, owing to the deficiency of the nutritive principles of the blood as compared with earlier life, the products of this pathological condition exhibit, as will hereafter be shown, but few of the characteristic structures of inflammation. There are, therefore, various shades of chronic bronchitis, some of which are characterised by a certain degree of constitutional power; others, by the sheer debility attendant on natural decay. The former cases are liable to the sudden supervention of acute attacks of bronchial inflammation; whilst the latter are not thus influenced. Again, the former may be limited in the extent of the membrane involved; nevertheless they invariably extend; the latter are more general in the extent of surface affected, but they are always stationary. Of the former kind, plastic bronchitis is an extreme example; of the latter, senile bronchitis. These extremes approach each other by cases of intermediate severity, each of which, by the character of its sputa, indicates, to a considerable extent, the particular pathological condition of the bronchial membrane which exists. To these kinds of sputa particular attention shall be given after the static and dynamic phenomena of the chest, as dependent on chronic bronchitis, have been examined.

In all cases of chronic bronchitis the bronchial membrane has sustained more or less of injury, which is manifested during life by a

modification of the respiratory function and of the natural secretion from its epithelial surface.

The modification of the respiratory function is induced by the swelling of the bronchial tissues generally; but more particularly by continued vascular engorgement, by hypertrophy of the basement structure, and by plastic exudation into the submucous tissue. Hence occurs, as a necessary consequence of such thickening of parts, a corresponding diminution in the caliber of the affected tubes, and, as a further consequence of such diminution, a deficient passage of air during inspiration. The pulmonary cells, therefore, to which such bronchi lead, are imperfectly inflated by each inspiratory movement; and this deficient inflation having, as a necessary consequence, a corresponding density of the pulmonary tissue, produces a diminution in the percussion-note of the chest immediately over the site of the affected parts. This modification of the percussion-note, however, depends entirely, as to its degree, upon the particular bronchi affected, the extent and degree of their narrowing, and the quantity of vesicular tissue thus influenced. Complete or partial occlusion of a large bronchus affects a greater extent of pulmonary surface than does the same condition of a smaller bronchus, simply because it gives off a greater number of branches, and thereby becomes the channel of communication with a greater extent of vesicular tissue. But, fortunately, it is seldom that a bronchus of the first or second magnitude becomes closed from the effects of inflammation; therefore it is to tubes from the third gradation in size downwards that we look for such occurrences. If these are few and perfectly closed, whilst other neighbouring tubes are more or less narrowed, the percussion-note is manifestly lower than natural; but if the neighbouring tubes are not affected—an occurrence seldom, indeed, to be expected—the lobular collapse, although of itself it diminishes the natural resonance of the affected part, yet, by occupying less space, and thereby offering less resistance to the surrounding pulmonary tissue, permits the cells of the latter portion to enlarge under the compensatory action which is now required of them, to occupy the vacancy produced by the collapsed portion of lung, and thus to mask or drown entirely the diminished resonance of the latter. When, on the other hand, without perfect obstruction of any, there is a partial narrowing of numerous, bronchi, and of the corresponding pulmonary cells, owing to the extension of disease to the substance of the walls of the latter, the percussion-note partakes of that generally diffused and partial dulness of character, which indicates the presence of a less quantity of air in the lung beneath than exists in the healthy condition of that organ. To compensate for this deficiency, the air enters the cells of the yet sound portions of lung in unusual quantity, and, by inflating them to an extent beyond that of healthy inspiration, gradually enlarges them, and ultimately gives rise to vesicular emphysema. The percussion-note, therefore, on the confines of the diseased lung, and, if the disease of the latter is extensive, in the

opposite lung, if sound; or in the upper portions of both lungs, if both their lower portions are diseased, is raised in its pitch above the standard of health. At the outset, too, of this unusual activity of the sound portions of lung, an increased loudness of the vesicular murmur is produced by the unusual pressure of air upon the interior of the cell-walls during the inspiratory act, and by a corresponding resistance thereby excited in the fibres of the cell-walls themselves. These two forces, acting in opposition to each other to an unusual extent, generate a murmur characterised by the term "puerile respiration;" but which would be more correctly designated by that of "augmented vesicular murmur." If, however, the process of narrowing continues in the affected bronchi and corresponding pulmonary cells, a still further degree of functional activity of the sound portions of lung is required, an increased muscular effort is made to expand the chest, a correspondingly increased volume of air presses into the pulmonary cells, which ultimately yield to the distending force, enlarge, and consequently offer but little resistance to the entering air, thereby changing the character of the resulting murmur to that of a reduced crackling and rubbing, as recognised in emphysema. It is evident that, if in the sound portions of lung an increased pressure of air into the pulmonary cells on the one hand, and an increased resistance to such distension by the cells on the other hand, generate a proportionately increased vesicular murmur, the opposite conditions in the diseased parts of the lung will give rise to a corresponding diminution of the respiratory sound, as referred to the standard of health. The deviations, therefore, in the vesicular murmur, with respect to this standard, are dependent on the volume and velocity of the entering air, and on the opposition offered to the latter by the contractile power of the pulmonary cells; and these latter conditions are again dependent on the degree of narrowing of the bronchi—on the state of the cell-walls—and on the power of the inspiratory muscles. If the bronchi have undergone a rather considerable diminution of caliber—if, nevertheless, the vesicular tissue is as yet free—and if the power of the inspiratory muscles still remains undiminished, as is the case in the early stage of chronic bronchitis in persons from forty-five to fifty-five years of age, the entering air presses with sufficient force into the pulmonary cells to produce a distinct, although generally a diminished, vesicular sound. But if, with considerable narrowing of the bronchi, there are a corresponding thickening of the cell-walls, and a deficient muscular power, as occur in cases of eighteen months' standing and upwards, the vesicular murmur is frequently so far reduced, that it is only heard on a forced inspiration; whilst the increased density of the lung, by augmenting its conducting power, transmits the sound of bronchial respiration and of the voice intensified to the walls of the chest.

In the extension of diseased action from the lining membrane of the bronchi to that of the pulmonary cells, there is a stage in which

the walls of the latter are thickened, and the action of their fibres thereby impeded, not destroyed—in which case the inspiratory sound is feeble and somewhat harsh; whilst the expiratory murmur is manifestly prolonged as in the first stage of pulmonary consumption. But the nature of the cause, in each case, will be satisfactorily determined by reference to the age and constitution, the part of the lung affected, and the previous history of the case.

In the senile form of chronic bronchitis, the generally relaxed state of the bronchio-pulmonary membrane, the engorged condition of its blood-vessels, the consequent and proportionate reduction in the caliber of the tubes and pulmonary cells, and the feeble action of the inspiratory muscles, produce a general diminution of the vesicular murmur throughout the chest; whilst the occlusion of one or more bronchi, by exudation or by inspissated mucus, completely destroys this murmur in the pulmonary cells to which such bronchi lead.

It has been already shown that, in the different shades of chronic bronchitis tending to the acute character, the tubes are narrowed by inflammatory exudation into the basement and submucous tissues, and by continued vascular engorgement. The air, therefore, on passing through these tubes, encounters more resistance than in the natural state of the parts, owing to which the bronchial note is absolutely raised in its pitch. If, in this pathological condition of the tubes, their caliber is still further reduced by a temporarily increased congestion of their membrane, produced either by exercise, and a consequently spasmodic action of their circular fibres, or by the latter alone, as occurs when a very cold, sharp air passes over the bronchio-pulmonary membrane denuded in part of its epithelium, and for the moment unprotected by its secretion, various sonorous, sibilant, and musical rhonchi are produced. The seat of their production corresponds more particularly with the points of epithelial or basement ulceration, because here the membrane is much more sensitive than in parts yet covered by epithelium, and consequently, any irritating qualities of the inspired air are more keenly felt, and the corresponding muscular fibres of the tubes thereby more energetically excited to contraction, than in other parts of the tubes where the membrane has undergone no such epithelial or basement ulceration. Such, indeed, is frequently the sensibility of these portions of the bronchio-pulmonary membrane, that the first exposure to a cold air, after the tubes have been cleared of their sputa by cough, is followed by a sensation of smarting in points of the affected bronchi. I have frequently heard this sensation described by intelligent patients as being similar to that which is experienced on the sudden exposure of an irritable sore to the cold air. In both cases the pathological conditions are identical; therefore the sensation is referrible to one and the same exciting cause—namely, to the action of oxygen on the exposed extremities of nerves rendered doubly irritable by disease.

A further modification of the natural auscultatory sounds of the chest is produced by the presence of fluid in the bronchial tubes. All the different shades of chronic bronchitis are accompanied by an increased discharge from the bronchial membrane; but in one variety only does it observe anything like a steady and uniform quantity. This is senile bronchitis, in which the constantly relaxed condition of the bronchial capillaries and of the membrane maintains a regular flow from the latter; whilst the other varieties of chronic bronchitis are more or less liable to sudden supervention of acute inflammation, and to various degrees of vascular engorgement and of vital tonicity in the capillary walls, by which the quantity of exudation, and of the natural secretion of the membrane, is from time to time altered.

The presence, however, of fluid in the bronchial tubes, furnishes a condition for the production of the characteristic moist rhonchi described under the pathology of the acute forms of the disease.

The extent of surface over which they are heard, will depend on the extent of membrane affected; whilst the quantity and tenacity of the sputa, and the velocity with which the air enters and emerges from the bronchi, will determine the duration and quality of such sounds.

In a moderately copious exudation and secretion, as observed in chronic bronchitis aggravated by a fresh accession of inflammation, the rhonchi are decidedly "mucous" in their duration and quality; in a still less copious, yet more consistent, exudation and secretion, accompanied with the usual effort of inspiration in such cases, the rhonchi partake more of the submucous character; whilst a very scanty exudation and secretion, as are frequently observed in chronic bronchitis in vigorous patients during the summer months, neither give rise to a mucous nor yet a submucous rhonchus. Such scanty exudations take place on the surface of isolated portions of denuded basement membrane, and are moulded, with the secretion of the surrounding epithelium, into pellets of a consistent and tenacious character, which do not permit the entering and emergent air to permeate them, and to constitute with them a series of bubbles, as in the more fluid forms of sputa; but which are slid up and down the bronchi by the entering and emergent columns of air; and thus, according to the degree of obstruction which they produce, give rise to a shifting, cooing, chirping, or clicking note. Sometimes, by a forcible inspiration following a fit of coughing, a pellet of the above kind is carried into one of the smaller bronchi, which it completely obstructs, and from which subsequent efforts fail to dislodge it. Hence arises lobular collapse of the portion of lung beyond the seat of obstruction. Such accidental occurrences, however, form the exceptions rather than the rule, according to which bronchial obstructions take place. Such obstructions have been before shown to depend on inflammation and narrowing of the bronchi at the seat of obstruction—on exudation upon the denuded surface of the base-

ment membrane at this point—and on its subsequent coagulation in the form of a cast, which obstructs the caliber of the tube.

In reference to the breathing, as influenced by the pathological condition of the bronchio-pulmonary membrane in chronic bronchitis, the deviations from the healthy standard are, in the main, commensurate with the amount of disease, with the degree of narrowing which the bronchi have undergone, and with the quantity of fluid which they contain.

Hence this function, according to the different pathological states of the membrane already detailed, may be very slightly accelerated and laboured, so as scarcely to be noticed by the patient except on exertion; or it may, at all times, be so distressingly laboured as to excite a constant sensation of shortness of breath. The former occurs in the earlier stages of the disease in persons otherwise healthy; the latter, when chronic bronchitis has existed for several years, when the hypertrophy of the bronchial tissues and the consequent narrowing of the tubes are extensive in both lungs, when the cells have already become implicated by thickening of their walls; and, more particularly so, when bronchial obstruction and pulmonary collapse, together with vesicular emphysema, are conjoined. This constant sensation of the want of breath is also present when, with a generally relaxed and engorged condition of the bronchial membrane, there are a copious secretion and exudation into the bronchial tubes; but, in this case, the sensation is intermediate in severity between the two extremes just described. Again, when chronic bronchitis in persons otherwise vigorous has extended to the air-cells, producing chronic cellulitis, the breathing is liable to asthmatic paroxysms, either from a sudden congestion of the lungs, or from the inhalation of a very cold air, or from the accumulation of mucus in the bronchial tubes, as may happen during sleep. In such cases the morbid sensibility of the bronchio-pulmonary membrane, denuded in part of its epithelium, the hypertrophied condition of the bronchial fibre, and of the fibrous tissue of the cells, are favourable to the excitation and production of spasm on the application of any cause which, as sudden capillary engorgement, an accumulation of mucus, the inhalation of sharp, cold air, or of particles of dust, or of irritant gases, unusually irritates the bronchial fibres and the terminal extremities of the sensitive nerves of the membrane.

As the different pathological conditions of the bronchio-pulmonary membrane, and of the other component bronchial tissues, produce proportionate modifications in the static and dynamic properties of the chest, so it is found that they exert a similar influence over the character of the cough. Thus, in the early stage of chronic bronchitis, in which the effects of the disease are limited to epithelial ulceration, basement and submucous hypertrophy, the cough is more or less paroxysmal, easily excited, powerful, deep, and attended by a somewhat free expectoration. When, with the above pathological con-

dition, hypertrophy of the bronchial fibres is conjoined, the contraction of these is considerable in the act of coughing. The tubes are, therefore, unusually narrowed during the forcible expulsion of their contained air, which, being thrown upon the closed glottis in reduced, yet rapid, columns, forces this by a very narrow aperture, which consequently elevates the pitch-note, and gives to the cough a sharp, shrill, and ringing character. When, on the other hand, disease has invaded the pulmonary cells by gradual extension from the bronchial tubes, and when also emphysema is present, the volume of admitted air, having undergone a gradual reduction, and having, in a negative manner, induced imperfect aëration of the blood, and through it imperfect activity of the functions,—and, therefore, imperfect nutrition of the body, is neither forcibly expelled, owing to the debility of the expiratory muscles, nor is its expulsion energetically opposed by powerful contraction of the glottis; consequently, a partial effort only, in point of power, is made by the expiratory muscles during coughing; and this effort being feebly opposed by the constrictor muscles of the glottis, a low, muffled, and, as it were, half-suppressed cough, is the result. This kind of cough requires to be repeated in quick succession for several times before sputa of even moderate tenacity are expelled. For the same reasons, the cough of senile bronchitis is of a similar character; nevertheless, from the quantity of fluid present in the bronchial tubes, it (the cough) is generally looser and more muffled in its expression.

Another very striking and interesting phenomenon, dependent on the pathological condition of the bronchio-pulmonary membrane in chronic bronchitis, is, the proportionate ratio of reduction in the weight of the body to the gradual extension of the disease, and to the consequent encroachment on the respiratory function. As the bronchial tubes narrow, the pulmonary cells become invaded, bronchial tubes obstructed, and pulmonary collapse and emphysema arise, the breathing-power of the lungs is proportionately diminished, —the consequence of which is, that the blood is imperfectly aërated, and the different functions are but very languidly performed.

Hence defective appetite, deficient assimilation, and consequent thinning of the body. This last-mentioned result is further favoured by the absolute drain from the affected membrane. In this way the volume of blood in the system is reduced to a quantity equal to the distributive capacity of the pulmonary capillaries; and after this equilibrium has been established, the body ceases to waste, if the progress of the disease is suspended. This statement, based on the fact, that for the due maintenance of the nutrition of the body, the quantity of blood circulating through the lungs must bear a certain ratio to that which is passing through the aggregate of the remainder of the body, negatively asserts, that the quantity of blood in the system exceeds the distributive power of the pulmonary capillaries; and that, consequently, disease in the lungs and wasting of the tissues have not observed an equal ratio; or, in other words, that

the former has outstripped the latter. Such an occurrence, notwithstanding its occasional existence, is the result of accident rather than of the absolute progress of disease; and it arises from any cause, which, acting at one point, may materially affect the function of parts beyond that point, as happens when the occlusion of several bronchi destroys or suspends the breathing-power of the pulmonary lobules to which they lead. The pulmonary collapse thus induced, does not permit the usual quantity of blood to pass through the capillaries of the affected lobules; and if the deficient circulation here cannot be compensated for by the other capillaries of the lungs, the balance between the pulmonic and systemic circulations, which is necessary to maintain the body at its present weight, is destroyed, and the volume of blood in the system must fall, and the tissues consequently waste, until this lost balance between the two circulatory systems has been restored. Hence the reason why sudden aggravations of chronic bronchitic inflammation are productive of considerable loss of weight during the first two or three weeks after their accession; and hence, also, the explanation of the cessation of this progressive reduction so soon as the required oxygenation of the system is lowered to the capacity of the respiratory function. In the early stages of chronic bronchitis, and more particularly in persons of otherwise vigorous constitution, the relative balance between the pulmonic and systemic circulations, and, therefore, the required ratio between the respiratory function and the nutrition and waste of the system, is continually fluctuating under the frequent accessions and partial cessations of disease in the bronchial membrane. Thus, under an aggravation of the disease, the circulation through the affected membrane is, after the first few hours, retarded, and ultimately stops; the breathing-power of the lungs is proportionately reduced; the respiratory function is correspondingly diminished; and the consequent imperfect aëration of the blood renders the latter fluid unable to maintain the activity of those molecular changes of the ultimate tissue, and the energy of those functions which are necessary for the healthy destruction and reparation of the body as it now stands. Destruction, therefore, proceeds; reparation is suspended; and as the cause has been sudden and quick in its operation, so the wasting of the body will be more or less rapid, until the pulmonic and systemic circulations, and, therefore, the respiratory function and ultimate organic nutrition again observe the relative ratio of health. Hence the system must waste for a time after the sudden aggravation of a chronic bronchitic inflammation. By the same rule, all things being equal, the body must increase in weight under a marked amendment in the pathological condition of the bronchial membrane; because the respiratory function is enlarged, the blood is more effectually oxygenated, the stimulation of the nutritive functions is thereby increased, and ultimate nutrition consequently exceeds ultimate decay.

With respect to the different sputa which are observed in chronic

bronchitis, they admit of four varieties, which depend on the particular pathological condition of the affected membrane, on the age, and constitutional powers. These I shall refer to the respective heads of—

1. Sputum of Plastic Bronchitis.
2. Sputum of the early stage of Simple Chronic Bronchitis.
3. Sputum of the latter stage of Simple Chronic Bronchitis.
4. Sputum of Senile Bronchitis.

Sputum of Plastic Bronchitis.

This variety of sputum consists essentially of solid or tubular portions of exudation, mingled more or less with exudation-cells, mucus-corpuscles, and frequently with blood discs. The solid or tubular portions, on microscopic examination, are seen to consist of a fibrillar or an amorphous substance, interspersed with exudation and plastic corpuscles—the former being the more numerous. The length of these casts varies from portions of $\frac{1}{1000}$th of an inch to that of several inches, in which latter case there are several branches corresponding with the divisions of the bronchi from which they have been expelled. They are usually of a bluish-white, or dirty yellowish-white colour, of a soft solid consistence, and are frequently streaked with blood on that surface which was attached to the bronchial membrane. When tubular, their sides collapse after expulsion, and present the appearance of a flattened band, frequently bent and twisted upon itself; but when solid, they retain the figure and diameter of the tubes in which they were moulded. On a careful microscopic examination, the tubular casts are frequently seen to resemble, in their anatomical structure, a mucous membrane, and to consist of a fibrous tissue, on which rests a basement structure, surmounted by an imperfectly developed epithelium.

The pathological interest attached to plastic bronchitic sputum, refers to the formation of the particular casts which are present. These are evidently the result of inflammatory action in the bronchial membrane, under which the whole extent of surface affected casts its epithelium; and upon the basement structnre thus laid bare, a slow exudation is poured out, which coagulates, and subsequently undergoes a partial development. The existence of casts *having* ramifications is characteristic of the chronicity of the disease; for, were the morbid action otherwise, the great quantity of permanently fluid exudation, which is always present under such circumstances, would wash away the fibrinous portions of the exudation in detached and coagulated masses, and in small bronchial casts, as occurs in acute sthenic bronchitis.

Plastic bronchitis may or may not be connected with a particular diathesis. When it is not so connected, the bronchial casts manifest more or less of a fibrillar arrangement, interspersed with exudation-cells from $\frac{1}{2500}$th to $\frac{1}{4000}$th of an inch in their diameter, and with

plastic corpuscles of nearly full growth. The last-mentioned bodies, however, are perhaps never assimilated to the tissues on which the morbid exudation rests. On the contrary, if the casts are not expelled after plastic cells have been formed, the action of the inspired air, aided by the oxygen of the blood, and by the heat and moisture of the affected part, converts them into pus-cells, as shown in the explanation of the origin of these bodies when treating of acute bronchitis.

When plastic bronchitis is connected with any particular diathesis, the latter is of a tuberculous character, in which case the exudation casts manifest but a very scanty, fibrillar arrangement, and are rather distinguished by an amorphous or granular condition of the exudation, which likewise shows a diminutive cell-development, varying from $\frac{1}{5000}$th to $\frac{1}{10,000}$th of an inch in diameter. This comparative absence of fibrillar arrangement, and the diminutive cell-growth with which it is accompanied, are owing to the inherent vitality of the fibrinous portion of the exudation, which is low in comparison with that of exudation occurring in plastic bronchitis, unconnected with tuberculous predisposition.

In both the simple and tuberculous variety of this disease the pathological condition of the bronchial membrane is gradually progressive; consequently the particular casts formed have, from time to time, a tendency to increase in length, which is manifestly the case in simple plastic bronchitis; but which, in the tuberculous variety, is seldom witnessed, owing to the friability of the exudation, which gives way under the force of continued cough, and which consequently appears in separate, broken, and distinct, portions in the sputum. Hence a superficial glance at plastic bronchitic sputum is almost sufficient to justify an opinion, as to its simple or tuberculous nature—the former being indicated by casts of some length, and frequently of a racemose appearance—the latter, by portions rarely exceeding half an inch in length, and never (at least according to my observations) with ramifications. But as the slow, progressive inflammation of plastic bronchitis is subject to the sudden supervention of more active inflammatory action, as the exudation casts thereby produced are less consistent than those already existing, and as the former are more easily dislodged than the latter, the sputum, under such circumstances, may contain but small portions of casts, and thus fallaciously indicate a tuberculous rather than a simple origin. An appeal, therefore, ought, in every instance, to be made to the microscope, with which valuable information is, at the same time, obtained by examining the other constituents of the sputum.

If the casts are the products of simple bronchitis, their associate bodies in the sputum consist of mucus-corpuscles, exudation masses and cells, and sometimes of plastic and pus-cells, together with a few blood-discs; but if they are linked with a tuberculous diathesis, there are, in addition to the above bodies, which, with the exception

of blood-discs, are generally of diminutive growth, numerous granules and epithelial scales studded with granules, which latter have been cast off by the other parts of the generally affected bronchio-pulmonary membrane. Of the pathology of such isolated granules and granular scales I shall hereafter treat, when considering the subject of pulmonary consumption. As to the presence of blood-discs, it is owing to the concurrent laceration of capillary blood-vessels during the forcible evulsion, by cough, of the exudation-casts from their attachment to the denuded basement membrane.

Sputum of the Early Stage of Simple Chronic Bronchitis.

This variety of sputum is usually of a drab colour, viscid, ropy, and of a consistence slightly less than that of the white of egg. In this fluid minute portions of yellow or yellowish-white substances are suspended in great numbers, giving to the sputum a mottled or variegated appearance.

Microscopically examined, this variety of sputum is seen to consist of great numbers of well formed and very granular mucus-corpuscles, of patches and masses of coagulated exudation, some of which do not transmit the light, whilst others are partially transparent, and exhibit numerous nuclei in the first stage of cell-development. Besides these, there are exudation-cells in all stages of growth, basement patches, plastic and pus-cells, bronchial casts, a few epithelial scales, and a granular or amorphous deposit of the sulphate and phosphate of lime.

Fig. 18.

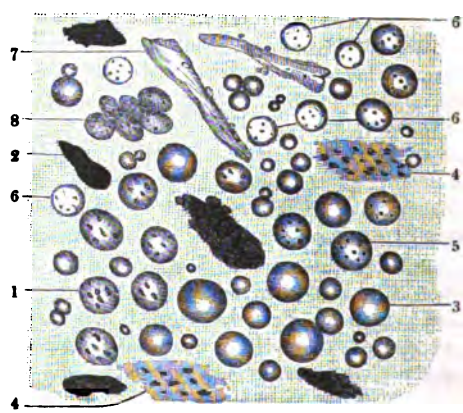


Fig. 18.—1. Mucus-corpuscles.
2. Coagulated exudation mass.
3. Exudation-cell.
4 Basement patches.
5. Plastic-cell.
6. Pus-cells.
7. Bronchial cast.
8. Epithelial patch.

The mean of twelve analyses gave the following composition :—

	In 100 parts.
Water, ...	97·36
Organic matter, ...	1·72
Chlorides of sodium and potassium, Alkaline sulphates and phosphates, Sulphate and phosphate of lime, ...	·92
	100·00

If we compare this analysis with that of the sputum of acute sthenic bronchitis and cellulitis, we find that the quantity of organic matter in a hundred parts, is to that of acute bronchitis as 1·72 to 2·15, and to that of cellulitis as 1·72 to 3·68; whilst the relative proportions of saline constituents are as 0·92 of chronic bronchitis to 1·10 of the acute, and to 1·52 of cellulitis. The quantity of solid matter, therefore, in chronic bronchitis, is less than in either of the other two diseases just quoted; whilst the relative proportion of the saline to the organic constituents is greater—a fact which, according to the explanation already given under the analysis of the sputum of acute sthenic bronchitis, we should naturally expect.

The origin of the different bodies contained in the above variety of sputum, and the pathological facts which they indicate, may be thus briefly explained :—

The very granular and well-formed mucus-cells are secreted by the bronchial epithelium in the immediate neighbourhood of the disease, the nutritive blastema supplied to which is somewhat in excess of the natural supply. The patches and masses of fibrine are portions of exudation which have coagulated on the denuded surface of the basement membrane, from which they have been almost immediately dislodged by the act of coughing. Those masses, however, which exhibit a trace of cell-growth have retained their position a longer time than those which manifest no such disposition to growth. The exudation-cells, both isolated and in mass, originate in fibrine which has retained its position on the denuded basement membrane a longer time than the before-mentioned bodies, and have consequently attained a higher development; whilst the plastic-corpuscles are the exudation-cells of full growth supplied with an excess of nutritive elements, and, therefore, originating at points of the bronchial membrane, whence exudation is freely poured out, and where inflammatory action is consequently the most active. The pus-cells are the plastic-cells in a state of adipocerous degeneration, as before shown. They are, therefore, to be regarded, not as possessing an inherent vitality as the exudation or plastic-cell, but as the latter organic structure in the process of decay; whilst the basement patches are portions of the basement structure, the molecular aggregation of which has been loosened by continued permeation of fluid exudation, and which have been detached by the

forcible contraction of the hypertrophied muscular fibres beneath them during the act of coughing.

The existence of bronchial casts is indicative of the denuded state of the basement membrane in certain bronchi, of the degree of obstruction of the latter, owing to such exudation-casts, and of the injury thereby done to the breathing-power of the pulmonary cells to which such bronchi lead.

In like manner, the presence of epithelial scales and patches demonstrates the gradual extension of inflammatory action to parts of the bronchial membrane, which were till now the seat of irritation only; whilst the occurrence of separate, isolated, amorphous sulphate and phosphate of lime points to the fluid portion of the exudation from the denuded basement membrane, as the source whence it is deposited after its removal from the circulation.

Thus, then, the physico-microscopical examination of the above kind of sputum characterises progressive inflammation of the bronchial membrane, coupled with epithelial and basement *ulceration*; whilst the quantity and the degree of development of the resulting organic structures (exudation and plastic-cells), taken in *conjunction* with the quantity of bronchial casts and of masses of simply coagulated or undeveloped exudation-plasma, indicate, to a considerable extent, that the constitutional powers are yet as strong, and that the vigour of the system has been but little impaired.

Sputum of the Latter Stage of Simple Chronic Bronchitis.

The sputum of the after stage of chronic bronchitis differs from the preceding variety, in being of a less tenacious and consistent character, as well as in being of a less variegated appearance. It rather resembles a thinly diluted and dirty-drab serum, in which gray, yellowish-gray, or grayish-white flocculi, and isolated, irregular-looking masses are suspended. These, after the sputum has remained at rest for a few hours, sink towards the bottom, *and* occupy the lowest stratum of the fluid. Under the microscope this variety of sputum exhibits the same kind of organic structures as the one last described; but they are manifestly fewer in number, and have acquired a less degree of development. The majority of the exudation-cells is limited to a diameter from $\frac{1}{5000}$th to $\frac{1}{10.000}$th of an inch; whilst those which have attained full growth, as represented by the plastic and pus-cells, vary from $\frac{1}{3000}$th to $\frac{1}{4200}$th *of* an inch. Even the mucus-corpuscles are fewer, less granular, and present a less defined, and more irregular, outline than those of the preceding variety of sputum, all of which conditions indicate a less degree of inherent vitality in the nutritive blastema, a less activity of growth, and, as a consequence of the imperfectly vitalised blastema, a correspondingly imperfect and quickly perishable cell.

The following is the composition of the above sputum according to the mean of eight analyses:—

	In 100 parts.
Water, ...	98·83
Organic matter, ...	1·02
Chlorides of sodium and potassium, Alkaline sulphates and phosphates, Sulphate and phosphate of lime, ...	·15
	100·00

On comparing the above analysis with that of the sputum expelled in the early stage of chronic bronchitis, it is at once apparent, that the amount of solid constituents is less, and that the relative ratio of salts to the organic matter of the former is considerably below that of the latter. It might, *à priori*, be expected that, inasmuch as chronic bronchitis is a progressive disease, the amount of solid constituents of the sputum would, in the latter stage, equal that of the sputum of the former stage of the affection. Reference, however, to the pathological condition of the bronchio-pulmonary membrane, during the progress of the disease, shows, that a gradual thickening of this structure takes place—that it manifests a progressive tendency to creep from the larger to the smaller bronchi, and thence to the pulmonary cells—that occlusion of many of the smaller bronchi, by inflammatory exudation, not unfrequently takes place—that this hypertrophy of the lining membrane of the bronchi and of the pulmonary cells exists at the expense of the capacity of such tubes and cells—that this reduction of capacity reduces in proportion the breathing power of such parts—that the respiratory function is further reduced by the pulmonary collapse dependent on bronchial occlusion—that, therefore, the bulk of the body is brought down in equivalent ratio to the diminished respiratory function—that the attenuation thus produced, together with the depreciation in the quantity and quality of the blood, is incompatible with severe inflammatory action; or, in other words, that neither can the products of inflammation, under such circumstances, be so copious as to quantity, nor can the vitality of the structures, arising out of them, be so great, as are the quantity and inherent vitality of the exudation plasma poured forth during inflammation, in one whose breathing capacity has not undergone so great a reduction by disease, and whose constitutional and physical powers are consequently in closer relation with the standard of perfect health. The morbid action, therefore, which exists in the bronchio-pulmonary membrane, during the latter stage of chronic bronchitis, is of a much more desultory character than that of the earlier stage, and hence the reason why patients, at this stage, will apparently be in one condition, as to the progress of the disease and the bulk of the body, for even months together. During this time the inflammatory exudation, owing to its comparative liquidity, drains as would serum from the denuded portions of the basement membrane, upon which but a small portion

of the exudation-plasma coagulates, and acquires the necessary conditions for subsequent development into cells. Hence the numerical deficiency of such structures now, as compared with similar bodies in the sputum at an earlier stage; and hence also the reason why the relative proportion of salts to the organic matter becomes less during the progress of the disease. This reduction in the proportion of salts, as well as the reduction in the quantity of organic matter, begins to take place when the pathological condition of the bronchial membrane has reached a certain maximum, when the capacity of the lungs is thereby sensibly diminished, and when, consequently, the restricted respiratory function is unable to support the present bulk of the body; and the minimum of such reduction is reached when the weight of the body has fallen to that point, at which the respiratory function can maintain it under the conditions then in force. The explanation of this occurrence, with respect to the great reduction of salts as compared with organic matter, is found in the negative of that which accounts for its proportionate excess in the sputum of acute sthenic bronchitis. I have already shown, when treating of the last-mentioned disease, that the exudation-plasma coagulates on the denuded surfaces of the basement membrane—that by this means it acquires a condition for its subsequent germination and growth—but that, to effect its development into cells, it requires, after a temporary or permanent cessation of inflammatory exudation, a supply of alkaline fluid from the blood, for the purpose of re-dissolving it, and thereby rendering it available by the germinating nuclei in the process of growth.

This necessity, on the part of the coagulated exudation-plasma, gives a large proportion of salts in comparison with the organic matter of the sputum; but, as in the latter stages of chronic bronchitis, only a small proportion of the exudation-plasma coagulates on the denuded portions of the basement membrane, this necessity for the alkaline fluid of the blood does not exist to the extent above mentioned, and, therefore, the salts are present in a correspondingly reduced quantity. If, nevertheless, we compare both the organic matter and salts with those of the healthy secretion of the bronchio-pulmonary membrane, we find that the organic matter of the sputum of the latter stage of chronic bronchitis is, to that of healthy mucus, as 1·02 to 0·68, and of salts, as 0·15 to 0·12.

It is thus evident, that a morbid action is still in progress in the bronchio-pulmonary membrane, and that, although inferior to that existing in the earlier stage of the disease, it is nevertheless in excess of that which is necessary for the production of the natural secretion.

Sputum of Senile Bronchitis.

The sputum of this variety of chronic bronchitis is of a less viscid, tenacious, and consistent character, than any which has yet been described. Its colour and consistence are similar to those of a thin

solution of gum, through which numerous small, irregular, yellowish-white or drab, flocculent, and semi-solid bodies are dispersed.

These, when the sputum is allowed to remain at rest for a short time, sink to the bottom, and occupy about $\frac{1}{14}$th of the height of the fluid. The specific gravity of a specimen now before me is 1·006, at a temperature of 65° F. In this case, which is a perfect specimen of senile bronchitis, and which has now existed (December 14, 1852) eight months in a person 77 years of age, fourteen ounces of sputum, by measure, are discharged in twenty-four hours.

On subjecting the supernatant fluid portion to the microscope, it is seen to consist of a thin menstruum, in which numerous nodules of fibrine, varying from $\frac{1}{1000}$th to $\frac{1}{18,000}$th of an inch in their longest diameter, are suspended. Besides these, there are a few isolated exudation-cells, having an average diameter of $\frac{1}{4600}$th of an inch—a few superficial basement patches—and a very scanty proportion of mucus-corpuscles, of which the form is irregularly oval, the longest diameter measuring $\frac{1}{2300}$th, and the shortest $\frac{1}{4000}$th, of an inch.

The sediment, on being submitted to microscopic examination, exhibits the same scanty numerical proportion of mucus-cells, the form and granular condition of which, however, approach more nearly those of the perfect cell. There are, in addition, a comparatively few isolated exudation-cells, the diameter of which varies from $\frac{1}{3000}$th to $\frac{1}{4200}$th of an inch. The number, however, of exudation masses is proportionately great; but they exhibit a mere trace of cell-development. With these are also associated superficial basement patches, bronchial casts, sometimes a few pus-cells and blood-globules, and thin, transparent, irregularly formed layers of coagulated exudation, of which one part of the surface is structureless and smooth, whilst the other is being developed into cells.

Fig. 19.

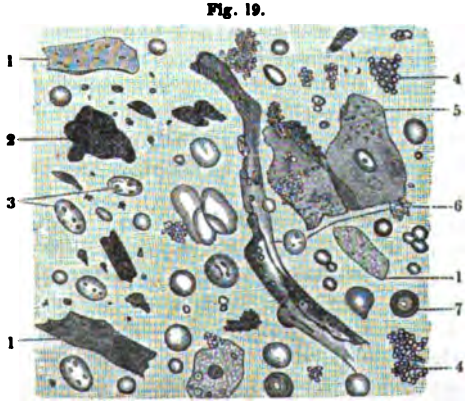


Fig. 19.—1. Basement patches.
2. Mass of coagulated exudation, showing traces of cell-development.
3. Mucus-corpuscles.
4. Exudation masses, showing diminutive cell-growth.
5. Thin layer of exudation, showing cell-development at points of its surface.
6. Bronchial cast.
7. Collapsed blood disc.

The mean of six analyses gave the following composition :—

	In 100 part.
Water, ...	99·14
Organic matter, ...	·72
Chlorides of sodium and potassium, Alkaline sulphates and phosphates, Sulphate and phosphate of lime, ...	·14
	100·00

In tracing the pathology of the above kind of sputum, it is evident, from the very small proportion of mucus-cells, that a very considerable portion of the epithelium has been shed, and that little or no attempt has been made towards its reparation. It is also evident, that from the denuded surfaces of the basement membrane a copious exudation takes place—that the amount of its organic matter is small in proportion to that of the exudation of acute, or even of the earlier stage of chronic, bronchitis—that the vitality of this exudation, as inferred from the number, size, and form of the resulting cells, and from the degree of cell-development of the exudation masses, is low in comparison with that of the exudation of most other varieties of bronchitis. Hence the coagulation of the exudation plasma but feebly takes place. The attachment between it and the denuded surfaces of the basement membrane, on which it is poured out, is correspondingly loose, and therefore liable to be easily dissevered. The result of this condition, and of the low degree of vitality in the exudation plasma, is, the severance of the coagulated portions of the latter from the basement membrane, shortly after the germination of their nuclei, which are manifestly incapable of developing fully-formed cells. Other portions of this exudation are detached, directly after coagulation, in opaque particles, and small, dark, irregularly-formed masses, which exhibit no trace of cell-development; whilst other particles, again, retain their position on the basement membrane sufficiently long, and manifest a sufficient degree of vitality, to acquire the development of diminutive exudation and plastic-cells, as which, and as pus-cells, resulting from the degeneration of the latter, they exist as separate and isolated structures in the sputum.

The presence of bronchial casts indicates that, in certain portions of the membrane, the inflammation is more active than in others, and that the exudation there poured forth is, in quantity and in its power of coagulating, sufficient to obstruct the corresponding tubes. These casts, however, give the same evidence of their deficient vitality as do the other portions of the exudation; inasmuch as they exhibit but very few cell-structures, all of which are considerably inferior, in point of development, to similar bodies observed in the casts of acute sthenic, or of the early stage of chronic, bronchitis.

Now, if we compare the analysis of the above kind of sputum with that of healthy mucus, we find that the proportion of organic matter

of the former to that of the latter, is as 0·72 to 0·68, and of salts, as 0·14 to 0·12. The difference, therefore, is very trifling, and might, on chemical analysis only, be readily ascribed to the varying amount of functional energy manifested by the bronchio-pulmonary membrane; but with a morphological examination of the respective sputa, such an explanation does not hold good. Thus the quantity of organic matter, in healthy mucus, is due to the presence of mucus-corpuscles, as is clearly shown by microscopical examination; whilst the same means of investigation at once demonstrates that such structures form but a very small proportion of the organic matter of the sputum of senile bronchitis; but that it is chiefly due to the existence of simply coagulated exudation-plasma, and to the different organic structures arising therefrom. Hence, then, the fact, that although, in senile bronchitis, the organic constituents of the sputum are but a trifle in excess of those of healthy mucus, yet they have a different origin, which is due to a less degree of the same kind of morbid action that characterises acute inflammation in the bronchio-pulmonary membrane.

The indications of treatment to be drawn from the foregoing examinations and pathological considerations are:—

1. To restore the vital tonicity of the bronchial capillaries, to thereby reduce them to their normal diameter, and thus to abolish the pathological cause of inflammatory exudation.
2. To preserve the patency of the bronchial tubes and pulmonary cells, by promoting the expectoration of inflammatory exudation and other sputa from the surface of the affected membrane.
3. To promote the healing of epithelial and basement ulceration, to procure the expulsion of bronchial casts, to reduce hypertrophied tissue, and thus to restore the normal anatomico-physiological condition of the lungs.

Now, the first of these indications is fulfilled by the direct application of a moderately cool air to the affected membrane, and by counter-irritation to the surface of the chest.

The former means is employed by the constant inhalation of air of the temperature of 60° F., by which the partially exhausted vital tonicity of the bronchial capillaries is gradually restored to a healthier degree of action, and thus the natural diameter of the vessels is regained, and the necessary pathological condition for the occurrence of inflammatory exudation thereby abolished. The second means is adopted by the use of blisters, of which a succession is generally required; or by the application of other counter-irritants to the chest, as the tartar-emetic ointment, the croton oil, or terebinthinate liniment.

The selection of any of these must be determined by the pathological condition of the membrane, as to the extent and degree of its morbid action, and also by the degree of constitutional power which

exists. If a considerable portion of the bronchial membrane is affected—if the morbid action would readily pass into the acute character—and if the constitutional powers are as yet strong, a succession of blisters should be applied; because the amount of inflammation thereby produced on the surface of the chest is greater, in proportion to the surface attacked, than that produced by the other means of counter-irritation just mentioned; whilst the contents of a full-sized blister, or the aggregate of a succession of blisters, will indirectly act as a salutary drain on the yet engorged capillaries, and thus favour their return to their normal diameter under the excitation of their contractile power, by the conjoint action of cool air, and by the reflex motor impulse exerted on their fibre through the inflammation existing on the surface. Hence it will be inferred, that blisters are, as a general rule, the most applicable in the earlier stages of chronic bronchitis, in which the necessary conditions for their use more particularly exist. When the disease is less active, and when the respiratory function has been so far reduced that a manifest loss of weight has taken place, the frequent drain from a succession of blisters would add to the already existing debility, and, on this account, preference should be given to the other forms of counter-irritants.

In senile bronchitis, in which both the drain from the bronchio-pulmonary membrane and the debility of system are great, it is important to economise the strength in every possible way; therefore it were better to excite, on the surface of the chest, a vivid erythema, by means of any rubefacient, than to promote any discharge therefrom by either the tartar-emetic ointment or the croton-oil liniment. It is consequently implied, that these two last-mentioned remedies are more particularly adapted to the latter stages of simple chronic bronchitis, in which the degree of inflammatory action in the bronchio-pulmonary membrane is of a more indolent and desultory character than that of an earlier stage—when, in consequence, the inflammation which they produce on the surface of the body is greater than that of the membrane itself, which is, therefore, controlled; and when the small quantity of purulent discharge thereby formed does not depress the vital energy of the system. To maintain this last-mentioned condition, and through it to improve the vital tonicity of the bronchial capillaries, light vegetable tonics, and a plain, nourishing diet are necessary in every shade of chronic bronchitis; whilst, in order to relieve the congested state of the affected membrane, to reduce as much as possible the labour of the lungs, and yet to enable them to discharge the amount of function of which they are capable, in the most advantageous manner, it is important to maintain a brisk circulation through the skin, and to breathe as pure an air as it is possible to obtain. Hence, then, the necessity for frequent tepid sponging of the body, followed by friction, and coupled with warm clothing, to which the use of a spacious and well-ventilated apartment, and, under favourable conditions of the

weather, exposure to the open air must be added. Exposure, however, must not lower the temperature of the skin, neither must it introduce the patient to an atmosphere, the temperature of which is considerably lower than that out of which he has just come. To meet the vicissitudes of our variable climate, exposure will involve the necessity for additional clothing, as well as the use of a respirator of the lowest power. The higher powers of this instrument, owing to the great warmth which they impart to the air passing inwards, and to the rarefaction thus occasioned, do not permit the inspired air sufficiently to excite the vital tonicity of the affected pulmonary capillaries; neither do they allow that aëration of the blood to occur, which a cooler and denser air, admitted by a lower power, necessarily produces.

The second indication of treatment, namely, to preserve the patency of the bronchial tubes and pulmonary cells, by promoting the expectoration of inflammatory exudation and other sputa, is fulfilled by the exhibition of the alkalies, by mild mercurials, by a nourishing diet, and, where muscular debility is the cause of imperfect expectoration, by expectorants strictly so called, and sometimes by a combination of the latter with stimulants. A sea voyage is highly conducive to recovery; because the marine air is impregnated with the saline particles of the ocean, which particles, during each inspiration, come more or less into contact with the products of inflammation resting upon the surface of the bronchial membrane, exert a liquefacient effect upon them, and thereby favour their expulsion from the affected tubes. If the amount of discharge from the affected membrane is great, and is evidently outrunning the nutrition of the body, as is frequently the case in senile bronchitis, the use of astringents is indicated; and of these the mineral acids, in combination with light vegetable tonics, are decidedly the best as internal remedies, and a dry, cool air, as a direct application to the membrane. Sometimes, however, it may be necessary to apply a greater stimulus than cool air to the affected capillaries, with a view of exciting the vital tonicity of their walls; and, in this case, a slight impregnation of the air of the patient's apartment with the vapour of tar, will be an effectual and admissible method of medicated inhalation. But stimulating and detergent inhalations are, as a general rule, to be utterly condemned, and are never to be employed, except in the latter stages of ordinary chronic bronchitis, and in that form which accompanies advanced age, in both of which the body is emaciated, and the affected membrane but little prone to acute inflammation. Under these conditions, too, advantage will be derived from the exhibition of iron, of which the most efficient preparation is the tincture of the sesquichloride, or the sulphate, dissolved in acidulated infusion of roses, either of which answers the double purpose of giving iron to the blood, by which the due proportion of its red globules is maintained, and of exerting an astringent influence on the relaxed bronchial capillaries, by which the quan-

tity of discharge from the affected membrane is gradually controlled.

It is also important, in such cases, to impart a temporary energy to the spinal and ganglionic systems of nerves, in order that the different functions over which they preside, may receive an impulse that will contribute to the well-being of the patient. This may be done by electro-galvanism, applied daily to the spine. I have not unfrequently witnessed a few applications of this agent followed by a greater muscular effort during inspiration, an increase in the force of the cough, improved appetite and digestion, together with an increased peripheral circulation, and augmentation of the temperature of the skin, all of which effects demonstrate the beneficial influence that must thereby be produced on the existing pathological condition of the bronchio-pulmonary membrane.

In fulfilling the third indication of treatment, the healing of epithelial and basement ulceration must be sought in the proper restoration of the vital tonicity of the affected capillaries by the means before stated, and by as great a degree of quietude of the affected parts as it is possible for the patient to observe. The reduction of the capillaries to their normal diameter, by the restoration of their vital tonicity, is followed by the immediate cessation of morbid, and by the re-establishment of healthy, action, which at once commences a healing process in the ulcerated portions of the membrane. Should, however, this process be retarded by frequent cough, as is frequently the case in the earlier stages of the disease, it will be necessary to allay the inordinate sensibility of the membrane, which is a necessary condition of its occurrence, by sedatives taken internally, or exhibited in the form of medicated inhalation. To the latter, there is here no objection whatever. On the contrary, it is rather to be preferred to the exhibition of sedatives by the mouth; because the application is direct, and, therefore, a less quantity of the medicinal agent is required; besides which, the energy of the cerebro-spinal system is less depressed, and, consequently, the healthy action of the different organs, in their functional manifestation, is less impaired than is the case under the administration of sedatives by the mouth. Should, however, inhalation fail to produce the desired effect, the different preparations of hyoscyamus, conium, papaver, or the compound tincture of camphor in a mucilaginous fluid, may be administered as often as the absolute necessity of the case may require.

To procure the expulsion of bronchial casts, the alkalies must be perseveringly employed, because they have the power, as I have already shown, of effecting their gradual disintegration, and, by this means, of facilitating their removal by cough. In the earlier stages of chronic bronchitis, and in the earlier stages only, their use is very advantageously coupled with that of an occasional emetic of sulphate of zinc. An emetic of this kind, exhibited after the use of the alkalies has been continued for four or five consecutive days, will effect the expulsion of many casts, which are readily detected by

the microscope in the sputa expelled. But as the effort of vomiting is productive of a temporary engorgement of the vessels of the bronchial membrane, and as it thereby tends to depress the vital tonicity of their walls, it is advisable, during the action of the emetic and for several hours afterwards, to maintain the due warmth of the general surface, and to breathe an air of a temperature 2° or 3° lower than that in which the patient was before the exhibition of the emetic. By these means the temporary loss produced by the emetic in the vital tonicity of the capillary walls is quickly regained, and its good effects only, as observed in the improved breathing, remain. Where these casts are dependent on the plastic form of bronchitis, the above remedies are still to constitute the major part of the treatment; but in that form of plastic bronchitis which is unconnected with any particular diathesis, it may be necessary to excite the mercurial inflammation, in order that the exudation to which this specific kind of morbid action gives rise, may be more copious than the former, may break it up, thereby favouring its expulsion, and thus promoting a return to the natural condition of the parts on the subsidence of the mercurial action. When, however, the casts of plastic bronchitis are associated with a particular diathesis, the object of the treatment must be, to subvert such diathesis, by improving the condition of the blood, and to subdue the local disease, as well as to effect the expulsion of casts, by the general measures already stated, exception being taken to any but the very moderate use of mercury. The alkaline preparations—the best adapted for this purpose—are, the iodide and bromide of potassium, chlorate of potash, liquor potassæ, and the citrate and acetate of ammonia. These last-mentioned remedies are likewise the most important agents in the treatment of hypertrophy of the bronchio-pulmonary membrane, in which their exhibition may be combined with that of the mercury with chalk, provided there is no contra-indication to the use of the latter, and with the moderate employment of purgatives when such remedies are admissible. But, as the conjoint action of these remedies is the gradual disintegration of abnormal deposit, its re-absorption, and its subsequent elimination from the system; and as the sea air, by impregnation with the particles of the above-mentioned salts, is imbued with all the properties of the latter, it were better, in many cases, to treat hypertrophied tissue by a residence on the coast, where every inspiration would be productive of a certain insensible yet beneficial result, than to weary the patient by the incessant administration of drugs.

(*To be continued.*)

ARTICLE III.—*Notes from Practice.* By GEORGE W. BALFOUR, M.D., Cramond.

On Scarlatina.

Of its Prophylaxis.—The sole vestige of Hahnemannism,[1] which has ever insinuated itself into regular practice, is the question of the prophylactic powers of belladonna in scarlatina; and it becomes those who encourage its lingering amongst us to consider whereto it leads, for its basis, if it have any, is the so-called homœopathic law, and evidence of its curative powers is as easily obtained, and of the same character with that on which its prophylactic powers are founded.

In June 1851, an epidemic of scarlatina broke out in this neighbourhood. The first case was that of a boy who had been sent a message to an adjoining parish, where no case of the disease at that time existed. On his return he was seized with symptoms of British cholera, and the rash appeared the next day, the disease proving subsequently mild. The second case occurred in a house nearly a mile distant from the first one, on the day after the appearance of the rash on him. There had been no communication whatever between the houses, nor between either of them and any other case, so far as could be traced. Other cases, some of the most severe description, subsequently occurred, but not always in the persons of those who had been most exposed to contagion. At first I was disposed to refer this to the prophylactic virtues of the belladonna solution, which, with the view of testing its supposed powers, had been supplied to the families of those attacked; but on reversing the conditions, by withholding the belladonna, I found the same thing still occurring, viz., that the disease passed by those in daily communication with the sick, and seized others who maintained as complete an isolation as possible,—that at times the escape was final, for that epidemic at least; at others, at a subsequent and uncertain period, the disease returned, and seized the brothers, sisters, or other relatives, or co-habitants of those who had been sick, and this entirely without reference to whether they had taken or were taking the belladonna solution; some of the most severe cases having occurred in those who were taking it when seized, while others, who escaped entirely, though exposed to contagion, never got any.

Thoroughly convinced of its utter inadequacy to prevent the disease, I have never since prescribed it; yet recently I have had many single cases, occurring in families who had not previously had the disease, and in whom ordinary precautions, and indeed, often very

[1] Amongst the higher classes this so-called prophylactic goes under the name of "*Sir James Clark's* specific." I lately had one of those treasured memoranda, so headed, handed to me for the instruction of my rustic ignorance.

imperfectly carried out, sufficed to prevent the spread of the disease beyond the person of the individual first attacked. While thus denying all positive advantages to the belladonna, I believe it to have positive disadvantages; as a continuance of its use, even in the ordinary small doses, is sure to be followed by disordered digestion, foul tongue, etc.—not the most likely condition of body to ward off any epidemic—and from personal experience of an earlier date, I know that no dose short of a poisonous one, even when continued for some time, has any other effect.

Of its Treatment.—Scarlatina is a disease caused by the circulation in the blood of a depressing narcotic poison, which is thrown off by the skin, and, more or less, by the mucous membranes—the extent to which the latter are affected varying in different epidemics and in individual cases. The accompanying fever, or delirium, or stupor, are not inflammatory in character but are the effects of the circulating poison on the central organs of sensation and circulation, and are best combated by favouring its elimination. It tends to a death more or less sudden, by the poison being more or less in excess of the powers of the system. As death then occurs from a want of power in the system to throw off the poison at all, or completely, a stimulating treatment is obviously indicated. Accordingly, we find that in many cases we are forced to commence by the administration of alcoholic stimuli; but as reaction proceeds a milder stimulant is indicated, and one, too, which, while supporting the vital powers, should determine to the skin and mucous membranes, and such an one we have in carbonate of ammonia, long ago vaunted by Dr Peart as a specific in this disease. The wildest delirium is no contra-indicant of its use. On the contrary, the intervals between the doses may be advantageously lessened, as it acts in such cases as a calmative. Of course the skin must be determined to by means of the application of moist heat. The mode of doing so is of little importance; but a tub large enough to contain an adult not being a usual appendage of the houses of the poor, the modified vapour-bath is for them indispensible. Cold affusion, or the wet sheet, while an effectual mode of producing the same effect is not always a safe one. Warm poultices to the throat, and assiduous sponging or gargling with plain hot water, are the best means of combating the local affection,—the pain and swelling there depend on the situation and nature of the tissues, and are no more to be combated by stimulants and cauterants[1] than is the redness of the skin. As from the same cause this local congestion is apt to become chronic, mild astringents, as acidulated gargles, may be subsequently employed; slight ulcerations are best treated by attention to the primæ viæ, which must always be unloaded of their depraved secretions; more severe

[1] I may add that since I discontinued the application of cauterants to the throat, swelling of the glands has been more rare than formerly. I am aware this *fact* is no *argument*, but it leaves the door open for future observations.

and foul ones are only aggravated by harsher *remedies*. Tonics, particularly iron, are generally required during convalescence, along with nutritious diet, warm clothing, and a careful prevention of exposure till desquamation is completed.

Of the Urine during its course.—Dr J. W. Begbie has recently called the attention of the profession to the state of the urine in scarlatina, declaring the presence of albumen in it to be "as essential a symptom of the disease as is desquamation of the cuticle," and propounding that, so soon as the stage of temporary albuminaria was passed, the danger of dropsy was over, although desquamation might not be complete. Conceiving that this, if confirmed, would be a great advantage, thus permitting the earlier relaxation of the confinement, always irksome and often inconvenient, I took four cases as they occurred, the first a boy of twelve, the second a youth of about twenty, the other two adults, a male and a female about thirty. I tested the whole amount of urine passed during each twenty-four hours, during the whole course of the disease from its commencement to the completion of the desquamative process, which took place in each case, yet I never found albumen. The treatment —that mentioned above—may have prevented its appearance. If so, it but proved its suitability; at all events, its absence shows that albuminaria is not an "essential symptom" of the disease, neither is it a constant concomitant of desquamation; hence, when we do find it we must be doubly careful in our after-treatment.

On Erysipelas.

Of its treatment with iron.—In consequence of having witnessed the cure of a case of erysipelas treated with iron, under the joint care of Dr Charles Bell and myself, and also in consequence of Dr Bell's assurance of its great and unfailing success, I was induced to give this method a trial, and this the more readily that opinions were very much divided regarding the proper treatment of this disease, and I myself had no great confidence in any. Since that time I have treated all my cases, upwards of twenty, with iron, and have had no cause to regret my doing so. On the contrary, erysipelas is one of the few diseases for which I now believe we have a certain and unfailing remedy, and this whether it be infantile or adult, idiopathic or traumatic.

The first case so treated was a highly scrofulous woman, with erysipelas of the scalp, arising from irritation of two large sores on it. She was cured in three days. The second was a man with erysipelas of the foot and ankle, cured in two days. The third was a case of traumatic erysipelas of the scalp in a woman; she was ill for a day or two before being seen, but was cured in about five days' treatment. The wound, a deep cut about three inches long, was healed within a week. In short, all the cases, many of them very severe, and accompanied with high delirium, some of them phlegma-

onous, others vesicular, and several occurring in children, were cured in less than a week on the average. Suppuration took place in none but two, in both of whom the treatment was not commenced till after effusion had taken place. Their convalescence was, of course, more tedious. The ninth day was that on which convalescence commenced even in the most severe cases, and *probably* the course of the disease would have been shorter even in them had the medicine been given more regularly; for so sure is it in its effects, that I can, with I may almost say absolute certainty, predict the state of the patient on ascertaining the quantity of the drug taken, and a glance at the bottle is fully as informing as to the state of the disease as a look at the affected parts themselves.

The tincture of the muriate is that preparation of iron I have hitherto employed, the dose varying with the age of the patient; the great object being to saturate the system with iron as speedily as possible, and to keep it so till the disease is abated. A few doses suffice to remove the pain, and lessen the heart's action; it acts also as a diuretic, and to some extent corrects the secretions, often cleaning the tongue as well as any purgative. It never produced headache nor other unpleasant symptom, and was continued with advantage throughout the highest delirium. The only other remedy employed was an occasional purgative, and the local application sometimes of a warm poultice, and at others of simple flour or starch powder and cotton wadding, the poultice being preferred, perhaps without much reason, when the situation or extent of the part affected did not throw difficulties in the way of its application.

Its principal curative action seems, as Mr G. Hamilton Bell supposes, to be exerted on the capillary vessels; and for this reason, that while its diuretic action is equally well maintained by smaller doses, less frequently repeated, its curative action is not obtained unless the system be saturated with it, and kept so for some time. Further, without denying that it may act as a renal purgative in disease, I may add that, from some experiments on myself, it does not seem to do so in health. The quantity of the urine was much increased by fully a half, yet the quantity of urea was increased relatively and absolutely only to so small an amount, a grain or two in twenty-four hours, as to be far within the usual limits of aberration, and much too insignificant, supposing it to be real, to account for the wonderful curative agency of the drug. On taking twenty minims of the tincture every two hours, the second and third doses produced some slight degree of tension in the head, a symptom which subsequently ceased, and did not return. At this period, too, the pulse was slightly accelerated, possibly from nervousness, it afterwards came down from 80, my usual average, to 64. The iron was taken at intervals for four days, regularly every two hours for the last twelve hours. Its diuretic action was always established within an hour after taking each dose, no iron could be detected in the urine, nor in the serum of the blood; but as this may depend on my want of

refinement in chemistry, I am willing to give any chemist who may be anxious another opportunity by repeating the experiment; for I know no method of treating any disease more worthy of investigation, and none more deserving of adoption by the profession than the treatment of erysipelas with iron. Some have supposed its efficacy to be owing to its *stimulant* action, that this is not the case was well shown in a case I had recently under my care,—a young man labouring under scarlatina, who had a patch of erysipelas vesicular over the sacrum, and one over the right armpit. Neither of these were at all influenced by the carbonate of ammonia, employed in the treatment of the co-existing scarlatina, but yielded at once to twenty-four hours' treatment with the muriate of iron. I append a few cases as samples:—

CASE I.—J. A., a male, aged 20, was seized six days ago with pain, redness and swelling about the right angle of the jaw. He has done nothing but dust it with flour. To-day, Dec. 24th, 1851, the swelling and redness extend over right cheek and right side of the scalp to the vertex, the right eye being completely closed; tongue foul; bowels constipated; pulse 108. ℞ Calomel, gr. x.; Pulv. Jalap, ʒj. Statim. Sum. ℞ Træ Ferri Mur. ʒviij.; Syrup Zinzib, ʒi.; Aq. Font ad. ʒjv. The parts have been dusted with flour, which is to be continued. Sigr. A teaspoonful every two hours.

Dec. 25th.—Pulse 80; bowels have been well moved; the tongue is now clean; parts affected neither so red, so swollen, nor so painful; the right eye is now open, but the left eye is closing, and the left side of the head and face becoming affected; to continue tincture regularly.

Dec. 26th.—Pulse 96; much the same; to continue tincture.

Dec. 27th.—Pulse 96, but softer; all the symptoms amended; to have dose of castor oil.

Dec. 28th.—Pulse 76; convalescent, and continued well.

Remarks.—The first effect of the treatment was so beneficial, that he was tempted to be less assiduous in his attentions to his bottle. I saw him on the 25th, just as the consequences of his negligence were becoming apparent. A more steady attention to his medicine very soon put him out of danger, and restored him to health.

CASE II.—Mrs W. R., aged 23, first seized four days ago. To-day, November 27th, 1851, her face and forehead are swollen red and painful; the eyes are both closed; the redness is extending over the scalp; the bowels are constipated; the *tongue loaded with a creamy fur;* pulse 120. Nothing has been done but dusting the face with barleymeal, which is to be continued. ℞ Calomel, gr. x. h.s.s. ℞ Pot. Sulph. ʒji.; c.m.s. ℞ Træ Ferri Mur. gr. xx. To be given every two hours in a little water.

Nov. 28th.—Pain and swelling in face better; a little redness and

pain extending into the neck; pulse 96; *tongue clean; bowels not moved for a week*, as powders were neglected to be taken; to take powders, and continue tincture.

Nov. 29th.—Powders taken; bowels freely moved; pain and redness gone; skin desquamating; one eye open, other nearly so; pulse 96.

Dec. 1st.—Convalescent; swelling not quite away; to have a dose of salts.

Dec. 3d.—Well.

This case shows well its action in cleaning the tongue, independent of any purgative.

CASE III.—J. Y.'s child, aged 4 months. Slight redness of the left labium pudendi was observed in this child two days ago. To-day, Dec. 20th, 1851, the redness and swelling affect both, and extend over the pelvis; to have a dose of oil, and two drops of the tincture of the muriate of iron in a teaspoonful of water every two hours.

Dec. 21st.—Bowels have been freely moved; redness now confined to right labium, which is much swollen, but it is also extending over the right hip and thigh; the parts to be kept carefully dusted with dry flour and covered with wadding, and the drops continued.

Dec. 22d.—Much the same.

Dec. 23d.—Redness paler; skin of labium less tense.

Dec. 24th.—Convalescent.

CASE IV.—Mrs B., aged 25, was seized on the evening of Feb. 6th, 1852, with violent headache, and bleeding from the left side of the throat, which is swollen red, and painful; the tongue is foul; bowels constipated; to have ten grains of calomel immediately, and a large dose of oil next morning; the throat to be steamed with hot water, and a poultice applied externally.

Feb. 7th.—Throat better; as the bowels have not been very freely moved, her medicine is to be repeated.

Feb. 8th.—Bowels have been well cleared out; the tongue is cleaning; the throat better; but the left cheek and ear are swollen red and painful; to have twenty drops of the tincture of the muriate of iron every two hours.

Feb. 9th.—A large bulla the size of the palm of the hand, has formed on the left cheek; the ear is very much swollen and painful, also the left side of the scalp, which is full of smaller bullæ; pulse 140; tongue foul; the hair to be cut out; the parts which have been hitherto fomented to be dried and dusted with flour, as the dampness is uncomfortable; ten grains of calomel to be given immediately, and followed in four hours by two drachms of sulphate of potash; to continue iron.

Feb. 10th.—Pulse 120; redness somewhat paler, otherwise much the same; bowels well moved; motions very offensive.

Feb. 11*th.*—Much the same.

Feb. 12*th.*—Ditto; whole face very much swollen and disfigured; the skin cracked; lips bleeding; bowels have been thrice moved; the motions not so offensive; to continue the iron.

Feb. 13*th.*—Has been very delirious all night, but is to-day looking and feeling better; pulse 120.

Feb. 14*th.*—Passed a better night; is to-day much easier; pulse 112.

She continued now steadily to improve, the pulse falling day by day, till on the 18th it was 76, and she was speedily quite well. Notwithstanding the great amount of swelling over the head, and the general severity of the disease, not a drop of matter formed.

CASE V.—A child four months old, on the eighth day of vaccination, had a slight blush below the elbow, on the same arm. A dose of oil was ordered, and the parts to be well dusted with flour. On the ninth day, the whole arm was swollen to double its natural size, and intensely red. Three drops of the tincture of the muriate of iron were ordered to be given every two hours. Next day the swelling and redness were almost entirely gone, and even the natural areola around the vesicles much paler than in general. The iron was ordered to be gradually diminished. The child was speedily well.

On the Treatment of certain Cases of Rheumatism with Aconite.

While the employment of aconite in rheumatic fever, where it is desirable speedily to affect the system, entails an amount of care and watching, from the powerful nature of the remedy, which renders it unsuitable for country practice, there are certain cases of sub-acute rheumatism for which it is most suitable. These are chiefly synovial in character, with a more or less marked febrile state of the pulse. Where that is awanting, as in more chronic cases, aconite is rarely useful. In less urgent cases of affection of only one joint, and in many cases of sciatica, regulation of the bowels, and the local employment of the aconite, are often sufficient.

As aconite, without being cumulative in its action—*i. e.*, it is never prolonged or developed after the immediate action of the last dose has ceased, yet has this peculiarity, that one dose paves the way for another, rendering its action more powerful often than was to be anticipated, the dose requires to be very cautiously increased, and very often gradually diminished. I have in one case gradually increased the dose of Dr Fleming's tincture to ten minims thrice a-day, and as gradually diminished it to one; still keeping up the same amount of action. The patient was much benefited, but not cured. Sometimes it produces symptoms of gastric disturbance, which, when they threaten, are best prevented by giving the aconite after a meal.

Diaphoresis was believed by Störk to be one of its principal actions, yet Dr Fleming never observed it, and I have only seen it once; but in that case it was most copious, the whole bedding being saturated after each dose. The patient was not benefited by his perspirations, and on account of them the aconite was stopped. To obtain the curative action of the aconite, its physiological one requires to be fully developed—the coincidence of the two is generally well marked. It is best given in plain water; and for external use, its tincture may be mixed in various proportions with plain soap liniment. The presence of opium, so often combined with it, is unfavourable to the development either of its physiological or curative action. I add two cases.

CASE I.—J. F., a male, aged 45. Sixteen years ago had an attack of acute rheumatism, affecting the left side of the chest and right shoulder, accompanied by palpitations, which have not since troubled him. The heart's action and sounds are normal. Sixteen weeks ago, had a return of his complaint, which he says has now fallen into his left knee, weakened probably by a previous luxation. For some weeks he has been almost unable to walk. The joint is not much swollen, but tense and painful, always half-bent. His pulse is 66. He was ordered to foment the joint with hot salt and water several times a-day, and to take a minim and a-half of Tr. Aconit. Nap. (Fleming's), three times a-day.

Jan. 2d.—Two days since last report is a little easier, and can bear more weight on the limb; dose increased to three minims thrice a-day; pulse 60.

Jan. 4th.—Dose increased to five minims thrice a-day.

Jan. 7th.—Afternoon.—Has taken no aconite since morning; pulse 64; can now extend the limb nearly straight, and move it more easily; little pain in the joint, unless it get a twist; continue five minims thrice a-day.

Jan. 10th.—Pulse 60; can stretch out leg fully, though still pained in so doing; slight gastric symptoms were present yesterday, but are now gone; physiological action developed; affecting the back of the throat and left haunch; six minims of the tincture to be given thrice a-day.

Jan. 14th.—Has been sick all day; vomiting everything; pulse 64, but raised by the action of some spirits he has taken to relieve the sickness; on attempting to rise, his head swims, and objects disappear, accompanied by a most intense prickling sensation all over him; pain in joint gone, motion as free as in the unaffected one, but it is still weak; aconite stopped, but hot douche to be continued. There was no return of the rheumatic pain, and the joint speedily got strong.

CASE II.—A. G., a male, aged 68. Three years ago, he had a

smart attack of acute rheumatism, chiefly affecting the wrists and shoulders. He has had occasional flying pains of rheumatic character, and has always had a constant pain in the right wrist, which incapacitates him for work; the wrist is slightly swollen and tense: the head of the ulna apparently thickened—as he says, shot out. When this took place, the pain was somewhat relieved, but it is still great and constant. The artery of this wrist beats fuller but not faster than that of the opposite one; pulse 60, full and strong. He has been long under the doctor's hands without relief, and was salivated about five weeks ago. Since then, he has been taking occasional hot baths, from which he has obtained trifling benefit; bowels opened by medicine last night; the wrist to be bathed with hot salt and water, and two minims of the tincture of aconite to be taken thrice a-day. Next day—

Dec. 20*th.*—Much the same; three minims to be taken thrice a-day.

Dec. 21*st.*—Took a hot bath last night, and slept better; pulse 60, but not so full; four minims thrice a-day.

Dec. 23*d.*—Is now freer of pain, and sleeps better than he has done for months; swelling of wrist slightly diminished, neither so tense nor hot; pulse 56.

Dec. 24*th.*—Still improving; five minims thrice a-day.

Dec. 26*th.*—Still improving; swelling diminishing; sleeps well, and is free from pain; pulse 56, soft and compressible, but there is no physiological action developed; six minims thrice a-day.

Dec. 28*th.*—Still improving; pulse 54; about a quarter of an hour after taking each dose, rawness of the throat is felt, accompanied by a prickling sensation, then passing down the affected arm and wrist, and speedily extending over the whole body; it lasts about three hours.

Jan. 7*th.*—Has been steadily improving since last report till yesterday, when he says his hand became worse; the wrist to-day is much swollen; no return of the pain, however, nor of the flying stitches; the physiological action of the aconite is still maintained; the pulse 60, soft and compressible; the swelling was therefore obviously produced by some cause acting locally; he was ordered to bathe the wrist well, and be more careful in future.

Jan. 16*th.*—Dismissed cured of the rheumatism, though the joint is still weak.

Remarks.—The action of the aconite on the pulse was in this case much more satisfactory than can be expressed either in words or figures. Shortly after commencing the use of the aconite, the flying pains, and those in the joint, began to leave him; and on its physiological action being developed, left him entirely. On the swelling of the joint diminishing, a sub-luxation of the ulna was found to exist where the hand was pronated. On rotation it slipped in, and when the hand was supinated, a mere thickening was felt over its situation. This is not uncommon in similar cases. I found that

previous to the 7th January, he had succeeded in getting a certificate of inability to work from one surgeon, which had been refused to be countersigned by another better known to fame, on the ground, after examination, that he was perfectly able for work; and, in revenge, he had gone and worked most manfully, with the view of injuring a joint he knew to be weak, and thus excite compassion—fortunately he failed.

ARTICLE IV.—*An Account of the Epidemic Yellow Fever on board H.M.S. "Highflyer" in* 1852. By J. WATSON, M.D., Surgeon, R.N., Royal Naval Hospital, Jamaica.

H.M. steam ship "Highflyer," reputed to be a very fine vessel of her class, and remarkably well ventilated, arrived at Port Royal, Jamaica, from Europe, on the 20th October 1852, where she remained until the 28th, the shipping and town of Port Royal being at the time, and long previously, quite free from fever.

On the day last named, the steamer proceeded to sea, and anchored at the Havanna on the 3d November, where yellow fever existed, but not extensively, among the shipping and in the town. She remained there until the 19th November, when she sailed for St Thomas, which she reached on the 30th. At this time epidemic yellow fever raged with the utmost malignancy at St Thomas, and it had been communicated, in a fatal form, to several of the R.M.S.P. Company's vessels.

The merchant vessels were lashed alongside the "Highflyer" to coal her, positive orders being given that no communication should be held between the "Highflyer's" people and those of the merchant ships. It appears this order was not strictly observed, as medical aid was requested by the master of one of them—the "Art Union"—for his crew, who were all "down" with fever, and it was rendered by the medical officers of the "Highflyer." This, however, appears to have been the extent of the direct communication between the parties.

The "Highflyer" having completed her coaling, sailed at one P.M. of the 2d December from St Thomas, where she had remained exactly two days and seven hours.

While at the Havanna the officers frequented the town daily, as they also did at St Thomas; but the crew was not permitted to land at either place, unless when employed on duty in boats between the ship and the shore.

Proceeding from St Thomas, the "Highflyer" anchored at Trinidad on the 6th of December, where no fever existed at the time, and remained there until one P.M. of the 17th December. While at Trinidad she was joined by the flag-ship "Cumberland," from which she received two assistant-surgeons and a captain. Both of the for-

mer were speedily attacked with fever after they joined, and one of them died with black vomit. At one P.M. of the 17th December, the "Highflyer" proceeded to sea; the day following touched at Grenada, where she remained only nine hours, and then prosecuted her voyage to this port, where she arrived on the 23d December.

From the time of the "Highflyer's" arrival within the limits of the West India station, to that of her departure from St Thomas, no case of fever of any kind had been entered on the sick list; but on the 4th December, two days after her departure from St Thomas, and fifteen after leaving the Havanna, a stoker was attacked with fever; on the 5th, six of the crew took fever, one of whom died with black vomit. From this time up to that of the "Highflyer's" arrival at Port Royal, on the 23d December, the disease continued its ravages until the number attacked of men and officers amounted to forty-three. When she arrived at Port Royal, the sick then on board were sent to hospital. Eight fresh cases occurred in the harbour up to the 27th December, and were also sent to hospital, when the disease entirely ceased on board.

Altogether fifty-one men and officers were attacked—viz., thirty-eight men and boys, of whom three died—eight per cent. Thirteen officers, of whom four died—thirty per cent. The captain, an assistant-surgeon, a clerk, and the gunner, died.

It will thus be seen that the mortality was much greater among the officers relatively to their numbers, than it was among the crew of the ship. It is also to be observed, that the total mortality and the number of attacks are much more limited than they have usually been, in ships of the "Highflyer's" complement, which have become subject to yellow fever on this station, after the disease assumed the epidemic form.

The above statement comprehends all the material facts which I have been able to collect respecting the disease on board the "Highflyer."

It cannot be reasonably doubted that the "Highflyer's" people received the germs of the malignant yellow fever, either at the Havanna or St Thomas, and in all probability at the latter. It is also equally clear, that the disease became localised in the ship, which afterwards became pestiferous to persons joining her. The death of the assistant-surgeon, with the characteristic symptoms of yellow fever, sufficiently proves this.

The great and most important practical question arises, Is it safe to permit the sick persons from such a vessel as the "Highflyer" to be landed among a healthy community? This question was put to me, and, fortified in the opinion at which I had arrived by the results of many similar experiments in this place, I recommended that there should be no interruption to the "Highflyer's" people communicating with the shore, but intimated that it would be dangerous for strangers to be exposed to the atmosphere of the ship, so long as the disease continued in her. There has been no interrup-

tion to the communication of the "Highflyer's" officers and men with the shore. Her sick have been in proximity with a considerable number of surgical cases from other ships, in the hospital, and with a large proportion of the crew of the "Persian," affected with a malarious fever. The surgeon, and the two assistant-surgeons, of the hospital are in hourly attendance. The former has been frequently exposed to the influence of epidemic yellow fever, but never had the disease, whereas the two latter never saw a case of the disease previously. The result of this experiment of exposing so many persons, who may be fairly presumed to be susceptible to the contagion of yellow fever to direct communication with the disease, has been, so far as it goes, highly adverse to the imposition of quarantine in such cases; for no single instance of any kind of fever followed the landing of the "Highflyer's" sick, or the free intercourse of the officers, stewards, and people with the town, either in the hospital or in the town.

It has excited considerable surprise here to learn that the R.M.S.P. Company's ship "Plata" was subjected to quarantine on her arrival in England, because she had yellow fever on board, after quarantine measures have been denounced as inefficient by the Central Board of Health. Reflecting on the rapid and constant intercourse which now takes place between England and the countries where yellow fever is endemic, bearing in mind, too, the atrocity of confining people in a pest-ship, in places where no proper houses are prepared for their reception, and the manifold annoyances which must accompany necessary or unnecessary quarantine, I have made the above representation, which proves, that in the instance of the "Highflyer" no harm resulted from landing persons who were in the worst stage of the malady. Such examples will encourage the advocates of the more humane system of dealing with such epidemics, as furnishing evidence of the uselessness of quarantine.

Without dogmatically asserting that yellow fever is never, under any circumstances, contagious, I have witnessed and watched the progress of so many epidemics in this place, as to feel perfectly satisfied that any contagious powers the disease may possess in crowded ships, is speedily rendered inoperative by moderately good ventilation; and that the best mode of checking the progress of such diseases, is to remove the sick to roomy quarters on shore as speedily as possible.

NAVAL HOSPITAL, JAMAICA,
10th January 1853.

27*th January* 1853.—It has been ascertained here, since the above was written, that while the "Highflyer" remained at Trinidad, she was visited for a few hours by the marine officer of the "Calypso," and by the surgeon of that ship, and that both these officers were in a few days attacked with malignant fever, the former having died in

his cabin on board with black vomit; and the latter, after remaining in the "Calypso," ill with the disease about two days, was landed, and died on shore.

These facts confirm the position asserted above, that the "Highflyer" became pestiferous, and proves the extreme danger to be apprehended if strangers are exposed to the morbific influence which prevails on board such vessels. But even under the most favourable conditions for the extension of the disease by contagion, it is remarkable, that though the two officers mentioned caught the disease on board the "Highflyer," and both were confined in the "Calypso," in communication with their messmates and shipmates for several days, and until one of them died with black vomit, no other individual on board the "Calypso" took the disease. I am also informed by Dr Chamberlain, the old experienced health officer, that patients affected with yellow fever were landed in lodging-houses in Kingston from the company's ships, and that the disease did not extend to a single individual there.

These facts, in addition to those embodied in the present report, clearly prove to my mind, that, however dangerous it may be to visit places or ships affected with epidemic yellow fever, there is no danger whatever in abolishing quarantine, if the sick be removed to well-ventilated quarters on shore. To enable the disease to prevail, extensively in any community, it appears that the unknown and mysterious SOMETHING, which we call epidemic influence, must be present. I am indebted to Mr M. Swinney, surgeon, R.N., for the facts respecting the progress of the disease in the "Highflyer," who was in medical charge of the ship then as assistant-surgeon, and who was afterwards deservedly promoted for his zealous conduct at that time.

J. W.

ARTICLE V.—*On the Danger of Gutta Percha Bougies.* By JAMES SYME, Esq., Professor of Clinical Surgery.

IN the month of August last, Dr Macfarlane, of Glasgow, recommended to my care a gentleman who had been so unfortunate as to break a bougie in his bladder. It appeared that the patient had suffered from a stricture, and been accustomed to palliate the symptoms by introducing instruments. In going to his shooting quarters in the Highlands, he employed for this purpose one of some gutta percha bougies, which had remained there since the preceding season, and did not remark anything particular until he attempted to withdraw it, when only a small portion came away. With some difficulty a further portion was extracted from the urethra; but as Dr Macfarlane found, by introducing a sound, that there was a piece in the bladder, he sent the patient to me.

The presence of the stricture forbidding any hope of extracting

the foreign body by means of instruments introduced through the urethra, I performed the usual operation of lithotomy, with the exception of not cutting through the base of the prostate, which was of course unnecessary for the removal of so small a body. The portion of bougie was between one and two inches in length, slightly encrusted by phosphatic deposition. In a few days the patient was quite well, and has had no trouble since, except from having a full sized bougie passed every second month to prevent contraction of the stricture, which I took the opportunity of dilating while he was under my care.

Gutta percha bougies and catheters—while perhaps never altogether trustworthy—are liable, from exposure to damp, to become so friable, that they cannot resist the slightest force, and therefore should be employed with extreme caution, if not entirely discarded from use, as in my opinion they ought to be.

ARTICLE VI.—*On a Cause of Deafness not hitherto described.* By JAMES SYME, Esq., Professor of Clinical Surgery.

DISEASES of the ear may be divided into the *curable* and the *incurable*. The former are remediable in accordance with the ordinary principles of surgery. The latter belong to the "Aurists." The two following cases pertain to the surgical department, and present examples of a morbid derangement in the auditory organ, which, so far as I know, has not been noticed by any other writer upon this subject, either general or special.

CASE I.—W. T., aged 17, was admitted into the Royal Infirmary on the 27th of September 1848, on account of severe pain and deafness of his left ear. On looking into it, I saw at the bottom of the tube a small white body apparently composed of bone, but so extremely sensitive of pressure that a satisfactory examination could not be accomplished until the patient was placed under the influence of chloroform. It was then ascertained that the growth possessed an osseous consistence, and as it did not so fully occupy the cavity as to prevent me from introducing the point of the small polypus forceps, I, by this means, easily effected its extraction. It was similar in size and form to the half of a small split-pea, and had adhered to the side of the cavity by a narrow neck. The patient sustained no inconvenience, and immediately regained his hearing, while the pain which had been so severe for the preceding three months as to render him unable for following his employment, completely ceased.

CASE II.—R. H., aged 35, was admitted into the Royal Infirmary on the 5th of April 1853, on account of deafness and uncom-

fortable sensations connected with his right ear, by which, he said, that for twenty years the ticking of his watch could not be heard. On looking into the tube, I saw an appearance so perfectly similar to that which has just been described, that I at once recognised the same condition. As the excrescence was larger than in the former case it did not allow space for introducing the point of the forceps, but allowed a probe to be passed round it, so as to let me ascertain the position of its connecting pedicle. By means of small straight scissors I divided this and then lifted out the piece of bone, which presented the form here represented of the exact size, and also magnified.

The patient immediately regained his power of hearing.

Part Second.

REVIEWS.

An Essay on the Action of Medicines in the System, or "on the Mode in which Therapeutic Agents introduced into the Stomach produce their peculiar Effects on the Animal Economy," being the Prize Essay to which the Medical Society of London awarded the Fothergillian Gold Medal for 1852. By FREDERICK WILLIAM HEADLAND, B.A., M.R.C.S., etc. 8vo. London: Churchill. 1852. —(*Continued from page* 345.)

MR HEADLAND'S last section of restorative hæmatics call for no remark. They are solvents; divided by him into anti-lithics—the solvents of uric acid and its salts, and anti-phosphates—the solvents of the phosphates.

He thus concludes his six orders of restoratives, and now introduces us to his second great division of hæmatics :—

"CATALYTICS.

"These constitute the second division of hæmatic medicines. The above name is applied to them on the assumption, that their operation in the blood results in the destruction or counteraction of certain morbid agencies. (καταλύω.)

"The differences in action between this and the preceding division of blood-medicines, have been stated at length in the remarks on hæmatics in general. On recurring to Prop. VII., it will be seen that it is there stated,—

"'That others (medicines of the first class), called catalytics, act so as to counteract a morbid material or process, and must pass out of the body.'

"The action of the remedies in this division, which are the surest and most potent of all those that are employed in the treatment of disease, is involved in a greater degree of doubt and obscurity than that of any other class. Though there may be in some cases a certain dim explanation which we may catch hold of, and strive to fix or to render definite, yet in many instances there is not even so much as this. How can we, for example, by any of the common terms which are made use of to designate the actions of mercury, of arsenic, and of iodine, express the peculiarity of their influence over syphilis, lepra, and scrofula, respectively? Does it not seem better to confess our ignorance, and to say that all we know for certain is, that these remedies have the power of antagonizing certain diseases?

"Having done this, we may afterwards try, if we can, to invent for this action a theoretical explanation. Our arguments and theories will not affect the fact, nor will they lead us into danger, if we have first laid down the truth, however incomprehensible, as the only secure foundation, in such a manner that it shall be incapable of being disturbed by the flimsy superstructure. Some philosophers have erred in this; that they have first, with much pains and labour, erected an airy fabric on a basis of incontestable truth,—but subsequently, relying too much on that which they have themselves raised, they have commenced to pull up the blocks on which it was founded, and have thus brought the whole speculation tumbling to the ground in ruins.

"Considering, then, that the action of catalytic medicines in each disorder is of a special and a peculiar kind, which I think will not be denied, I have grouped them in orders which are named according to the several morbid states in which they are employed.

CATALYTICA.

Ord. 1. Antiphlogistica.
Ord. 2. Antisyphilitica.
Ord. 3. Antiscrofulosa.
Ord. 4. Antiarthritica.
Ord. 5. Antiscorbutica.
Ord. 6. Antiperiodica.
Ord. 7. Anticonvulsiva.
Ord. 8. Antisquamosa.

"The medicines of the first order are employed in inflammatory disorders generally, and possess an influence over the process of sthenic inflammation, however first produced. The second order contains those that are used in the several forms of syphilis. The third, those that are employed in scrofula. The fourth, those that are found to be useful in the cure of painful affections of the joints, as gout and rheumatism, and also of oxaluria, lithiasis, diabetes, and other disorders of secondary assimilation. The fifth order contains those that are useful in scurvy. The sixth, those that are employed in ague, remittents, and other periodic disorders on the catalytic principle. The seventh,

those used in convulsive disorders. Lastly, the eighth, those that are capable of curing lepra, psoriasis vulgaris, and some other skin diseases.

"The action of these medicines being much more obscure than that of restoratives, I shall therefore have less to say about them individually, and shall not go so much into particulars. That part of Prop. VII. which refers to catalytics, may be divided in the following minor propositions:—

"*m. p.* 1.—That they act in the blood, and that their effect is permanent.

"*m. p.* 2.—That each of itself tends to work out a peculiar operation in the blood.

"*m. p.* 3.—That the diseases in which they are used depend on certain morbid materials or actions in the blood.

"*m. p.* 4.—That the result of the action of a catalytic medicine is in some way to neutralise or counteract some one or more of these morbid processes.

"*m. p.* 5.—That these medicines are all unnatural to the blood, and must at length pass out of the system."—Pp. 145–148.

We have already devoted so much space to the review of Mr Headland's work that we must refrain from further comments on details. We do not, therefore, intend to offer any remarks on all his minor propositions, but we must take leave to dissent from part of that one, so important both pathologically and therapeutically, which affirms that the diseases in which these medicines are employed are active blood diseases. We are not going at present either to deny or affirm the title of syphilis, scrofula, gout, scurvy, marsh fever, and skin diseases, to be considered as blood disorders, and Mr Headland acknowledges the difficulty which we experience in accounting for convulsive diseases upon hæmatic principles; but we must protest against being held as assenting to the doctrine, that inflammation is essentially a blood disease. We would therefore make a single remark upon the subject of antiphlogistics, which constitute Mr Headland's first section of catalytics:—

"ORD. I.—ANTIPHLOGISTICS.

"The inflammatory process—the chief seat of which, however it may first originate, is in the circulating blood—exerts a powerful influence over the nervous system, and may, by this influence, cause death. When fever is produced to any extent, there must be danger.

"We are enabled in two ways to control or to mitigate this state of things. We may produce an action on the nervous system, or we may direct our attention to the process in the blood.

"Of neurotic medicines, which we shall have to consider hereafter, there are some which may be made use of at the commencement of the attack, and others which are appropriate when the process is more advanced. Of the first kind is antimony, considered in its neurotic action. Employing it in large doses, we may produce a powerful effect upon the nerves which supply the heart, and thus, by diminishing the impulse upon the inflamed parts, or on the vascular system generally, promote absorption and resolution. Blood-letting produces the same action, but in an inverse mode. Antimony diminishes the pressure on the vessels by weakening the force of the heart; blood-letting weakens the force of the heart by diminishing the pressure on the vessels."—Pp. 173, 174.

We give our most hearty concurrence to the view which our author here takes of the action of antimony in inflammations, and when he has got so sound and suitable an explanation to offer, we

think that he may very well dispense with any catalytic effect to complete his doctrine; but his pathology involves him in the necessity for this, for if it be true that the chief seat of the inflammatory process is in the blood, a mere lowering of the heart's action, either by antimony or blood-letting, will not explain its cure. But we utterly deny the truth of this pathological doctrine; he exactly reverses the cause and effect. The state of the blood does not cause the inflammatory process, but the inflammatory process causes the state of the blood; and we do not cure the inflammation by destroying something in the blood, but we cause what is morbid in the blood to disappear by removing the inflammatory process,—*sublata causa tollitur effectus.* If it be true that inflammation is an active blood disorder, because "the blood then contains an excess of fibrine, causing it, on coagulation, to exhibit the buffy coat," and because "there are also found in this, according to Mulder, some hyperoxides of proteine," then is a fœtus an active blood disorder, because we meet with the same condition of the blood in a pregnant woman, and we might reasonably expect to be able to appease the ghost of Malthus, and check redundant population in a quiet way, by the catalytic action of an antimonial solution, or a box of blue ointment.

Mr Headland has, we think, given, in the passage which we have just quoted, the true, and only true, solution of the action of antimony as an antiphlogistic. We do not blame him for not being equally successful with his other great antiphlogistic, mercury. We shall throw no stones at him, seeing that we, *quoad hoc*, live in a glass house ourselves, and have no very satisfactory explanation of its valuable effects to offer. We take, however, a piece of advice, which Mr Headland gives us in a passage which we formerly quoted, and, instead of employing any of the common terms which are used to designate the action of this invaluable medicine, we honestly confess our ignorance. In return for his piece of good advice, we give him another. Let him not continue to separate, as he does, between the physiological and therapeutical actions of remedies, but let him bend his intelligent mind to the study of the physiological action of mercury, and, when he has learned something precise on this point, we will venture to say, that he will throw his hæmatic pathology of inflammation, and his catalytic theory of the antiphlogistic action of mercury, to the winds.

We regret that space does not permit us to go over his various orders of catalytics in detail; we would wish our readers, however, to know what are the agents discussed in this interesting chapter, and we therefore present them with Mr Headland's catalogue:—

"*Ord* 1. *Antiphlogistics.*
Antimonials; Mercurials; Alkalies; Salines.
"*Ord.* 2. *Antisyphilitics.*
Mercurials; Preparations of Gold; Iodide of Potassium; Sarsaparilla (?)
"*Ord.* 3. *Antiscrofulics.*
Iodine; Bromine; Chlorine; Iodides and Bromides; Potash.

"*Ord.* 4. *Antiarthritics.*
Colchicum; Nitro-hydrochloric Acid; Lemon-juice.
"*Ord.* 5. *Antiscorbutics.*
Citric Acid; Lemon-juice; Fresh Vegetables; Salts of Potash (?
"*Ord.* 6. *Antiperiodics.*
Arsenious Acid; Arsenite of Potash; Alum; Chloride of Sodium.
"*Ord.* 7. *Anticonvulsives.*
Preparations of Arsenic, Silver, Zinc, Lead, and Copper.
"*Ord.* 8. *Antisquamics.*
Preparations of Arsenic; Sulphur; Pitch."—P. 153.

Taking leave of the hæmatics, we come now to the other great class of remedies, neurotics, which are thus introduced to us in Proposition VIII.:—

"PROP. VIII.—*That a second class of medicines, called* NEUROTICS, *act by passing from the blood to the nerves or nerve-centres, which they influence.*
"1. *That of these some, called* STIMULANTS, *act so as to exalt nervous force, in general or in particular.*
"2. *That others, called* NARCOTICS, *act so as first to exalt nervous force, and then to depress it; and have also a special influence on the intellectual part of the brain.*
"3. *That others again, called* SEDATIVES, *act so as to depress nervous force, in general or in particular.*"—P. 205.

In this, and indeed in the greater, part of the remainder of the work, we do not differ from Mr Headland in any great fundamental points of doctrine, and therefore, though far from adopting all his opinions, we do not think it incumbent on us now to show cause against them. But we wish to furnish to our readers a brief abstract of the contents of the remainder of the volume, and Mr Headland's succinct and most convenient plan of stating his views in the form of propositions, enables us to do this almost entirely in his own words. The following are his minor propositions regarding neurotics generally:—

"*m. p.* 1.—That neurotics are medicines which pass into the blood.
"*m. p.* 2.—That their action is evidenced by a change in one or more of those functions which are attributed to the nervous system.
"*m. p.* 3.—That it is necessary that they should pass from the blood to that part of the nervous system which is influenced by them.
"*m. p.* 4.—That they are of use in an over-excited or depressed state of the nervous system.
"*m. p.* 5.—That they are transitory in action, and cannot remain in the blood."—P. 210.

He divides stimulants into general and special, according as "they exalt nervous force in general or in particular. That is, they may extend their operation more or less to the whole nervous system, having a general tendency to communicate nervous energy, or they may confine their action to particular departments of this system, having no manifest influence on other parts." The following is his catalogue of general stimulants:—

"ORD. I.—GENERAL STIMULANTS.
"*Mineral substances.*—Ammonia and its Carbonates. Phosphorus.

"*Animal substances.*—Musk and Castor.

"*Vegetables containing volatile oils.*—The aromatic Labiatæ, Compositæ, and Umbelliferæ. Cloves and Nutmeg. Cinnamon, Cassia, and Sassafras. Rue, Barosma. The Aurantiaceæ. Canella. Valerian. Mustard. Cajuput and Pimenta. Hops. Juniper, Turpentine. Cardamom. Onion.

"*Vegetables containing acrid principles.*—Senega. Horse-radish. Serpentary. Cascarilla. Pepper. Contrajerva. Ginger. Capsicum. Mezereon.

"*Resinous substances.*—Guaiacum. Mastich, Olibanum, Myrrh, Elemi. Copaiba. Peru, Tolu. Assafœtida, Ammoniacum, Galbanum. Benzoin, Storax. Pine resin.

"This list of general stimulants is long, but it might even be further extended. The majority are produced by the vegetable kingdom. The acrid principles to which some owe their power are intermediate in nature between volatile oils and resins. Some of them are volatile, like the former; others fixed, like the latter. Some of the resinous products contain also a volatile oil. The principles of capsicum and mezereon are very similar in their chemical nature to the true resins. (*Vide* page 73.)"—P. 215.

We cannot compliment Mr Headland upon the scientific character of these divisions. Why should mustard, horse-radish, and assafœtida, each be placed in a separate class, whilst one and all of them owe their activity to a volatile oil? We find also some substances among his general stimulants which we suspect have no business to be in such company. We doubt if hop has any stimulant action at all. In virtue of its bitter matter, it is a stomachic tonic, and in virtue (of its volatile oil?) a hypnotic; and cascarilla, though it certainly does contain a volatile oil in small quantity, does not owe any of its medicinal qualities to it, but rather to its bitter principle, the precise nature of which is not yet determined, and is more a tonic than anything else.

We must enter our dissent, also, against the following physiological doctrine:—

"Thus ammonia is used in syncope, a state which no doubt depends upon a shock received by the brain. And there are several things which show that it relieves this state more by stimulating the brain than by merely rousing the heart and vascular system. It is found to be most effectual when inhaled through the nose, by which means it could easily pass at once into the cerebral circulation."—P. 216.

Mr Headland has fallen into error here by making too extensive an application of his minor proposition, that stimulants enter the blood and pass to that part of the nervous system which is influenced by them. This we agree with him in considering to be true of their internal administration, but not of their external application. Ammonia held to the nose acts in syncope, we believe, on the heart and respiration, by a reflex propagation of its pungent impression on the nasal branches of the fifth pair, and may be placed, both physiologically and therapeutically, in the same category with aspersion of cold water on the face and the application of sinapisms to the calves of the legs, in neither of which cases, we apprehend, does any absorption take place. We must, moreover, say, that we suspect that the number of persons who have "no

doubt that syncope depends upon a shock received by the brain" is not very great.

The special stimulants include—strychnia, brucia, toxicodendron, ergot of rye, borax, rue. With respect to the three last, he says,

"Ergot of rye is a stimulant to the muscular nerves of the uterus of the female, but to no other nerves in any marked degree. Borax and rue possess a similar action, but are not so efficient.

* * * * * *

"When given in an over-dose, ergot has a dangerous action on the brain, producing at some times narcotism, at other times syncope. But this is not the effect for which it is employed, and is altogether distinct from its operation as a special stimulant, which is exerted only upon the ganglionic nerves of the muscular uterus. In small doses it produces no other effect than this."—Pp. 221, 222.

We are not in a condition to explain, any more than Mr Headland, how ergot acts on the uterus; his statement just quoted is no explanation at all. We can hardly hope for satisfaction on this point till we know something more precise than we now do as to how the uterus acts with or without ergot. In the meantime we may express our doubts if borax and rue have at all the action which is ascribed to them. The latter most graveolent commodity has, we suspect, little other effect than helping to expel wind, and will not contribute much to the expulsion of a fœtus. It ought, we think, to be found in the list of general stimulants, alongside of valerian, or rather in the rear of it, for it is vastly its inferior.

The next division of Neurotics, Narcotics, is divided into the three heads of Inebriants, Soporifics, and Deliriants.

Many persons who are in the habit, as most of us are, of talking of alcohol as a stimulant, or rather as *the* stimulant par excellence, may be disposed to think that Mr Headland has fallen into grave error in placing it among narcotics. We would have them, however, suspend their judgment on this point; for we must own that we have always experienced some little difficulty in assigning to alcohol and opium their proper relative places in a classification of remedies. Perhaps our dubieties on this point may in part have arisen from an early impression made upon us by the speech of a Hibernian fellow-student, who, when the theory of the action of alcohol was under discussion in a debating society, delivered the following sententious decision on the question,—"Mr President, I believe that alcohol is a stimulant up to the three-and-twentieth tumbler!" Our doubts, however, have another and more perplexing source than the remark of our Milesian friend, who doubtless argued on directly inductive principles from an experimental acquaintance with the potheen, but who did not profess to give any very accurate account of the effect of the twenty-fourth noggin. The facts of the case are yet in dispute among authors. Mr Headland says, when discussing the action of narcotics generally:—

"Narcotics exert in the first place a stimulant, and in the second place a sedative action; but these actions have no relation in degree, for one of them always

exceeds the other, as we have just seen. Thus in the whole effect, either the stimulation must exceed the depression, or the depression must be greater than the stimulation."—P. 224.

Now to this statement we are inclined to demur. Certainly in the case of an undoubted narcotic, such as opium, the depression is quite disproportionate to the previous excitation, but we doubt if this is true of alcohol. On the contrary, we hold that the depression following the action of alcohol is directly proportionate to the forerunning excitement; and hence we are inclined to doubt the absolute correctness of the arrangement followed by Mr Headland, and are possessed in favour of calling alcohol a stimulant, and separating it from narcotics altogether. Our distinction between a stimulant, our author's "inebriant," and a narcotic, his "soporific," is, that the former produces excitement, followed by depression in a corresponding ratio; the latter produces excitement, but the depression is proportionally much greater. In estimating the truth or falseness of this, we must not take as our guide the mere effect of the drug upon the intellectual functions, but the *tout ensemble* of its action upon the nervous system, as manifested by its effect on other functions than those of the brain,—the circulation, for instance. Many a man, we believe, may get very drunk, and yet never manifest any mental elevation at all; but his pale face and clammy skin, chilly hands and feeble pulse, which he may have when deposited in bed with his boots on, will just be proportionate to the flushing and warmth and force of circulation which his excited nervous system produced when he was "speaking thick" at the dinner table; whilst another fellow of more generous temperament may become very talkative and insist upon singing, and yet walk to bed after a fashion, without ever becoming in the least degree comatose, because his nervous system, though excitable in the intellectual department, was not very sensitive to the action of the alcohol otherwise. We will never ascertain correctly, then, whether it is true or false that alcohol produces a depression proportionate to the antecedent excitation, if we look merely to the intellectual functions.

But whether we admit or deny the absolute correctness of Mr Headland's arrangement, we must acknowledge that it is convenient, and we think that by his division of narcotics into the three orders of inebriants, soporifics, and deliriants, we are able in a very satisfactory manner to classify such drugs as alcohol and chloroform, opium and lactucarium, belladonna and hyoscyamus. We must remark, however, that Mr Headland uses a very mild phrase indeed when he calls tobacco and lobelia exceptional members of the order inebriants. Tobacco used medicinally, as in the form of the vinum tabaci, or the enema, is as little of an inebriant as may be, and must not, we think, be viewed in the same light with the weed when inhaled in smoke from a cigar, or when it titillates the schneiderian membrane in the shape of rappee; and that man must be born to be the high priest of Bacchus who could extract any inebriation from lobelia. To-

bacco smoke, we are inclined to believe, comes nearer to ammonia than alcohol, both in its stimulant and poisonous action, and we presume it is the use of the nicotiana in this form, to which Mr Headland has more particularly adverted, when he was induced to place it among his inebriants. Is it not possible that in reality tobacco smoke is quite a different thing from tobacco in substance; that a volatile alkaloid, perhaps resembling picoline in its nature, is inhaled; and that the original base of the tobacco, the nicotine, so far as it escapes decomposition by the heat, is deposited in the form of the oil, which renders the wet end of a cigar, or the liquor of a pipe, repulsive even to the most practised smoker? And ought not, therefore, the smoke, if it be permitted to enter a classification of remedies at all, be placed among the stimulants resembling ammonia, and the drug, as used medicinally, be accounted a real sedative!

Mr Headland's last division of neurotics, sedatives, which he very properly defines to be medicines which directly depress nervous force, are divided also into general and special.

The general sedatives enumerated by him are hydrocyanic acid, creosote, aconite, conium, colchicum, and, to our great astonishment, tea and coffee. We were at a loss to know upon what right these last articles presumed to occupy such a position, and we cannot say that we think Mr Headland has established a good title for them by the following very round-about explanation of their action:—

"Tea and coffee, common articles of diet, are slightly sedative to the nervous system generally. They lower the pulse, and, by diminishing congestion of the brain, tend to clear and tranquillise the action of the mind. This activity of thought is endangered by the cerebral congestion which exists early in the morning, on account of recent sleep,—and again towards the evening, from the full meal of the middle of the day. It is thus at these times that they are found so useful, and are so universally adopted. They are, in fact, almost indispensable to the daily existence of civilised man, with whom a continual energy of mind is a necessary of life."—P. 236.

Very different is the account of the matter given by Lehmann (Op. supra cit., p. 138), in describing the effect of caffeine:—"A quantity from two to ten grains will produce the most violent excitement of the vascular and nervous systems,—palpitation of the heart, extraordinary frequency, irregularity, and often intermission of the pulse, oppression of the chest, pains in the head, confusion of the senses, singing in the ears, scintillations before the eyes, sleeplessness, erections, and delirium." This is not like a sedative. Even without Lehmann's mention of delirium, which we have known to be produced by green tea taken in the usual way, we should, if we followed Mr Headland's classification, place them among his inebriants, upon the strength of their not tranquillising, but most decidedly exciting, the mental faculties, and producing in poets, philosophers, and mature spinsters, respectively, brilliant fancies, startling theories, and astounding scandal.

The special sedatives are antimonials, ipecacuanha, digitalis.

"These are medicines which, like the last, depress nervous force, yet they do not exert their influence on the nervous system as a whole, but only on certain parts of it. So far they resemble special stimulants; but their effect is of a directly opposite nature. They have also no direct influence on any part of the brain.

"Antimony, ipecacuanha, and digitalis, have each of them a number of different actions. The first has already been included among catalytic hæmatics, as tending to counteract in the blood the process of inflammation. But it possesses further a neurotic power, by which it is gifted indirectly with a more powerful control over acute inflammations than could be exerted by any slow-acting blood-medicine."—P. 237.

Mr Headland conceives that they act solely as sedatives to the branches of the vagus nerve. We have no doubt that they act on the vagus, but are by no means satisfied that they do not at the same time affect the sympathetic also. Certainly the following reasons for this exclusion are not satisfactory to us:—

"They cannot act upon the sympathetic, for two reasons. If they did so, we might reasonably expect that the other parts of this nerve would be influenced at the same time. But this does not appear to be the case. And again, an action upon the sympathetic nerve could not be suddenly and violently evidenced, for the natural action of this nerve is slow, chronic and persistent. At all events, the effects for which they are used could not be produced by an affection of this nerve."—P. 238.

Mr Headland's ninth proposition is:—

"PROP. IX.—*That a third class of medicines, called* ASTRINGENTS, *act by passing from the blood to muscular fibre, which they excite to contraction.*"—P. 244.

And his minor propositions are:—

"*m. p.* 1.—That they are medicines which pass into the blood.
"*m. p.* 2.—That they have the power of causing the contraction of muscular fibre, living or dead.
"*m. p.* 3.—That their operation is to diminish secretion, to repress hæmorrhage, and to give tone to the muscular system.
"*m. p.* 4.—That these results are to be accounted for by their action on muscular fibre, to which they pass from the blood."—P. 245.

We have nothing to say against any of these dogmata, but we must express our doubt as to the correctness of the opinion which Mr Headland holds, in connection with so many other writers, that their action on the muscular fibre is chemical. At least he seems to us to express it too boldly when he says:—

"In the same way as the mechanical irritation of a small artery will act upon the unstriped muscular fibre contained in its coat, and thus cause the vessel to contract at one point; so it seems that an astringent, by a chemical action on the same muscular tissue, is able to cause the capillary vessels seen under the microscope to contract and diminish in size."—P. 247.

Had he contented himself with the less positive opinion which he expresses a few lines higher up, that their dynamical property of causing the contraction of vessels is connected somehow with their chemical properties, we should have admitted this as probable, but

it is too broad a statement to say, that the unstriped muscular fibre undergoes a chemical action.

Mr Headland divides the astringents into mineral and vegetable. We find no notice taken of cold, the best of all astringents, and which acts on parts distant from that to which it is locally applied. Perhaps he did not think that this fell under the domain of his book, which refers to medicines, but he might have brought it in under the form of ice, which, by the way, ought to have found a place among his sedatives. There is nothing like it for checking vomiting.

Mr Headland's tenth proposition, and the minor propositions relative thereto, are as follows:—

"PROP. X.—*That a fourth class of medicines, called* ELIMINATIVES, *act by passing out of the blood through the glands, which they excite to the performance of their functions.*"—P. 256.

"*m. p.* 1.—That eliminatives are medicines which pass into the blood.

"*m. p.* 2.—That they cannot remain there, but must pass out of the body.

"*m. p.* 3.—That in so doing, they tend to pass out by some glands more than by others.

"*m. p.* 4.—That the result of their passage through a gland is to increase its secretion.

"*m. p.* 5.—That they are of use when the state of the system requires that the function of a gland should be restored or promoted."—Pp. 260, 261.

He arranges them under the heads of sialagogues, expectorants, cathartics, cholagogues, diaphoretics, and diuretics.

We regret to be obliged, by the length to which we have extended our remarks, to avoid the discussion of many interesting points connected with these, and we must pass by altogether his fourth chapter on the action of some of the more important medicines in particular.

We have in the course of this extended review set before our readers the principal contents of Mr Headland's interesting work, and they will now be better able to understand his classification of remedies, which, though given by him at the beginning, we prefer now stating at the end:—

"A CLASSIFICATION OF MEDICINES WHICH ACT AFTER ENTERING INTO THE BLOOD, ACCORDING TO THEIR SUPPOSED MODES OF OPERATION.

CLASS I. HÆMATICA.
- DIV. I. RESTAURANTIA.
  - Ordo 1. Alimenta.
  - Ordo 2. Acida.
  - Ordo 3. Alkalia.
  - Ordo 4. Tonica.
  - Ordo 5. Chalybeata.
  - Ordo 6. Solventia.
- DIV. II. CATALYTICA.
  - Ordo 1. Antiphlogistica.
  - Ordo 2. Antisyphilitica.
  - Ordo 3. Antiscrofulosa.
  - Ordo 4. Antiarthritica.
  - Ordo 5. Antiscorbutica.
  - Ordo 6. Antiperiodica.

DIV. II. NARCOTICA.
- Ordo 1. Inebriantia.
- Ordo 2. Somnifera.
- Ordo 3. Deliriantia.

DIV. III. SEDANTIA.
- Ordo 1. Sedantia Generalia.
- Ordo 2. Sedantia Specifica.

CLASS III. ASTRINGENTIA.
- Ordo 1. Astringentia Mineralia.
- Ordo 2. Astringentia Vegetabilia.

CLASS IV. ELIMINANTIA.
- Ordo 1. Sialagoga
- Ordo 2. Expectorantia.

Ordo 7. Anticonvulsiva.
Ordo 8. Antisquamosa.

Class II. NEUROTICA.
Div. I. Stimulantia.
Ordo 1. Stimulantia Generalia.
Ordo 2. Stimulantia Specifica.
Ordo 3. Cathartica.
Ordo 4. Cholagoga.
Ordo 5. Diaphoretica.
Ordo 6. Diuretica."
—Pp. ix., x.

We have been so much occupied in expressing dissent from opinions of Mr Headland, that we may be supposed to have practically retracted the general commendation which we bestowed upon the book and its author; but we would correct any such impression by repeating most emphatically, that no amount of discordance between our notions and his doctrines, can make us insensible to the merits of an author, who devotes his time and talents to the discussion of so difficult a subject as the theory of the action of remedies, or make us forget to thank him for the pleasure we have derived from the perusal and reviewing of his work. We have dealt chiefly with those matters in which we take leave to differ from him, because our object has been to excite and keep up a desire on the part of our professional brethren to discuss and investigate this important department of medical science. We hope that we have, in expressing our own ideas, not neglected the example which Mr Headland has set before us of avoiding in the statement of our opinions, a spirit of dogmatism.

We are sure that many who may read these remarks will not be able to assent to them, but all we can beg of them is, that if they think that they have climbed the tree higher, and got a firmer footing than we can boast of, they will not look coldly upon us from their eminence, but kindly throw us down some of the fruit which they have gathered.

A. D. M.

Traité de Chimie Anatomique et Physiologique. Par MM. Robin et Verdeil: accompagné d'un Atlas de 45 Planches. Paris, 1853.

Atlas der Physiologischen Chemie, zugleich als Supplement zu C. G. Lehmann's Lehrbuch: 15 Tafeln, nach dem Microscop gezeichnet und erläutert, von Dr Otto Funke. Leipzig. 1853.

Die Verdauungssaefte und der Stoffwechsel. Eine Physiologisch-chemische Untersuchung, von Dr F. Bidder und Dr C. Schmidt. Mitau und Leipzig. 1852.

De Adipis Concoctione et Absorptione. Diss. inaug. auct. A. Lenz. Dorpati. 1850.

[Note.—We beg of the reader to correct the following rather important errata, which, among many others less considerable, have crept into our last

notice of MM. R. and V.'s work:—In page 246, lines 6 and 7, for "venous" *read* "arterial," and *vice versa*. In third line from foot of page 247, for "kidney" *read* "spleen." In page 248, line 12, for "Pumic" *read* "Pneumic."]

To complete our examination of the doctrines of MM. Robin and Verdeil as to the general nature of what they call the disassimilative process, it seems to us sufficient to present the reader with the conclusions of our authors, with respect to the formation and disappearance in the economy of the fats and saccharine compounds. We think that, in the history of these two groups of immediate principles, the discussion of most of the questions on which we have not already touched may be included. But before entering on this task, let us revert for a moment to the general considerations on which we dwelt in our last notice of MM. Robin and Verdeil's work, and especially to the physiological signification assigned to the fats and sugars by the supporters of the usually received doctrine of combustion. We shall find that here, where observation and experiment have been most successful, their results have displayed the least correspondence with the doctrine in question.

The sugars and fats are placed by Liebig among the respiratory elements of the food, the relation of all of which to the living organism, we are taught to believe, is extremely simple. They are formed and stored up by plants in order that animals may burn them. The end of their existence, in short, is to serve for fuel. If this be true, the analogy which has been insisted on between man and the steam-engine is close indeed, and we may complacently sit down and congratulate ourselves on the solution which we have found for the riddle of life, and say with the combustionists, How beautifully simple are the means which the Creator employs for its preservations in organised beings!—all the time forgetting that the simplicity and beauty which we so much admire is but the product of our own imaginations. The very utmost that we can do *is to* know the phenomena which constitute our existence in their relations to each other, and to the conditions by which they are immediately surrounded, without pretending to assign their final causes. To the mind which is impressed by the complexity and minuteness of the intimate changes of which living beings are the subjects, there is nothing more distasteful than the attempt to place in an artificial and fictitious causal relation to each other, conditions which *are* actually separated by extensive series of intermediate and subordinate phenomena not yet investigated. We think that it is undeniable that this error has been committed by the chemical physiologists to an egregious extent.

Of the mode of formation and disappearance of the sugars in the animal organism, we can speak with more certainty than of almost any other group of intermediate principles. We have two series of molecular changes (in which the saccharine compounds bear a part), which are consequent, though not dependant, one upon the other. The first of these commences in the stomach by the ingestion of the

amylaceous and saccharine compounds derived from plants, and terminates in the formation of grape-sugar in the liver. The second commences with the formation of grape-sugar, and terminates in that of lactic acid. Let us examine each of these processes in detail.

The alimentary substances capable of yielding glucose are starch, cane-sugar, and sugar of milk. Starch is unaffected by the saliva and gastric juice, but is converted in the intestine into dextrine. Cane-sugar and sugar of milk remain unaltered, and enter the portal veins as such. A trace, however, of glucose is found in the intestine during digestion of amylaceous substances. Dextrine and cane-sugar appear to undergo no change in the portal vein; but the moment that they come in contact with the parenchyma of the liver they are converted into glucose. This is effected, say our authors, by a process of assimilative catalysis, of which they consider "the molecular contact of the dissolved sugar (or dextrine) with the anatomical elements of the liver," to be the essential condition. It has been ascertained that the contact of the hepatic parenchyma with a solution of cane-sugar out of the body, is sufficient to cause it to undergo the glucosic fermentation; but it remains to be observed whether, in the living organ, the change does not take place with much greater rapidity. At all events, we think MM. Robin and Verdeil have been guilty of again disregarding their own principles in considering it as a mere catalysis, inasmuch as other conditions, not belonging to catalyses, are certainly present. And when we reflect that these same conditions are those under which the formation of glucose from elements which have no resemblance to it in chemical composition, is brought about, and that this process is not only peculiar to living beings, but altogether without analogue in the mineral world, it becomes highly probable that both are the results of the organic rather than of the molecular properties of the organ. Respecting the process of the glucose from dextrine, etc., we know the point of departure and the result, and this is, in general, as much as we know even of inorganic molecular changes. But of the other process, beyond the positive fact that the animal is capable of furnishing materials at the expense of its other principles, all that we are sure of is, that the special conditions required are afforded by the liver. Of these conditions we are acquainted only with the most general, those, namely, which refer to the state of the hepatic circulation and of the nervous system.[1] We are entitled to conclude, from M. Cl. Bernard's experiments, that the origin of the sugar may be attributed either to the fats or to the albuminous compounds; and inasmuch as there is reason to believe that the former disappear from the organism in another form, we may consider grape-sugar as a result of the general disassimilative process to

[1] M. Bernard has found that, in a dog, from whose liver the sugar has been caused to disappear by starvation, it may be restored in a few hours by feeding it exclusively with meat.

which we adverted in our former notice. Here, again, we are at fault; for between the albuminous compounds and glucose we are acquainted with no product which we can either consider as intermediate, or, on the other hand, as complementary to the latter, supposing its productions to be the result of what our authors term a "*dédoublement.*"

How does glucose disappear in the organism? Here we have very accurate and satisfactory observations to prove that it undergoes a molecular change remarkable for its rapidity, which results in the formation of lactic acid, or of some intermediate principle. This, the combustionists tell us is a species of burning. Let us hear what our authors say on this subject. After speaking of the impossibility of investigating the question from an exclusively chemical point of view, they observe—

"The truth of this was felt by M. Cl. Bernard, when he said, 'It has been thought lately that the chemical theory of the incomplete combustion of sugar might be supported by the fact that etherisation and other asphyxiative causes which diminish the respiration, are capable of causing sugar to appear in the urine. This, however, does not prove that the passage of sugar into the urine depends upon its incomplete combustion in the lung.' In fact, one of the most certain and most powerful means of diminishing the activity of respiration consists in cutting the two pneumo-gastrics in the region of the neck. Never, however, has M. Cl. Bernard, in his numerous experiments, seen this operation result in glycosuria. Ether, and other means employed, act in the same manner as chlorine, that is, as irritants of the lungs. The irritation is transmitted to the brain by the pneumo-gastric, and the reaction on the liver takes place by the great sympathetic." [1]

To the chapters which our authors have devoted to the physiological history of the fats, we think it is to be objected that they have allowed themselves to be too much influenced by that spirit of exclusiveness, of which, though it nowhere attains to proportions so gigantic as in the English metropolis, the indications are too easily recognised in the writings of the Parisian, and, unfortunately, even of the Edinburgh school. It is for this reason that we have placed at the head of this notice the titles of several very recent German works, which, among the innumerable authors quoted on various subjects, have been altogether overlooked. With respect to the researches of Bidder and Schmidt, in particular, the omission seems inexcusable, inasmuch as they bear not only on the subject immediately before us, but on others, treated of quite as directly as those of M. Cl. Bernard, which are brought before the reader in every chapter.

We may consider the difficult question of the origin of the fatty principles as already decided, in so far as we know that they not only enter the organism as such, but that, under certain conditions,

[1] We think that here MM. Robin and Verdeil have misunderstood M. Cl. Bernard, as, according to his experiments, the reaction in question takes place not by the great sympathetic, but by the spinal cord and splanchnic nerves.

they originate by deriving their elements from pre-existent principles belonging to other groups. All observers are now agreed in admitting the validity of the doctrine first promulgated by Liebig in 1842, that the fats of herbivorous animals are derived from principles of different chemical constitution from themselves. There is no sufficient ground, however, for the opinion, that they owe their origin, in animals generally, to the amylaceous compounds, as Liebig at that time maintained. All that has been proved in reference to the question is, that the whole quantity of fat contained in the aliments ingested by an animal in a given time is, in many cases, insufficient to account for the quantity that accumulates in the tissues, along with that which escapes with the secretions and excrements.

"As the increase of fat takes place alike, whether the amylaceous constituents of the food exceed the nitrogenous or the reverse, we are authorised to say that both orders of principles furnish the materials for the formation of the fats. Besides, it is to be remarked, that, in the experiments which have been made, the materials in question are examined only at their entrance and exit, without following them in their passage through the organism; so that it is impossible to say whether, under ordinary circumstances, they are furnished by one group of principles, in preference to, or to the exclusion of, the other. In a word, in this case we formed our decision rather because we do not see how it could be otherwise, than as the result of direct experiment. Certainly it is still unattained to; and it is possible that, without changing in any respect our general ultimate conclusions, we may eventually recognise several intermediate facts in reference to the compounds which furnish these materials."

All the experiments which have been hitherto quoted have to do with the point of departure and ultimate results of the process, and the question of the principles from which the fatty compounds are immediately derived remains open to discussion. The field, observe our authors, is left to two hypotheses, either of which may be admitted, without affecting the general conclusion.

"1. It may be supposed that the amylaceous or saccharine principles, and the nitrogenous substances, pass into the condition of fatty principles by an assimilative catalysis, without having been fixed in the anatomical elements, and without having ceased to form a part of the blood into which they had penetrated by endosmose—a process which would be in some measure comparable to that by which dextrine or cane-sugar pass into the condition of glucose in the liver. 2. Or, on the other hand, principles already assimilated, and already forming part of anatomical elements, may, under the influence of new conditions, induced by the ingestion of certain aliments, pass into the state of fatty principles by a catalysis of '*dédoublement*' or disassimilation. This last process would be analogous to that by which sugar is formed in the liver—at the expense probably of the nitrogenous substances—either in an animal nourished exclusively with flesh, or in one which, after being fed long and continuously on aliments containing no sugar, forms it from the moment that it is fed with flesh; it takes place, in a word, by an act similar to that by which all the principles of the second class are formed, without taking into consideration the glucose which is derived from cane-sugar or dextrine."

As far as the chemical relations of the various classes of intermediate principles are concerned, we have observations which show

that the transformation either of the amylaceous or of the albuminous compounds into fats is perfectly possible. MM. Pelouze and Gelis[1] have succeeded in producing butyric acid from the former, by a process of fermentation, carried on under certain conditions of temperature, etc., which are at least somewhat analogous to those which actually occur in the living organism; and M. Wurtz[2] has obtained the same body from fibrine. Mr Gulliver has also recorded in the "London Medico-Chirurgical Transactions," experiments which consisted in the gradual conversion of fibrine of blood, when placed under certain conditions of heat and moisture, into fat.

On the much disputed question, so important in a therapeutic and dietetic point of view, of the absorption of the fats from the alimentary canal, MM. Robin and Verdeil are almost silent, although it is certainly included in the proposed scope of the work. The problem of the manner in which the neutral fats (oleine, margarine, butyrine) enter the lacteal vessels from the intestine, yet remains unsolved. The passage of a fatty body through an animal membrane is a phenomenon for which no analogy can be found outside of the living organism; and it certainly seems somewhat unaccountable that, with all the superiority of method on which MM. Robin and Verdeil lay so much stress, they should have almost overlooked this remarkable property of the fatty principles. When the fact was announced by M. Cl. Bernard, that the pancreatic juice possesses the property of decomposing the neutral fats into bases and fatty acids outside of the organism, it was thought that the problem in question had found a solution; that no doubt this decomposition actually took place in the intestinal canal; and that the fats ingested as food entered the circulation in a saponified condition.[3] M. Bernard, however, from the first left this question altogether undecided, and from later experiments, and the comparison of his own with previous observations, especially those relating to the condition of the fats in the lacteal vessels, was led to conclude that saponification did not take place, although he was unable to show by what agency it was prevented. This lacuna was filled up by the observations of Lenz, who made the following experiments with a view of determining it. He first ascertained that in a healthy cat, killed a sufficient time after feeding on fat, there was to be found neither in alimentary canal, nor in the thoracic duct, a trace of butyric acid, although the lacteals were completely injected with a milky fluid. In other animals of the same species, after thirty-six hours' fasting, a ligature was applied to the intestinal canal immediately below the pyloric opening, and below the ligature an incision was made into the duodenum, by which melted butter was injected. A second ligature

[1] Pelouze and Gélis, Comptes Rendus, vol. xvi. p. 1262. 1843.
[2] Wurtz, Comptes Rendus, vol. xviii. p. 704. 1844.
[3] It is upon this theory, we suppose, that those have founded their practice who consider that, in the therapeutic administration of the neutral fats (cod-liver oil), the alcalis ought to be given at the same time.

was then placed immediately above the entrance of the biliary and pancreatic ducts, to hinder the escape of the contents of the duodenum into the peritonæal cavity. On killing the animals from nine to eleven hours later, the contents of the intestine smelt of butyric acid, and reddened litmus paper. But in the thoracic duct, portal vein, etc., it could not be recognised by any means which were applied. For the purpose of proving that this production of butyric acid in the intestine was produced by the agency of the pancreatic juice, the above experiment was varied by replacing the second ligature by another on the pancreatic duct itself. In this case no butyric acid was to be detected either in the intestine or elsewhere. From the preceding experiments it follows that, even in the living organism, the pancreatic juice possesses the property of gradually decomposing the neutral fats, but that this is prevented by the presence of the gastric juice. Further experiments, in which the two secretions were employed outside of the body, have shown that the power of the gastric juice to suspend the decomposition in question, varies according to its acidity, and is entirely lost when it is neutralised by an alkali; the same result, in fact, was obtained by the substitution of a weak solution of hydro-chloric acid, as when the secretion itself is employed. Hence it is obvious that, so far from being beneficial, the administration of alkalis with fatty food must be prejudicial.

Although M. Bernard now admits that "the action of the pancreatic juice on the fats is neither a saponification nor a chemical combination," he maintains that the presence of this secretion is an essential condition of their absorption in another way—namely, as being the only means by which they can be emulsionized; it being taken for granted that they are only capable of absorption when thus finely divided. This last notion, however, seems to have no better foundation than the mere fact that, in the chyle vessels, the fat is found in a state of fine division, for experiments which were originally made by Valentin, and which have been repeated by Lenz (Op. Cit., p. 43), have proved that fatty bodies cannot be caused to pass through an animal membrane in accordance with any known laws of diffusion. Lenz employed in his experiments various animal membranes, among which was included the mucous membrane of the intestine, the fluid placed on the opposite side of the membrane to the emulsion being either water or serum. Admitting, however, the condition of emulsion to be essential to the digestion of the fats, we now see good reason to dispute the proofs, which seemed before to be so irrefragable, of the title of the pancreatic juice to be considered as necessary for the production of this condition. In the first place, the power of forming a permanent emulsion seems to reside, to a limited extent, in other secretions. The bile, in particular, seems to possess it as distinctly as the pancreatic juice, although in a much less degree; that is, a much larger

quantity of bile is required to emulsionize the same quantity of fat, a fact which seems to correspond partly to its inferior concentration, but principally to its containing a much smaller proportion of tenacious mucus.

The observation, however, which seemed to be most conclusive in support of Bernard's doctrine was that, according to which, in dogs and rabbits fed on aliments rich in fat after ligature of the pancreatic duct, the chyle vessels remained transparent, and no emulsion was formed in the intestines. The subsequent experiments of Frerichs, however, did not confirm its accuracy. Frerichs varied the method of Bernard in several ways. In some cases he ligatured the pancreatic duct; in others, he destroyed the gland itself by a succession of ligatures drawn through its substance, and fed the animals with fat; while in others, the animals having been kept a long time fasting, he applied a ligature round the intestine itself, below the point at which the two ducts enter, and injected olive oil into the intestinal cavity. In every instance the chyle vessels were completely filled with a white fluid. When, however, a ligature having been placed upon the intestine about the middle, it was divided, and olive oil injected into both sections, the impletion of the lacteals was more complete in the superior than in the inferior portions, a fact which Frerichs considered to indicate that the bile and pancreatic juice, although not essential to the absorption of the fats, have an influence in promoting it.

These experiments have been objected to by the supporters of M. Bernard, on the ground that, in all of them, the presence of a small quantity of pancreatic juice remaining in the intestinal canal, might be sufficient to explain the results. Whether this objection be valid or not, it is completely met by the still later and more exact experiments of Bidder and Schmidt, who operated as follows. After tying the pancreatic duct in the cat, they divided it between the ligature and the intestine. Then, after allowing the animal to fast at least thirty-six hours, so as to insure the complete disappearance of every trace of previously secreted pancreatic juice, it was fed with an abundant meal of melted butter. This was repeated on the two following days, and, from five to six hours after the last meal, the animal was killed; and in each case a dissection of the parts was made, to insure the complete occlusion of the pancreatic duct. In every experiment the chyle vessels, the receptaculum, and the thoracic duct were filled with white contents. The experiments were repeated with variations, for the details of which we refer the reader to the work itself. We can only account for the discrepancy between our authors and M. Bernard, by supposing that what he observed was the result of the too violent traumatic re-action, and that M. Bernard "had ascribed to the absence of the pancreatic juice, what was simply a consequence of an interruption of the absorbent action in the chyle vessels, induced by inflammation."

Our authors next inquired into the accuracy of the observation

recorded by M. Bernard, and sanctioned by the Academy of Sciences, on the peculiar arrangement of the lacteals in rabbits, according to which, when examined after the ingestion of fats, they are only to be found filled with chyle *below* the opening of the pancreatic duct.[1] Bidder and Schmidt, on the contrary, found that in the greater number of their experiments, the chyle-vessels were injected above the opening of the duct, and that in those in which the examination was made at a short period after the ingestion of the fat, not only were the chyle vessels in question filled with milky fluid, but below, where the fat had not had time to reach, they remained transparent. Only in cases in which the digestive process approached its termination did the appearances described by Bernard present themselves.[2]

If we take into consideration all the experiments of which the outline is given above, we think we must coincide with Bidder and Schmidt in concluding that, in this research, M. Bernard, in spite of the extraordinary caution with which his observations are in general conducted, has, for once, come short of scientific accuracy.

"At all events," they observe, "we must consider the doctrine of Bernard as to the importance of the pancreatic juice in the absorption of the fats, after the detailed examination to which we have submitted them with the strongest prejudice in their favour, as in all its points erroneous, and we would expressly repeat, that the pancreatic juice has nothing to do with the preparation of this kind of aliment."

In reference to the question of the influence which the bile exercises in the digestion of the fat, as first maintained many years ago by Sir B. Brodie, and confirmed by Tiedemann and Gmelin, Bidder and Schmidt, and Lenz under their direction, have recorded some equally valuable experiments. By comparing the quantity of fat ingested by an animal in a given time after the ligature of the *ductus choledochus*, with the fat rejected in the same time, it was found that a very considerable absorption took place. It still remained to determine whether or not, in animals in which the bile is excluded from the intestinal canal, this absorption is normal in its extent. For this purpose it was first ascertained, by experiments confirmatory of those of Boussingault in other animals, that in cats the normal hourly absorption of fat does not exceed 0·6 grammes to each kilogramme of the weight of the animal, and Bidder and Schmidt have arrived at 0·465 grammes as the average of their results in dogs. On the other hand, when the same experiment was repeated on a dog in which the *ductus choledochus* was ligatured, no more than from 0·06 grammes to 0·09 grammes was absorbed; a difference too enormous to be accounted for by any conceivable sources of error. For the further determination of the question,

[1] This observation is facilitated by the peculiarity in the position of the pancreatic duct in these animals, in which it opens into the duodenum about 35 cent. (= 14 inches) below the opening of the biliary duct. This is well shown in the beautiful plate which accompanies M. Bernard's Mémoire.

[2] Lehmann has confirmed this observation. See Op. Cit. Bd. iii. S. 319.

quantities of chyle obtained from animals in the normal condition, and from others in which the *ductus choledochus* was ligatured, were submitted to comparative analyses. In the chyle of the former was found 3·244 per cent. of neutral fats, and 0·058 per cent. of fatty acids existing in the form of soaps, while in that of the latter there was only 0·19 per cent. of neutral fats, and 0·113 per cent. of fatty acids. With respect to the other constituents the differences were extremely inconsiderable.

Although the above researches go to prove the importance of the influence which is exerted by the bile on the absorption of the fats, we must admit that the nature of this influence is yet unexplained. In the absence of more distinct indications, it only remains for us with Lehmann,[1] to assume—

"That the bile conditionates altered relations of adhesion between the oily fluids and membranes capable of being wetted with water, as a consequence of which the passage of fats through these membranes may become possible. The doctrine of the physical relations of fluids of various kinds to different membranes is as yet so little developed, that such an assumption as the above need, in itself, have nothing inadmissible. Bidder and Schmidt, indeed, relate an experiment which seems to point pretty distinctly to such a relation; it was the following: they dipped in oil two capillary tubes of glass, one of which had been previously moistened interiorly with bile; the oil rose much higher in the tube which had been wetted with bile than in the other which was either quite dry, or had been moistened with a saline fluid."

Besides the influence which the bile exerts under normal conditions, it is probable that, when the acidity of the gastric juice is diminished or neutralized, by the ingestion of alcalis, or otherwise, the soda which it contains may conspire to the saponification of the neutral fats under the influence of the pancreatic juice. These soaps may either be absorbed, or, if magnesia or lime be present in the food, may be converted into insoluble forms to be rejected.

Of the mode in which the fatty principles are separated from the organism, we have not such accurate information, and much remains to be observed.

"They disappear," say MM. Robin and Verdeil, "as a species of fatty principles, in order to form other species, without going out of the body. Like the sugars, it is in the organism that these principles cease to exist as compounds of organic origin; it is in the economy that they again become similar to the compounds which are to be found in the external media, and which, as such, are rejected. Thus, the principles of the oils and fats equally undergo a transformation before their exit from the body, for they are only to be found in very small quantity in the products destined for rejection. How does this transformation take place? This is a point that we cannot decide; the instability, however, of fatty deposits in the economy, and their rapid disappearance under certain circumstances, without the increase of any particular principles in the excretions, would tend to induce us to consider the fats as transforming themselves almost directly into carbonic acid and water, either by successive *dédoublements*, or even by direct slow exudation."

According to the combustion doctrine, as it is usually understood,

[1] Lehmann Lehrbuch der Phys. Chem. Art. Verdauung, Bd. iii. S. 323.

the fats are placed at the head of the combustible aliments, and it is thought that, while still in the condition in which they enter the blood, they are at once burnt to keep up the necessary temperature of the body. The various considerations on which we entered in our last notice, led us to question the truth of this doctrine. In the direct experiments of Bidder and Schmidt, we have satisfactory proof of its untenableness. In their treatise on the "*Stoffwechsel*" (or exchange of material which constantly takes place in the living organism), they record the following observations, of which we shall give the details, not only on account of their value in relation to the question before us, but as a specimen of the mode of investigation which they have employed:—

"For the purpose of determining the influence of a fat diet, in comparison with one consisting of flesh exclusively, a cat, which at the commencement of the experiment, weighed 1·45 kilogrammes, after long continued meat diet, was fed for three days with 175 grammes of fat. The animal was then allowed to fast for forty-eight hours; and during the five following days was fed with about 50 grammes of meat daily. Lastly, it was fed with butter for four days more, after which it was again allowed to fast forty-eight hours. During the first three days of fat diet, it was found that, although the weight of the animal diminished from 1453·5 grammes to 1286·1 grammes, the weight of carbonic acid hourly expired had not increased, but, on the contrary, had sensibly diminished. During the second period, when the animal was fed with meat, its weight increased from 1286·1 grammes to 1441·6 grammes, while the weight of CO_2 hourly expired remained nearly the same, viz., on an average, 2·384 grammes. In the third period, the same facts were observed as in the first, namely, the gradual diminution of the weight of the body, and, at the same time, of the expired CO_2. The above results are precisely the opposite of those which we should have expected, in accordance with the theory, that the weight of the body remaining the same, all the absorbed aliment is directly oxydised. They prove that, during the exclusive feeding of carnivorous animals with fat, the constituents of the tissues (albuminous substances) themselves go to supply the loss of material; while the fats, on the other hand, whether as existing in the blood, or as deposited in the tissues, only assume the form of carbonic acid, etc., after going through a greater or less number of intermediate conditions.[1] Of the nature of these conditions we know but little. Experiment has shown that, in animals with biliary *fistulæ*, a rapid diminution of the fat of the tissues takes place, from which fact we may assure ourselves, that at least a part of the fat in question assumes, sooner or later, the form of some of the principles which constitute the bile. Of these the most important, in this point of view, is the cholalic acid, which, as coupled with the two nitrogen-containing principles of the bile, taurine and glycocol—forms the two essential acids of this secretion. This body, by the large proportion which it contains of carbon and hydrogen, approximates to the fatty principles, from which we may suppose it to be derived by a nearly direct transition."

Before bidding farewell to MM. Robin and Verdeil, we wish,

[1] For the more minute details of the above experiments, we refer our readers to the work itself, in which they will be found arranged in a tabular form. They were repeated with similar results; other kinds of fat, however, being employed in the different series of experiments. See Bidder and Schmidt, Stoffwechsel, S. 360-363.

with the permission of the reader, to draw his attention to the extreme beauty of the illustrative atlas by which the work is crowned. We do not know whether most to admire the artistic skill by which the very truth of nature, in the delineation of some of her most beautiful productions, is preserved and perpetuated, or the industry and patience displayed in the collection of examples so numerous and, in many cases, so rare. The attempt to represent, in a systematic manner, the forms of all the crystalline principles which exist in the economy, is unprecedented, and its success is alike creditable to the authors and to the artist, M. Lacherbauer, whose unrivalled anatomical water-colour drawings are already well known. Among the most beautiful of the forty-five plates of which the atlas consists, are those (Nos. 46, 47) in which we have the various forms assumed by M. Verdeil's newly discovered acid (pneumic acid), to which we have already alluded, as probably playing a very important part in the decomposition of the alkaline carbonates of the blood.

If we compare the atlas of MM. Verdeil and Robin with that more recently published by Funke, as supplementary to the "Physiological Chemistry" of Professor Lehmann, we cannot but admit the marked superiority of the former in point of execution, and perhaps of arrangement. But, as the work of Lehmann stands unrivalled, as the most comprehensive book of reference and information extant, on every branch of the subject on which it treats, so the atlas commends itself to the physiological student as much more practically useful than its Parisian rival. Its author proposes not only to represent the forms of the crystallisable immediate principles, but to include "all the objects of physiological chemistry, in reference to which microscopical or micro-chemical research is of importance."

Funke has recorded some original observations as to the much disputed question of the nature of the blood-crystals of Kunde and others. He has succeeded in obtaining these bodies, of whose crystalline nature there can now be no doubt, from the blood of different species of animals. As they are represented in Plate 10 of the atlas, they do not at all resemble in form the hæmatoidin crystals of Virchow. We have been recently informed, on the authority of a private communication from Professor Lehmann, that that observer has obtained a means by which they can be prepared in sufficient quantity for analysis, and that his further observations have confirmed him in his previous conclusions as to their albuminoid constitution. We look forward to the publication of these results with considerable anxiety; as, if accurate, they will set at rest the important question of the crystallisability of the albuminous compounds.

Illustrazione di tutti gli Strumenti Chirurgici scavati in Ercolano e in Pompei e che ora conservansi nel Real Museo Borbonico di Napoli. Compresa in Sette Memorie lette all' Academia Ercolanese. Dal Cavaliere BENEDETTO VULPES, Professore di Clinica-Medica nell' Università di Napoli. Napoli: 1847. 4to. Pp. 94.

Description of all the Surgical Instruments excavated at Herculaneum and Pompeii, and presently preserved in the Royal Bourbon Museum of Naples. Comprised in Seven Memoirs read to the Herculanean Academy. By BENEDETTO VULPES, Cavalier, etc. etc. Naples: 1847.

ALTHOUGH the fact of the discovery of numerous surgical instruments in the excavations at Herculaneum and Pompei, and their preservation in the magnificent Bourbon Museum at Naples, has been long known and frequently alluded to in the works of different travellers, it has remained for the distinguished Professor of Clinical Medicine, in the Neapolitan University, to direct attention to these interesting relics, by the publication of a distinct memoir. Assuredly no one was better fitted for the task, and no one could have more satisfactorily accomplished it. It is consistent with our own knowledge, that with most assiduous care and untiring diligence, Professor Vulpes, seizing the intervals in the occupation of an extensive private practice, and his laborious duties at the Hospital (degl' Incurabili), has made the examination of the nearly two hundred instruments, of which the collection at present consists. The circumstance that, with the exception of a most interesting article in the "Dublin Quarterly Journal of Medical Science" for August 1852, the investigations of Professor Vulpes have not, so far as we are aware, been alluded to in the medical periodical literature of this country, has made us the more desirous to lay before our readers a short summary of what our author has written regarding the armament of a race of surgeons who flourished before the commencement of the Christian era, or at all events, antecedent to the close of its first century.[1]

In 1846, Professor Vulpes having been appointed a corresponding member of the Royal Herculanean Academy, undertook the study and arrangement of all the discovered instruments. This completed, he presented to the Academy, between March 1846 and January of the following year, seven distinct Memoirs, in which the whole collection was classified, and the different instruments described. These Memoirs received the approval and "imprimatur" of the Academy, and hence resulted the publication of the elegant volume which we have now before us. We have also a former publication of Professor Vulpes, being a short extract from the 14th volume of the magnificent "Real Museo Borbonico;" but as the larger work contains the

[1] The destruction of Herculaneum and Pompei occurred A.D. 79.

description of all the instruments, and was published more recently we need not again refer to the latter. The book is not a mere catalogue, or, at best, faithful description of the various instruments of which the collection consists; it is far more—interest, accuracy, and a high degree of authenticity have been secured by numerous allusions to the works of authors who flourished about the period of the destruction of the cities. References, too, are frequently made to Hippocrates and Galen, to Aetius, Paulus Ægineta, and Celsus: and by these it is scarcely necessary to add, the value of the work is not a little enhanced.

"If," says the learned author, in concluding his preface, "these instruments have undergone great improvements from the perfection to which the art of Podalirius and Machaon has attained in our time, acquaintance with them may serve to exhibit the history of that art, and may be useful in throwing light on certain passages of the ancient authors, which would otherwise be obscure and unintelligible; and an opportunity is also afforded for seeing some instruments, of which in these authors merely an incidental notice is made." —P. 4.[1]

With these few remarks we proceed to the consideration of the various instruments in detail, prefacing this by their collective enumeration and by the index to the different memoirs :—

Forceps	With bent or curved claws,	1	
	With claws half spoon-shaped,	1	
			2
Canulas	For the removal of the fluid in ascites,	2	
	For the same purpose, and for the introduction of medicated fluids into any cavity,	3	
			5
Probes	At one extremity having the form of a spatula, at the other being ovoidal (*spatomeli*),	18	
	Ovoidal at one, and concave at the other extremity (*ciatischi*)	21	
	Ovoidal at both extremities (*diperini*)	2	
	Ovoidal at one part, and terminated by a flat button at the other,	1	
	With one extremity pointed, and the other in the form of a spatula,	1	
	Very long, and with a pointed extremity,	2	
			45

[1] It appears that the first discovery of surgical instruments was made at Herculaneum, and that, at a very early period of the excavations. These were commenced in 1713, and Bayardi, in his "Catalogo degli antichi monumenti disotterrati dalla discoperta città di Ercolano," published in 1754, enumerates them. Forty instruments were afterwards discovered at Pompei, in a house on the other side of the gate from the "Strada dei Sepolcri," and called in consequence the "Casa di Chirurgo," and by some the "Scuola Chirurgica." That it really was a school of surgery is highly problematical, and Professor Vulpes, like many others, believes the house either to have been the residence of a surgeon or one in which a surgeon lodged. In addition to these forty instruments, Romanelli, in his "Viaggio a Pompei," published in 1817, mentions the discovery of five instruments in the shop of an oil-merchant, situated between the "Casa di Panza" and the "Via Consolare."

Brought forward,			52
Fourchette for the division of the frœnum of the tongue,			1
Catheters with double curves,			1
Specula	Speculum magnum matricis,	1	
	Speculum ani,	1	
			2
Pincers or Forceps	For anatomical purposes,	15	
	With the form of a tenaille or tenaculum,	1	
	For the removal of hairs,	72	
			88
Tenaculums	Simple,	13	
	With the extremity of the handle shaped like a spatula,	1	
			14
Trident for the cautery,			1
Small Spatula for removing the eschar formed after the use of the cautery,			1
Lancet with blade of silver,			1
Small Spoon for examining the blood drawn from a vein,			1
Cupping Instruments	Cone-shaped,	8	
	Spheroidal,	5	
			13
Instrument for bleeding horses,			1
Levers for elevating depressed portions of bone, etc.,			2
Instruments of Iron	Bistouries,	13	
	Scalpels with bronze handles,	2	
	Scalpels altogether iron,	1	
	Hooks with bronze handles,	2	
	Cautery iron,	1	
	Similar instruments of bronze for like purposes,	2	
			21
Total,			199

We have inserted this list of the discovered instruments because, besides showing their number, it exhibits at a glance the various uses which they fulfilled. What follows is the author's index to the different memoirs :—

"The *first* contains the description of the forceps with the curved claws.

"The *second* discusses the probable use of a peculiar forceps, the extremities of whose limbs are half-spoonshaped and indented. It also contains the description of two canulas for the removal of the dropsical fluid from the abdomen. Besides this canula, other three canulas are also described.

"The *third* relates to the probes and similar instruments.

"The *fourth* to the speculum magnum matricis and speculum ani.

"The *fifth* to pincers, tenaculums, needles, and the cautery trident.

"The *sixth* contains observations on the lancet, the cupping instruments, and the levers; and

"The *seventh* and concluding chapter relates to those discovered instruments which are made of iron."

In shortly noticing the various instruments, we shall follow the arrangement the author has himself observed in his chapters, with this exception, that we shall make those referred to in the fourth chapter or memoir, the subjects of our first remarks.

The fourth memoir, then, which was read to the Academy on the

24th of November 1846, is occupied with the description of two remarkable instruments, the "speculum magnum matricis," and the "speculum ani." The former was found at Pompeii in 1818, and was the first discovered instrument which consists entirely of bronze. It appears to have been immediately recognised as the "speculum matricis." From a very early period, remarks Professor Vulpes, this instrument was known among surgeons, though it afterwards fell into disuse. The fact of its discovery at Pompeii, where it has lain buried during nearly eighteen centuries, clearly proves that at the very commencement of the present era, surgery possessed most satisfactory means for discovering the diseases of certain of the internal cavities of the body. That the knowledge of this means of diagnosis was enjoyed by the ancient physicians is, moreover, abundantly proved by the writings of several authors, and to some of these Professor Vulpes refers. Hippocrates, for example, though evidently unacquainted with the female speculum, speaks of an instrument for dilating the anus, and for observing the condition of hemorrhoids. Aetius informs us that Archigenes used his "dioptrum" for dilating the vagina and for observing ulcers of the uterus; Paulus Ægineta also speaks of his small "dioptrum," for dilating the anus. "With regard to blind fistulæ" (we quote Paulus Ægineta, Sydenham edition, Book 6th, Section 78), Leonides says, "When the fistula is deep, and penetrates the sphincter, whether beginning in the fundament, or arising from a distance, and terminating in the sphincter, after the examination which has been described, we dilate the anus as we do the female vagina, with the instrument for that purpose." Again, in treating of abscess of the womb, Paulus Ægineta observes, "The operator sitting on the right side is to make an examination with a speculum proportionate to her age. The person using the speculum should measure with a probe the depth of the woman's vagina, lest the stalk of the speculum being too long it should happen that the uterus should be pressed upon. If it be ascertained that the stalk is larger than the vagina, folded compresses are to be laid upon the alæ pudendi, in order that the speculum may be placed upon them. The stalk is to be introduced, having a screw at the upper part, and the speculum is to be held by the operator, but the screw is to be turned by the assistant, so that the laminæ of the stalk being separated, the vagina may be distended." In his commentary upon this section, the learned editor of the copy of Paulus Ægineta, from which we have quoted, remarks, "the only difficulty in comprehending his description (that of Aetius), or our author's, arises from our unacquaintance with the construction of the ancient dioptræ or specula." The discovery of the Pompeian instrument removes this difficulty; and in all probability the speculum we shall presently describe, or some modification of it, was the one referred to by Paulus Ægineta, and by Aetius. In this opinion Professor Vulpes evidently coincides. It is manifest, he says, that the speculum with the branches was known to the ancients, it is the

representation of this instrument which is found in the works of Ambrose Paré, of Scultetus, and some other authors. The figures given by them are, moreover, nearly identical with the speculum discovered at Pompeii. One other observation of Professor Vulpes' is important, though the fact which it enunciates is self-evident, that the Pompeian instrument, and those others figured by the authors referred to, are not, strictly speaking, speculums, but dilators. They differ altogether from the ingenious invention of Recamier, at the commencement of the present century, and only serve as speculums by dilating the parts where they are applied.

We have given two illustrations of the "speculum magnum," of which Fig. 1 represents the entire instrument, and Fig. 2 a side

Fig. 1.

Fig. 2.

view of the same. The further explanation of these, as also of the mode of use of the speculum, will be added hereafter.

The third figure which we have given represents the "speculum ani." It, like the "speculum magnum matricis," is made entirely of bronze. It is composed of two pieces, of which the two lower serve as handles, and the two upper as the branches. The two portions of the instrument are approximated by means of a hinge, and are so disposed, that when the handles are brought together, the branches are expanded, and so, when in use, they serve to dilate

the anus. The distance of the greatest divergence of the branches is one inch and one line.

"It is of this instrument," says Professor Vulpes, "that Hippocrates speaks under the name of 'Catoptere' (κατοπτηρ); and when treating of hemorrhoids, he thus alludes to it:—'But if the condy-

Fig. 3.

loma be higher up, you must examine it with the speculum, and you should take care not to be deceived by the speculum; for, when expanded, it renders the condyloma level with the surrounding parts, but when contracted, it shows the tumour right again."[1]

Both of the instruments, of which mention has now been made, and the figures of which have been given, are in a high state of preservation.

(*To be continued.*)

On the Construction and Use of the Microscope. By ADOLPHE HANNOVER, M.D. Edited by JOHN GOODSIR, F.R.S.E. 1853. 8vo. Pp. 100.

THIS little treatise, which originally appeared in the Danish language, and has been translated into English by Professor Hannover

[1] Adam's Edit., vol. ii. p. 829.

himself, will be found a useful guide to much elementary knowledge connected with the use of the microscope. The principles involved in the construction of the instrument are here treated of in considerable detail, but without the appalling array of figures and formulæ which is apt to scare the unmathematical reader. And we may add, that the knowledge thus conveyed is by no means superfluous: for accuracy of observation and avoidance of the numerous sources of fallacy can only be ensured when the observer is well acquainted with the properties of the instrument with which his researches are conducted. The various descriptions of apparatus, as well as the figures in the plates, refer for the most part to microscopes of foreign make; but this will hardly be regarded as a disadvantage by those who know that the cheap and excellent instruments furnished by Nachet and Oberhæuser, are those with which our students generally content themselves.

Part Fifth.

MEDICAL NEWS.

EDINBURGH PHYSIOLOGICAL SOCIETY.

MEETING VII.—*February* 19, 1853.—DR BENNETT, President, in the Chair.

1. *Dr Sanders* showed a biliary calculus taken from the cystic duct, together with a small oval-shaped concretion, white, soft, and resembling putty in appearance, from the same situation. The soft concretion, examined under the microscope, presented numerous crystals of cholesterin, radiated circular crystals of carbonate of lime, similar to those found in the urine of the horse, and acicular crystals soluble in nitric acid with effervescence.

Dr Bennett observed that cholesterin belonged to the fatty matters elaborated during the secondary disintegration of the textures, which are quite distinct physiologically from the primary fatty element of nutrition present in the chyle. In tubercular phthisis, Dr Bennett believed the latter, that is, the fatty or oleaginous element of nutrition, to be deficient, whilst the former, the fatty products of disintegration, are frequently in excess, as observed in the fatty liver, common in that disease. He was of opinion that these different classes of fatty compounds might, in the living body, be converted into one another, but the laws which regulated their transformation were as yet unknown. The distinction, however, should be remembered in our attempts to explain the rationale of the treatment of phthisis pulmonalis by cod-liver oil. Some discussion ensued, in which Drs Bennett, Gairdner, Sanders, and Cobbold, took part.

2. *Mr Barlow* exhibited, under the microscope, a fine transparent blue injection of the blood-vessels of the hair follicle.

3. *Mr Dalsell* showed, under the microscope, a demonstration of the epithelium developed in the claspers of the male frog during the copulative season.

CONGENITAL DISPLACEMENT OF HEAD OF FEMUR.

4. *Mr Mercer Adam* exhibited a dissection he had made of a fœtus, having congenital displacement of the head of the femur (*luxation originelle* of Breschet, etc.) on both sides. Opportunities of examining this malformation in fœtal life, as was long ago observed by Dupuytren, were very rare; hence the present case was interesting. The head of the femur was displaced from a well-formed cotyloid cavity, and lay on the dorsum of the ilium, where it had formed for itself a new cartilaginous acetabulum. The muscles of the thigh and gluteal region were very much atrophied and contracted. Owing to the contraction of the rectus femoris, the patella and head of the tibia were drawn up over the condyles of the femur, and the knee-joint in consequence could not be flexed. The axis of the neck of the femur was abnormal, being placed nearly horizontally in relation to the shaft of the bone, instead of its natural oblique direction. The ligamentum teres was elongated, thicker, and stronger than is usual in fœtal life. This seemed to confirm the views entertained of its function as a purely suspensory ligament, and evidenced in a remarkable manner the provision made by nature to counterbalance the malformation; for, had this child lived, the whole weight of the body must necessarily have been supported by this ligament, which was formed of unusual strength for this end. From the immobility of the knee-joint the fœtus had been unable to assume its normal ovoid form while *in utero*, and accordingly a false joint had been formed in the lumbar region of the vertebral column, to enable the child to adapt itself to the cavity of the uterus. Mr Adam made a few remarks on the probable etiology of such cases; and considered that in the present case the displacement had been caused by the abnormal retraction exercised on the head of the bone by the contracted condition of the muscles affixed to it. The position of the fœtus in utero, as it lies with the legs flexed on the abdomen, was the position of all others which would predispose the head of the femur to slip out of the acetabulum, and to mount upwards and backwards, as in this case, on the dorsum of the ilium.

MEETING VIII.—*March 5*, 1853.—DR BENNETT, President, in the Chair.

1. *Mr de Chaumont* read the following communication

ON THE EFFECTS OF CHLOROFORM ON THE BLOOD.

Much controversy has arisen as to the real nature of the effect of chloroform on the blood. Amussat, Sédillot, etc., have contended that the blood is darkened by it, and that the effect is quite analogous to asphyxia. Gruby, on the other hand, contends that not only is the arterial blood not darkened but that the venous blood is even rendered as bright as the arterial. I have found that when blood is agitated with chloroform it does not coagulate, but becomes quite transparent; and when the blood is venous the colour is rendered as brilliant as that of arterial blood. Sulphuric ether, on the contrary, does not affect the colour of venous blood. Chloroform seems thus to have a special effect of brightening colours, not only in this but in other cases; as, for instance, when it dissolves iodine, the solution has not the deep-brown colour obtained when water, alcohol, etc., are used, but has the beautiful violet hue of the vapour. In some cases chloroform restores the colour of old blood stains on linen, etc., but the effect is not constant. When blood, acted upon by that agent, is examined by means of the microscope, the red globules are found completely dissolved, the field presenting only a homogenous coloured expanse, in which the white globules float unaffected. Chloroform acts on pus very similarly to acetic acid, as it brings into view the double and tripartite nuclei of the globules, although the effect is not so speedily accomplished as by acetic acid. When the blood of leucocythemia is acted upon, the red globules dissolve, and the white present the reaction not of the ordinary white globules of the blood but of pus. With the view of

observing the effects of the administration of chloroform on the blood in the living body, I attempted to anæsthetise a rabbit by the rectum, so as not to interfere with the respiration. Although two drachms were injected no other effect was produced except an intoxicated appearance, which did not give immunity from pain. This corresponds with what was observed by Flourens in the early days of etherisation. Another rabbit was anæsthetised by inhalation in the ordinary way, and died suddenly during the operation. On opening the body the lungs were found florid, and without trace of asphyxia; the auricles of the heart continued to contract for an hour after death; the peristaltic action of the intestines and bladder continued also with tolerable vigour. Venous blood from this animal presented the same reactions as before mentioned. The next experiment was upon mice. Four of these animals were placed alive in a wide-mouthed bottle, and a few drops of chloroform poured into the bottom of the vessel. This speedily caused a great excitement among them, and they soon after died. The examination of their bodies showed the same signs as in the rabbit. The brain was also normal. Another rabbit was anæsthetised by the lungs, and the crural vessels of the right leg exposed and carefully watched during the whole process of inhalation, but without there being any evidence of change in the colour of the venous or the arterial blood. In this animal also an attempt was afterwards made to induce insensibility by the rectum, but without success. The chloroform was, in this case mixed with water, and the intoxicating effect produced was greater and continued longer than in the former. The temperature was also apparently lowered, although no exact observation by the thermometer was made. Duméril and Dumarquay have stated that the temperature is lowered in anæsthesia, and under the influence of ether and alcohol, distinguishing the effects of these agents from common narcotism, as by laudanum, in which the temperature is raised. This last rabbit died after a few hours, and the post-mortem appearances showed that death had been occasioned by acute peritonitis. Here, too, there were no signs of asphyxia, the lungs being of a bright rose colour. In the liver was found a cyst, containing a white putty-like mass, which, under the microscope, showed beautiful oval-shaped cells containing a round nucleus, with several, generally three, oval nucleoli. These, Dr Bennett informed me, were entozoa. The action of chloroform on them was curious; the nucleus swelled up so as to fill the cell, while in another part of the field were seen cells floating empty, and nucleoli floating about in threes, joined by filaments. Hence it seemed as if the nucleus had alone been dissolved, a curious fact when taken in connection with the solution of the red globules of the blood, these being also considered as nuclei.

The above experiments were all subsequently confirmed by a committee of the Royal Medical Society, by whom it was also observed that chloroform dropped upon the intestines and bladder caused violent peristaltic action, and that dropped upon the heart, while that organ was contracting, it caused a cessation of the pulsations until it had all evaporated, having a temporary paralysing effect on that organ.

From the above experiments I am inclined to think that chloroform does not act by asphyxia, and that, when this does occur, it results from the manner of the administration: that its action is directly upon the nervous centres, first upon that part connected with the perception of sensation, and lastly on the spinal cord, or generator of motive force: that although it arterialises the colour of the venous blood out of the body, I have not yet been able to observe the same effect in the body: and that, on the other hand, the arterial blood is in no way affected in colour by its inhalation.

Dr Bennett referred to a report on the action of chloroform drawn up by himself, and printed in the Transactions of the Edinburgh Medico-Chirurgical Society about the time when chloroform was introduced as an anæsthetic. (*Monthly Journal*, Jan. 1848). The results of the experiments there detailed were similar in some respects to those of Mr de Chaumont.

Dr Matthews Duncan remarked, that rabbits were not good subjects for experiments with chloroform, on account of the rapidity with which fatal effects

succeeded the anæsthesia. From recent trials, he was convinced that chloroform introduced into the stomach could produce anæsthetic effects.

A committee, consisting of Drs Dobie, Sanders, and Mr M. Adam, was appointed to report on this communication in reference to Mr de Chaumont's admission as a member of the Society.

MENSTRUATION AND SUPERFŒTATION.

2. *Dr Mathews Duncan* exhibited to the Society a dissection of a gravid uterus, at about the end of the second month of pregnancy. He specially pointed out the mucous structure of the decidua vera, and of the decidua reflexa, and the open state of the Fallopian tubes and cervix uteri up till this period. The adherent plug of cervical mucus was also well seen.

The preparation illustrated the possibility of the menstrual discharge, which occasionally occurred in early pregnancy, being derived from its ordinary source, the lining membrane of the cavity of the uterus. This might be the source of the discharge till the cavity of the uterus was completely occluded by the contact and coalescence of the decidua vera and reflexa, which took place in the third month of pregnancy. The only obstacle to the issuing of the discharge from the uterus was the cervical mucus. But this was also present in the unimpregnated organ, and had to be displaced or opened up for each ordinary menstrual period.

On the same grounds, Dr M. Duncan believed that superfœtation might take place up till some time in the third month of pregnancy. The communication between the vagina and the ovaries was quite free up till that time. By supposing the first child, in a case of superfœtation, born at the end of the seventh month, and already viable, and the second to have been conceived in the end of the third month of the pregnancy of the former, an interval of five months is gained between the births of viable children, a space of time that will include and explain most authentic cases of this occurrence.

Dr D. also made some remarks on the fanciful notions of authors as to the function of the decidua reflexa, in regulating the motions of the ovum on its entrance into the uterus, and showed that they were untenable. (See *Monthly Journal of Medical Science*, April 1853, p. 326).

MEETING IX.—*March* 19, 1853.—DR BENNETT, President, in the Chair.

The reports of the committees recommending members for admission having been read, the following gentlemen were balloted for, and unanimously elected members of the Society:—Dr Mathews Duncan, Mr F. S. B. F. de Chaumont, Mr John Kirk.

1. *Mr Barlow*, on the part of the committee on the spiral structure of muscle, exhibited a demonstration which he had made of the primitive muscular fasciculus (elementary fibre of Bowman) from the fœtal calf, in which the dark transverse striæ were seen to cross each other obliquely, giving rise to the appearance of a spiral arrangement.

Dr Bennett considered, that the appearance presented was to be explained by the polygonal shape of the fibre, and did not indicate a spiral structure.

2. *Mr Kirk* showed a solution of arsenious acid prepared according to Harting's directions (*Monthly Medical Journal*, 1852, p. 375), in which vegetable mould had appeared, although the solution had been made and preserved with care.

CONFERVÆ ON THE PLEURA.

3. *Dr Gairdner* exhibited a portion of tubercular lung, in which perforation had probably taken place, establishing pneumothorax. In the pleural surface were developed a large number of white circular deposits, varying in size from extreme minuteness to $\frac{1}{8}$th of an inch diameter, and extending about $\frac{1}{16}$th of an inch into the substance of the lung. Under the microscope they presented a struc-

ture of the lowest fungoid type, consisting of a mass of very minute oval bodies, sometimes arranged in linear groups of three or four, and insinuating themselves between the fibres of the original tissue. After remaining in contact with a portion of kidney for two hours, similar deposits were found on its surface; but on further exposure for several days, to air and moisture, they were observed to have undergone no change.

Dr Bennett remarked, after examining the preparations, that the vegetable growth now exhibited was the most simple in its formation ever yet described, and was exceedingly interesting in a histological point of view.

TRICHINA SPIRALIS.

4. *Dr W. T. Gairdner* exhibited specimens of trichina spiralis in the pectoralis major and muscles of the thigh, of a man æt. 60, who died after an injury, apparently from purulent infection of the blood. Nothing was known of his previous history as regards the muscular system.

The parasite was found in all the voluntary muscles examined, and generally in all the muscles having striated fibre, except the heart. Several of the worms were found in the recti muscles of the eyes, and in the constrictors of the pharynx, as well as in the upper part of the œsophagus. The cysts enclosing the parasite appeared to be disposed in the interstices of the muscular fasciculi, which were in no case observed to be subdivided by the foreign body. The cysts were almost universally quite opaque from calcareous matter, so that the contained worm could only be seen after steeping the fibre in dilute muriatic acid. Surrounding the spindle-shaped extremities of the cysts, there was seen, almost invariably, a number of rounded yellowish vesicles, having the character of fat vesicles of very small size. The muscular fibres, although rather dry and pale to the naked eye, presented nothing unusual under the microscope.

The structure of the parasite has been carefully studied by various members of the Society. Dr G. referred to Dr Sanders and Mr Kirk for a description of some points in more detail. His own observations had borne out generally the descriptions of Owen.

The animal was evidently a nematode worm, without any very distinct internal organs, and spirally coiled up within a double cyst, of very constant appearance and form. Dr G. said he had not been able to detect more than two layers in the cyst; but it was possible the inner layer (which, according to his observations, was of considerable thickness and transversely nucleated) might have yet another thin hyaline membrane on its internal aspect. Both layers of the cyst were spindle-shaped externally; but the cavity in which the worm floated was quite spherical, and filled with a granular matter. The whole appearance of the parasite was such as seemed strongly to bear out Owen's view, that it was merely the first stage of an animal destined for further development. Dr G. thought it very probable that the muscle was only the hot-bed of ova, which, for their development into perfect animals, required some other habitat. Considering it to be not unlikely that this further development of trichina might take place in the intestinal canal of some carnivorous animal, Dr G. had sent specimens of the muscles of this man, in the fresh state, to Mr Barlow, who had kindly consented to take up the inquiry, and had accordingly administered portions of the muscle to dogs and cats, which he intended to keep under notice for some time, if possible. Should any results of importance accrue from this experiment, Mr Barlow would lay them before the Society.

5. According to *Dr Sanders'* observations, with which those of *Mr Kirk* mostly agreed, the arrangement of the cysts comprised,—1st. An external cyst of fibrous tissue, in which the mineral matter was lodged; it was generally of an elongated spindle-shape, but in some apparently younger cysts was more rounded. Internal to this was,—2d. A tolerably thick layer of a white transparent homogeneous substance, which did not present any distinct structure; but, under the action of re-agents, as hydrochloric acid, exhibited concentric marks. This sub-

stance filled the space between the outer elongated, and 3d. The inner spherical cyst, which contained a granular material, and one, rarely two, trichinæ closely coiled up. This inner cyst was much smaller than the outer one. When well seen, it presented a very finely striated appearance, which Dr Sanders thought was owing to minute cilia, but which was considered to indicate fibres by Mr Kirk and Dr Gairdner.

When the granular matter contained in this inner cyst was effused, it consisted of a plasma, some granular matter, and cells, generally clear, with a single small nucleus, sometimes larger, and containing granules. Mr Kirk thought that these cells were arranged as an epithelium on the interior of the inner cyst.

In the white homogeneous substance placed between the outer and inner cysts, there was sometimes observed, near the narrow end of the cyst, a small round body or vesicle, generally surrounded by granular matter. It was also noticed that a small cyst was occasionally attached at the spindle-shaped end of a larger one; in some examples, conveying the notion that the extremity of the cyst had become elongated, and had afterwards separated to form a new one. It is, therefore, possible that, by means of the vesicle just mentioned, and the process of elongation and division, the trichinæ may be propagated, although it is obvious from the great multitudes of them, and uniformity in their appearance and stage of growth, that some general method of deposition must have taken place. The blood and the surrounding muscular texture were examined, without any traces of their ova being found.

A lengthened discussion ensued, in which Drs Bennett, Gairdner, Sanders, Cobbold, and Mr Kirk, took part.

EXTRA-UTERINE FŒTATION.

6. *Dr Cobbold* exhibited a dissection which he had received from Mr Goodsir, taken from a case of extra-uterine fœtation within the Fallopian tube, which had proved fatal by rupture, at about the twelfth week of gestation. It was stated at last meeting, during the discussion, that in several instances of tubal fœtation, where only one corpus luteum was present, that body had been found in the ovary of the opposite side; and a case of this kind had lately come under Dr Simpson's notice. In the present instance, as also in a very similar case reported by Dr C. in the "Provincial Medical Journal" for 1847, one corpus luteum only was present; but, in both these cases, it occurred in the ovarium corresponding to the side on which the tubal gestation had taken place. The Society's attention was, in particular, directed to the very anomalous position of the left Fallopian tube, the connection between it and the uterine cavity being situated at the cervix uteri, and not on one side of the fundus, as usual. A similar mal-position is recorded by Dr Granville, in the "Philosophical Transactions," accompanied with an engraving of the parts.

7. A specimen of tape-worm, 15 feet long, was shown, which had been expelled by means of the ethereal extract of the male shield-fern (Nephrodium filix mas.)

EXAMINATION OF THE HUMAN MILK IN LEGAL MEDICINE.

8. *Mr Mercer Adam* called the attention of the Society to a new and important use of the microscope in legal medicine. He remarked that there were few cases in medical jurisprudence more difficult to decide than whether, after a few weeks had elapsed, parturition had occurred recently or at a remote period. In such cases of doubt, where delivery is circumstantially believed to have recently occurred, but where all the physical signs may, with equal propriety, be reckoned evidences of this having been at a remote period, he believed that the detection of colostrum corpuscles in the milk would at once decide the question, and, almost with certainty, prove the delivery to have been recent. In illustration of this, he cited the following case, which had recently come under his notice. The body of a newly-born child, much decomposed, was found in a moss in the south of Scotland. It was impossible to decide *secundum artem* whether it had been born alive; but it appeared to have been dead for four or five weeks. Proceedings were taken

to discover the mother, and suspicion fell on a young woman who was *supposed* to have been secretly delivered about four or five weeks previously,—that is, about the same date as the infant was thought to have been exposed. On being arrested on the charge of concealed pregnancy, she said she had had a child a year and a-half before, which she had nursed till within three months of her apprehension, and firmly denied having been recently delivered. The two medical men, who were judicially appointed to examine her, came to different decisions, so equivocal were all the signs, as to the period which had elapsed since her delivery. A microscopic examination of her milk was suggested, and it was found to abound in colostric globules. This decided the *questio vexata*, and showed parturition to have lately occurred. The girl, on being told that imposture no longer avalled, confessed having recently given birth to a still-born child, thereby confirming the accuracy of the revelations of the microscope. Mr Adam considered, that in such cases, the microscope was likely to be as serviceable to the medical jurist as it was in the detection of blood globules, spermatozoa, etc.

VARIETIES, CORRESPONDENCE, ETC.

YELLOW FEVER STRIPPED OF ALL ITS TERRORS.

Mr J. Riddell, acting consul-general at Caraccas, has communicated to Dr Hector Gavin, and Dr Gavin has imparted to the medical authorities of the West Indian Islands, that "an old woman, named Mariquita Orfila, has discovered a perfect remedy for the black vomit and yellow fever, by means of which several persons have been completely cured, after a consultation of doctors had declared that the cases were quite hopeless, and that the patients must die in a few hours. The remedy is the juice of the pounded leaves of the *Berbena*, given in small doses three times a-day, and injections of the same every two hours, until the bowels are emptied. The Berbena is a wild shrub, to be found growing almost everywhere, and particularly in low moist grounds. All our doctors have adopted its use, and now few or none die of those late fearful diseases."

Dr Dempster, deputy inspector-general of hospitals, who has forwarded to us this information, in a letter dated 26th February last, has also transmitted a small specimen of the plant, which Professor Balfour has identified as a species of the natural order *Verbenaceæ*, identical, or very nearly so, with the *Stachytarpheta Jamaïcensis*. On turning up the article *Verbenaceæ*, in Mérat's *Dictionnaire de Matière Médicale*, we find he observes of the *Verbena officinalis*, a plant now in utter neglect, that "its infusion was recommended for headaches, sore throat, jaundice, colic, inflammation of the eyes, and dropsy, *vertus qui tiendraient effectivement du miracle, si elles etaient vraies.*" And such is our opinion of the verbenaceous plant of Mariquita Orfila. It is marvellous, if it be true, that an herb, so innocent in taste, odour, and every other sensible quality, as the article sent to us, is a remedy for yellow fever, or for anything else.

POLITE ASSURANCE COMPANIES.

It is no unusual thing in the intercourse of society for blundering superciliousness to be replaced by cringing servility. It was, therefore, no surprise to us to find, after the Assurance Companies had long denied common justice to the members of the medical profession, in the matter of assurance fees, that, when they had at length come to see their error in contemning so powerful a body, some of them should resort to the very opposite line of tactics, and try to purchase favour by bribery. This has been attempted cautiously, and not without success, by a company which is favoured with the patronage of a notorious medical editor, and which offers for the professional certificate of the

medical attendant of every proposer of assurance, double the ordinary and reasonable fee paid by other companies for this information. But we confess we were not prepared for such a bold attempt at seduction as that which has been more recently made by the directors of a company calling itself the "Merchant's and Tradesman's Mutual Life Assurance Society." In a circular on "Medical Fees," which has been sent to us and most medical men in Edinburgh, we are delicately informed that, "Many gentlemen of the medical profession having an objection to receive a commission on business introduced by them without the intervention of an agent, the directors of this society, anxious to meet the views of the profession, have come to the resolution of allowing the following medical fees in all such cases, viz., L.1, 1s. under L.200; L.3, 3s. for L.200 and under L.500; and L.5, 5s. for L.500 and upwards." Now, if a medical practitioner chooses to unite his profession with the occupation of an insurance agent, let him by all means do so in a regular way, be acknowledged and advertised as such, and take his commission as well as the natural consequences. But, to receive a douceur, under the rose, for his services in supplying "business introduced by him without the intervention of an agent," is a totally different matter. And so we trust our brethren will pause before they swallow the tempting bait of this company—especially as they have forgotten to annex the usual condition in all such equivocal proceedings, that "the utmost secrecy will be observed in these transactions."

RECLAMATION ADDRESSED BY MR SYME TO THE IMPERIAL ACADEMY OF MEDICINE OF PARIS, RELATIVE TO THE REMARKS OF THAT BODY ON HIS MODE OF TREATING OBSTINATE STRICTURES OF THE URETHRA.

Edinburgh, 13th November 1852.

Mr President, the Bulletin of the Academy of Medicine for the 12th of September last contains the report of your committee, appointed to decide upon the claims of candidates for the prize instituted by Monsieur d'Argenteuil for improvements in the treatment of urethral disease, and especially stricture. It is far from my wish or intention to question the justice of their decision. I was not a competitor, and am not affected by the result. But my name has been introduced into the report in such a way as to convey a most erroneous idea of the operation which I have devised for the remedy of obstinate strictures. And I feel persuaded that the members of the Academy of Medicine, when acquainted with the extent to which, however unintentionally they have misrepresented my practice, will not hesitate to relieve it from a reproach that I hope to show is wholly unmerited.

The report states that my operation is more severe than that of Monsieur Reybard, since it requires division "of the whole thickness of the soft parts of the perineum," and characterises it as an "extreme measure,"—the employment of which, by testifying to the urgency of the circumstances requiring relief, enhances the value of Monsieur Reybard's milder method. "I do not wish (says your reporter) to make any comparison between the urethrotomy of Mr Syme and that of our countryman. The two methods, indeed, are not to be compared with reference to their employment in practice." I have to beg that you will permit me to do what your committee have considered out of the question, and compare the two operations in regard, 1st, to their performance, and, 2d, to their effects.

The operation of M. Reybard is performed by introducing through the stricture a sheath containing a blade which may be expanded to any requisite extent by means of a central rod. But as such an apparatus could not be passed in cases of tight stricture, we are told that the patient must be prepared by dilatation sufficient to admit the "Urethrotome," and that if this method proves too slow, or too painful, scarification of the contracted part is proper for accelerating the process. It is also said, that whatever be the form of the

stricture, the incision ought always to be directed laterally, so as to avoid the artery of the bulb placed below. Now, this I affirm to be an anatomical error of the gravest consequences in practice, the arteries of the bulb being seated not below, but at the sides of the canal, and therefore endangered by any incision which is not strictly confined to the middle line corresponding with the raphe of the perineum. In the after treatment of his incision, which, it is said, should be from five to six millimetres in depth, and six centimetres in length, M. Reybard introduces into the urethra, either a metallic dilator with expanding branches, or a bag expansible by mercury, and repeats this process every day for four or five weeks.

In my operation a grooved director is passed through the stricture of such a size as is admitted without using force or causing pain or bleeding, and I maintain that there is no stricture whatever through which an instrument of this kind may not be passed. The patient's limbs being then held up, an incision about four centimetres in length is made exactly in the middle line of the perineum through the integuments and fascia. While the director is held in the left hand of the operator, the knife is guided over the forefinger of his right hand so as to have its point inserted into the groove at the bulb, and is then carried forwards completely through the strictured part of the canal. If the contraction be at any other part of the urethra, the process is conducted in the same way; but as it never exists any farther back than the bulbous portion, it is in no case necessary to cut, as your reporter says, the whole thickness of the soft parts of the perineum, or any thing more than the integuments, fascia and corpus spongiosum, which, being divided in the middle line, has the trunks of its arteries kept perfectly secure from injury, so as to prevent the possibility of hemorrhage. A moderate sized silver catheter is retained in the bladder for two days, and the only treatment afterwards required is the occasional introduction of a full-sized bougie at the distance of eight or ten days, or at a later period, of as many weeks.

With regard to the effects of the two operations, it is necessary to remark that Mons. Reybard maintains all strictures to depend upon the formation of a new texture, which, in every case, essentially requires a free longitudinal division for effecting complete recovery; and also, that he considers urethrotomy inapplicable to the firm non-dilatable strictures which occur in old people, or those in bad health, for which he thinks palliative measures more expedient. It may hence be concluded that in the class of cases subjected to operation by M. Reybard, the strictures were not of the most inveterate, obstinate, and unmanageable kind. Indeed, of all the seven cases particularly mentioned in the report as having been remedied by him, there is no allusion whatever to the duration of the disease, the unyieldingness of its character, or the failure of previous treatment. On the other hand, I maintain that strictures vary in their nature, some yielding to dilatation, others resisting it, and a third sort yielding to the bougie, but permitting no advantage to be derived from it in consequence of a resilient or contractile disposition, which almost immediately renews the symptoms. By means of the simple metallic bougie, I have been accustomed to remedy all the ordinary forms in which the disease presents itself; and even in cases of twenty years' standing, or a still longer duration, should expect this method to prove successful as the general rule—the comparatively small number of those which, from their obstinacy, prove exceptions, being alone deemed proper subjects for my operation. Thus, there is at present under my care a patient who came from Montreal on purpose to have the operation performed, in consequence of prolonged and careful trials of the ordinary means having failed to afford relief. Almost all the cases in which I have operated possessed a similar character of obstinacy, and I have never declined interference on account of the age or health of the patient. In these circumstances it could hardly be expected that the immediate effects of my operations should be less severe than those of M. Reybard. Yet it appears that in thirty-two cases operated upon by him, *one* patient died, while in sixty-six cases operated

on by me, *not one* died; that in his thirty-two cases hemorrhage occurred ten times, but in my sixty-six cases not once; and that in all of his cases bleeding continued for four or five days, while in mine it has never lasted beyond four or five hours.

I therefore contend that my operation is more simple, certain, and safe than M. Reybard's, and also more extensively applicable to that class of cases which chiefly require its performance, from resisting other modes of treatment.

The quarter of a century has elapsed since Mr Stafford, of London, proposed to remedy strictures by internal incision; and, ten years ago, I tried this method in a case which had resisted all the then known means of treatment. I cut the urethra no less freely than M. Reybard, by means of an instrument which, like his, consisted of a sheath, containing a blade protruded at the side by a central rod—so freely, indeed, that without any subsequent dilatation, the largest instruments were passed with ease; but I found that the patient's relief was only of a few days' duration, and therefore concluded that the operation was not adequate to the remedy of stricture in its most obstinate form. The same opinion is now, with hardly any exception, entertained in this country, from the results of ample and varied experience.

In conclusion, I beg to state, as the principles of my proposal—

1. That there is no stricture truly *impermeable*, and that with time and care, in every case, an instrument may be passed through it, and serve as a guide for the knife.

2. That all strictures which cannot be remedied by simple dilatation, admit of effectual relief only through a free division of the contracted part of the canal.

3. That this object can be attained with certainty and safety only by an external incision, in a line corresponding with the raphe of the perineum, upon a grooved director passed through the stricture.

4. That the only after-treatment required, is the introduction of a catheter during forty-eight hours, with the subsequent use of a full-sized bougie at distant intervals.

5. That the operation, if properly performed, is free from any risk whatever of hemorrhage, extravasation of urine, or fistulous opening.

I have the honour to be, Mr President, your most obedient servant,

JAMES SYME.

Paris, le 17th Novembre 1852.

Le Secrétaire perpétuel de l'Académie, a Monsieur le Docteur James Syme, Professeur Royal de Clinique Chirurgicale à l'Universitée d' Edinbrurg.

Monsièur,—L'Académie a reçu, dans sa Seance du 16th Novembre 1852, votre note manuscrit, relative au traitement des rétrécissements de l'urètre, d'àprès une méthode qui vous est particulière.

J'ai l'honneur de vous prévenir que ce travail sera examiné, par une Commission composée de MM. Larrey et Robert.[1]

Agréez, Monsieur, l'assurance de ma considération très distinguée.

DUBOIS.

Edinburgh, 4th April 1853.

Sir,—I beg you will have the goodness to inform me if the committee to whom my reclamation was referred on the 16th of November last have reported on the subject.—I have the honour to be, Sir, your most obedient servant,

JAMES SYME.[2]

Dr Dubois.

[1] M. Robert was the author of the report impugned.

[2] To this communication no answer has as yet been received—April 22, 1853.

THE PROVINCIAL ASSOCIATION OF ENGLAND AND MR SYME.

(*Mr Syme to the Editor of the Association Journal.*)

Edinburgh, 15th April 1853.

SIR,—In the 19th volume of the Transactions of the Provincial Medical and Surgical Association, I have just read the following passage of an "Address in Surgery," delivered at Oxford in July 1852, by James Torry Hester, Esq.:—

"If we cast our eyes beyond the Tweed, we see the melancholy exhibition of a city, celebrated through long ages as the birth-place of genius, torn by two rival factions and a petty squabble about the perineal section. It is not in these days a question whether the operation originated with Dessault or Syme, nor whether it be adapted for the cases in which it has been recommended. The ultimate condition of the patient is lost sight of in fierce animosity and personal invective, and the two leading surgeons have appealed to a Court of Law upon a question of truth. *Proh pudor!* It were vain to expect them to issue from the contest humbler, better, and wiser men. But should their repentance be ever so great, it will never efface the scandal such proceedings bring upon the profession.

"That such scenes may shortly cease to be enacted in the modern Athens, is my most earnest prayer; and as to ourselves, may we ever be delivered from envy, hatred, malice, anger, and all such pestiferous perturbations, which militate in no small degree against that tranquillity of mind so essential to the practice of the healing art. Were we to take a retrospect of the whole of this unhappy quarrel, in which direction must we turn for liberality or courteous bearing? Where is the trace of industry or of research? It is not even clothed in sparkling wit or glowing language. No! The terms which have been selected would have better suited the tavern or the hustings. If there be value in the practice recommended, we, as independent but anxious trustees of the public health, have a right to ask for a new series of experiments, under circumstances which would admit of no misunderstanding. Until then, no violence of language, no unblushing assertion, even in the form of advertisements in the public journals, will have weight to influence our judgment."

It is possible that the opinions of Mr James Torry Hester, whose name is entirely new to me, may not possess the greatest weight, and it would be unreasonable to hold the Association responsible for all that is uttered by their annual orators. But when a statement such as that just quoted is made at a public meeting, and afterwards published in the "Transactions," it acquires a stamp of approval which entitles it to pass current as deserving of credit. Now, having throughout the whole discussion relative to the treatment of stricture by external incision, scrupulously avoided the use of language calculated to give personal offence—having resisted the urgent advice of my legal agents to prosecute the libellous calumnies on my practice which have been published in London, re-published in Edinburgh, and anonymously circulated throughout the country—having been satisfied with denying the truth of these injurious allegations, and declined a controversy with persons whom I regarded as utterly unworthy of notice—having had this defensive course twisted into a charge of aggression, which a jury on two different occasions, without hesitation, declared to be groundless—and having thus incurred an expense of more than L.500 in maintaining a principle of treatment, which I held to be of practical value, I did not expect to receive the censure of such a body as the Provincial Association, especially when the result of my efforts had been an addition to the resources of surgery. The "faction" opposed to it in Edinburgh never consisted of more than Dr Müllar and his associate; and as the former for reasons which your ingenuity may probably suggest, has lately disappeared, the latter is now its sole representative.—I am, etc.,

JAMES SYME.

(*Mr Syme to the President of the Provincial Association.*)

Edinburgh, 22d April 1853.

SIR,—I beg to direct your attention to the 158th page of the 19th volume of the Transactions of the Association just published. As it is impossible for me to remain connected with a body which could tolerate such a statement, or sanction its publication, I must desire my name to be withdrawn from the list of Associates.—I have the honour to be, Sir, your most obedient servant,

JAMES SYME.

The President of the Provincial Association.

PUBLICATIONS RECEIVED.

On Pyæmia. By Joseph S. Gamgee, Esq. From Association Medical Journal. London. 1858.

On Continuous Molecular Changes, more particularly in relation to Epidemic Diseases. By John Snow, M.D. London: Churchill. 1853.

On Diseases of Women and Ovarian Inflammation. By Edward John Tilt, M.D. London: Churchill. 1853. Second edit.

First Report of New York Ophthalmic Hospital for 1852. New York. 1853.

Report in Senate of the United Senate on Anæsthetics. 32d Congress, 2d Session. 1853.

The Modern Advancement of Practical Medicine and Surgery: An Inaugural Address to the Medico-Chirurgical Society of Edinburgh. By James Y. Simpson, M.D. From Monthly Journal of Medical Science. Edinburgh. 1853.

Contributions to Pathology. By William Aitken, M.D. From Glasgow Medical Journal. Glasgow. 1853.

Nederlandsch Weekblad voor Geneeskundigen. Afleveringen voor October, November, December, Januarij. Amsterdam. 1852-3.

Nederlandsch Lancet, Julij, Augustus, en September. s'Gravenhage. 1852.

Vierteljahrsschrift für gerichtliche und öffentliche Medicin. Von J. L. Casper, II. Band, 2 Heft. Berlin. 1852.

The right of Marischal College and University, Aberdeen, to confer Degrees. By one of the Professors. Lewis Smith: Aberdeen. 1853.

Verhandlungen der Physicalisch Medicinischen-Gesellschaft in Würzburg. Dritter Band. III Heft. Würzburg. 1852.

On Inflammation of the Breast, and Milk Abscess. By Thomas William Nunn. London: Renshaw. 1853.

De Voedingsbeginselen: Grondslagen eener Algemeene Voedingsleer. Door F. C. Donders. Tiel. Gebr. Campagne. 1852.

Over den Invloed der Luchtsdrukking op de Hartswerking. Door F. C. Donders. (From Nederlandsch Lancet.)

Two other parcels, directed to Professor Bennett and Dr Sanders, by Professor Donders, have been received and duly transmitted.

TO OUR SUBSCRIBERS.

The lithographic plates which should have illustrated Dr Peacock's paper in our present Number, had not reached Edinburgh when the Journal was sent to press. Our London subscribers, and others who may be disappointed, will receive copies of the plate with the June Number of the Journal.—EDITOR.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Case of Gun-shot Wound of the Left Lung terminating in Recovery, and a Case of Gun-shot Wound of the Skull, followed by great loss of cerebral substance, also terminating in Recovery.* By GEORGE MACKAY, M.D., Edinburgh, L.R.C.S.E., H.E.I.C.S., one of the Medical Officers attached to the Madras Division of the Army of Burmah.

THE two cases which I am now about to relate have both occurred in natives of Burmah during the late hostilities in this country; and I am induced to lay them before the profession as instances of the serious injury that two such important organs as the lungs and brain may occasionally sustain without necessarily leading to a fatal termination.

CASE I.—On the 26th October 1852, a Burman of the middle age, spare habit of body, and healthy appearance, was admitted into the hospital of the 35th Madras regiment, under my charge. On his way up the Irrawaddy river to Prome, to seek protection in the British camp, he was wounded with a musket-ball by a Burmese soldier. The ball entered the chest from behind, close to the lower angle of the left scapula, passed through his body, and could be felt under the integuments and muscles at the lower margin of, and projecting beyond the level of the anterior surface of the fourth rib, about three inches from the left margin of the sternum. The patient expectorated large quantities of blood, and there was considerable emphysema around the ball anteriorly and at the posterior opening. His countenance was expressive of great anxiety; breathing difficult and hurried; and he was unable to lie down. Pulse small, wiry, and quick; skin warm; tongue foul; bowels said to be regular. The wound had occurred about ten hours previous to his admission, and he was said to have lost a large quantity of blood, chiefly by expectoration.

A pad of lint, wet in cold water, was placed to the wound on the back,—a broad bandage applied round the chest,—the patient placed in a semi-recumbent position, and a solution of tartar emetic, with

laudanum, given every hour. Rice-water as a drink was the only nourishment allowed.

28*th*. He continues in great pain with difficulty of breathing; still expectorates blood, but in less quantity; the external emphysema continues, but has not increased; pulse small and weak. Tartar emetic and laudanum mixture to be continued every second hour, and the other treatment as before.

November 1*st*. Breathing much more easy, talks freely, and can now lie down. Expectorates very little blood; emphysema diminishing; pulse still small, rather quick; bowels confined. Wound on the back is inclined to slough. ℞. Ol. Ricini ʒj., to be given immediately. Continue tartar emetic and laudanum mixture three times a day. Spirits of camphor and a poultice to be applied to the wound.

3*d*. Is doing well; the expectoration of blood has ceased. Omit the mixture. Continue poultice to the wound.

16*th*. Has continued to improve since last report. The emphysema has entirely disappeared. Says he has no pain; breathes easy and feels well, but weak. The wound in the back is cleaning, and taking on a healthy action. Poultice to be continued. He is now allowed his usual diet, namely, boiled rice.

The ball could still be felt in the position above described; and it was considered advisable to extract it. For this purpose, the patient being placed in the sitting posture, the skin of the thorax was drawn upwards as far as possible, held there by an assistant, and a horizontal incision about one and a half inches long was made down to the ball, which was then seized with a pair of common dressing forceps, and with some little difficulty extracted. The difficulty arose partly from the shape of the ball, which, instead of being smooth and round, was smaller than an ordinary musket bullet, and, like most Burmese bullets, consisted of a piece of hammered lead, presenting numerous rough angles, and it had not entirely passed through the rib, the posterior half being still embedded in the bone. The ball being extracted, and a few loose granular portions of bone removed, the integuments were let go, when the incision in the skin was found to be at least an inch below the injured rib, thus effectually closing the opening into the cavity of the chest, which might have existed on the removal of the ball. A few straps of adhesive plaster were applied, and the patient continued to do well. The wound in the back healed, as also the incision made in front; and on the 26th November, exactly one month from the date of admission, he was discharged cured, expressing himself greatly astonished at his rapid and perfect recovery.

Remarks.—The perfect confidence with which this patient, and many other Burmese, who have applied for aid at my hospital during the last few months, submitted to European treatment, is very remarkable. Experience alone has obliged most of the natives of India to acknowledge our superiority; but the Burmese have no

absurd differences of caste, and consequently are free from all the prejudices inseparably connected therewith. Their own native doctors have no idea of medicine, properly so speaking, and of course still less of surgery. The mortality among their patients is, from all accounts, something fearful. The above certainly proved a much more successful case than the symptoms at first could have warranted one in anticipating. We hear frequently of musket balls taking an extraordinary course, passing round the parietes of a cavity instead of going through it, etc., etc., instances of which I have met with during the present war, but, in the above case, the profuse expectoration of blood, great difficulty of breathing, and emphysema around the wound, in the back as well as under the integuments of the thorax in front, sufficiently prove that the ball had traversed the lung. Regarding the treatment, I have only to say a few words on the time selected for extracting the ball. My reason for not cutting it out on the patient's admission was, that from the course it seemed to have taken, the quantity of blood he had expectorated, and was still expectorating, and the general severity of the symptoms, I thought it not unlikely that some of the vessels in the neighbourhood of the heart, or even that organ itself, had been wounded, in which case removing the ball, inflicting another external wound for that purpose, could not benefit the patient or suppress the hemorrhage going on internally, while in a Burman I feared no bad effects from allowing it to remain, which I would have hesitated to do had my patient been an irritable or plethoric European, in whom the presence of the ball would probably have been productive of serious evil from its irritation, particularly as it was lodged in a bone. Non-professional persons in general attach great importance to the extraction of a ball; and this patient was anxious for its removal; it was accordingly effected when all symptoms of the internal wound had ceased, to save him from all risk of future annoyance from its presence.

Case II.—On the 8th November 1852, a Burman, a middle-aged man, was brought to hospital, having received a gun-shot wound on the head, about the middle of the left parietal bone. The ball seemed to have, as it were, ploughed through the structures for a space of about two inches, tearing the scalp and fracturing the skull to nearly the same extent, then passing off without entering the cavity. On introducing the finger into the wound, it passed into the cranial cavity, where numerous small fragments of bone were felt pressing directly on the brain. These, to the number of fourteen, were carefully removed with a dressing forceps, a portion of protruding brain was cut off, the head shaved, the edges of the lacerated scalp were brought together, one or two sutures applied, also a few straps of adhesive plaster, and lint dipped in cold lotion ordered to be kept constantly over the part. The wound had been inflicted about four hours previous to his admission, and had bled

freely. He was perfectly sensible; pulse weak; skin cool. With assistance he walked up a few steps into the ward, and was observed to drag his right leg slightly. He was ordered to be kept perfectly quiet; to have rice-water to drink; no medicine.

9*th*. Pulse has risen during the night—is at present strong; skin warm; bowels not moved; right leg more paralytic, and right arm completely so. He complains of pain all down the right side. Strapping to be renewed; continue cold lotion to the head. ℞. Pulv. Jalap Co. ʒj., to be taken immediately.

10*th*. Skin still warm; pulse full; bowels moved three times yesterday; tongue furred; complains of pain in the head, particularly on being moved. The brain is protruding between the edges of the wound in the scalp. Projecting portions of brain to be cut away; strapping renewed; continue cold lotion. ℞. Calomel gr. iij., Pulv. Jalapæ Co., ʒij. M., to be taken immediately.

12*th*. Two of the sutures have given way; the edges of the wound in the scalp look unhealthy, and are discharging freely; a fresh portion of the brain is rising up between them. Pulse quiet; skin cool; tongue clean; bowels open; urine free. Diet to be a little more nourishing. No medicine.

14*th*. Wound much the same, discharging freely; protruding portion of brain becoming offensive; bowels regular; pulse and skin natural; tongue clean; complete paralysis of right side of body, with little or no sense of feeling. The portion of brain protruding beyond the skull (about the size of a large nutmeg) to be cut away; a graduated compress of pieces of lint, oiled, placed over the opening, and secured with straps of adhesive plaster. Meat diet.

19*th*. Brain still continues to protrude. Two portions, about the size of a nutmeg, have been removed since last report; discharge less—still offensive. The right side of the body is wasting. The wound is dressed daily, and the graduated compress still used.

24*th*. Continues much the same; discharge from the wound still copious. Brain continues to protrude; two more portions of the same size have been removed. Is feverish this morning; tongue furred; pulse quick. ℞. Mist. Salinæ ʒss.; Spt. Ether. Nitrici ʒj.; M. to be taken three times a day. ℞. Pulv. Antim. Co. gr. v.; Calomel gr. iij.; Potass. Nitrat. gr. viij. M. Ft. Pulv. To be taken at bedtime.

25*th*. Bowels moved very freely; fever gone; wound still discharging freely, but the protrusion of brain, since it was last removed, has been very slight. Continue graduated compress and keep the wound clean.

December 6*th*. Brain continues to protrude; the graduated compress, with strapping and bandage, does not prevent it; three portions have been cut off since last report. The wound requires to be frequently dressed on account of the copious offensive discharge. He is becoming much emaciated; now takes animal food three times a day, and Pulv. Cinchon. ʒss. every fourth hour.

14*th*. Only one portion of brain has been removed since last report, and none has protruded since the 11th. Discharge still copious, less offensive; edges of the wound looking healthy and granulating. Continue compress; bark and diet as before.

19*th*. No further protrusion of brain; has been doing well since last report. Since yesterday has been observed to move his right arm slightly. Wound in the scalp contracting; healing at both ends. Continue.

22*d*. Wound doing well. Can now move both the right arm and leg slightly.

29*th*. Wound continues to contract, and the power of motion in the arm and leg is increasing.

January 13*th*, 1853. Has improved steadily since last report. The wound in the scalp has contracted; but there is still a space over the broadest portion of the fractured skull about the size of the point of the thumb, and from the thickening of the scalp the edges cannot be brought together, but it is gradually contracting and granulating from below. The brain, particularly on any exertion, can be seen pulsating at the bottom of the opening. Strapping across the wound. Continue nourishing diet and bark.

17*th*. Is gaining flesh, and daily getting more power in the right arm and leg. He can now raise his hand to his mouth in eating, and walks with a little assistance; he feels well.

20*th*. Yesterday he left the hospital without asking leave, and returned this morning. He had walked to his own house in the bazaar, a distance of nearly a mile; says he is quite well, and is very anxious to be discharged and allowed to go home. The thickening in the scalp is diminishing, and the opening contracting. He continues to gain flesh and strength, and the right arm and leg are nearly as useful as the other.

21*st*. He was advised to remain in hospital a few days longer, but he felt so well that he could see no necessity for so doing,—so walked away of his own accord, and as he has not returned, or applied again for assistance, there is no doubt but that he has continued well.

Remarks.—The slight constitutional disturbance presented by this patient from the first, is remarkable, considering the severity and extent of the wound; also the recovery of the power of motion and sensation in the right arm and leg after so great a loss of cerebral substance. The elongated and irregular shape of the opening in the skull, rendered it difficult to apply a compress effectually, added to which its application producing uneasiness, the patient made a point, when not narrowly watched, of removing the bandage or plaster; and knowing the difficulty there would be in keeping any dressing properly adjusted, was my reason for, in the first place, applying the interrupted suture to the wound in the scalp, a practice which, in general, is considered inexpedient.

Such cases prove that hope may be entertained even after most desperate wounds. These are two of the most remarkable I have

met with during the late hostilities in Burmah. Fortunately, considering the extent of territory gained, our losses by the hand of the enemy have not been very great; but enemies far more formidable than Burmese soldiers can ever be, have been creating sad havoc in our camps. Cholera, dysentery, and fever, have been thinning our ranks. The army surgeon is frequently, of necessity, exposed to the bullets of the enemy; should he be wounded it is a matter of no consequence, he is a non-combatant and only a doctor; should he escape, he must be ready to act the part of a skilful surgeon, and generally under disadvantageous circumstances; but even before the enemy, this is but a small portion of his duty, particularly in the East, which has now become so extensive a field. He will find his energies much more severely taxed in combating those deadly enemies, *coup de soleil*, cholera, dysentery, fever, etc.—a circumstance that cannot be too strongly impressed on the minds of those about to enter the medical department of the army. Military surgery is too frequently made to comprehend only the treatment of wounds and injuries as met with in an army in the field, which in reality differs little, if at all, from that which the civil surgeon is called upon to exercise in his daily hospital practice; whereas there *is no* class of medical men whose duties are so varied as those of the army surgeon.

CAMP MREADAY, BURMAH,
21st Feb. 1853.

ARTICLE II.—*Cases of Amputation at the Ankle Joint, and Restoration of the Lower Lip.* By CHARLES CROKER KING, M.D., F.R.C.S.I., M.R.I.A., Professor of Anatomy and Physiology, Queen's College, Galway, etc.

CASE I.—Biddy Fanington, aged 28 years, a patient in the County Galway Infirmary. Disease, warty ulcer of Marjolin, occupying the entire sole of the foot in front of the heel.

In consultation on the 21st of June 1852, amputation was decided upon, and the next question for consideration was as to its site. Amputation below the knee was proposed, and consented to by the patient, but was not adopted, as, on consideration, the case appeared to be a favourable one for amputation at the ankle-joint, after the manner recommended by Mr Syme. Accordingly, the patient having been placed under the influence of chloroform, the point of a small-sized amputating knife was inserted close to the fibular malleolus, and carried across the sole of the foot, in front of the heel, to a point on the same level on the opposite side, which was a little below the tibial malleolus. The knife was then carried rapidly across the dorsum of the foot in a semi-circular direction, so as to connect the extremities of the first incision; the posterior flap

was dissected, the edge of the knife being kept close to the os calcis; the ankle-joint opened; the lateral ligaments divided; the entire foot removed; and the operation completed by sawing off the two malleoli, and a thin slice of the tibia connecting those processes. Three vessels required ligatures, namely, the anterior tibial, and the two plantar arteries, care having been taken not to divide the posterior tibial before its division.

The operation only occupied a few minutes, and little or no blood was lost.

The flaps matched admirably. They were held together by adhesive plaster, and three points of interrupted suture. Water dressing was applied.

Almost the entire of the wound healed by the first intention. A small portion of the cicatrix remaining tender, and discharging a few drops of pus, slightly protracted the cure; but this shortly healing, the patient possesses a most efficient stump, upon which, having been provided with a suitable boot, she is able to walk rapidly, perfectly, and without even the assistance of a stick.

CASE II.—Mary Barker, aged 66, admitted into the Galway Infirmary, 11th August 1852. A large cancerous ulcer occupies the lower lip, extending from one angle of the mouth to the opposite, and on the left side even extending beyond the commissure of the lips. The disease was of four years' growth. One gland under cover of the lower jaw was enlarged.

The entire of the diseased mass was removed by a triangular incision, and the contaminated gland also removed. The incisions included the entire of the lower lip, consequently the front of the lower jaw, gums, and teeth were exposed. The incisions were now extended into the neck, and two triangular flaps formed, as recommended by Mr Syme. The entire of the incisions (with the exception of raw edge, which now formed the free margin of the new lip) healed by the first intention.

May 1853—nine months after the operation.—No return of disease; can articulate even labial consonants most perfectly, and retain any fluid in the mouth.

CASE III.—Thomas Ward, aged 40, cancer of lower lip of eighteen months' standing. Disease occupies the entire lip; the patient is obliged to wear a bandage across the chin to hide a huge ulcer.

A similar operation performed with *equally* favourable results. Wounds healed by first intention.

I have had drawings made of both cases, before and after the operation. The cicatrices are scarcely visible. A neighbouring practitioner, who saw Thomas Ward's case both before and after operation, is sadly puzzled. He perceives that the disease has been removed, and a new lip formed; but where the latter came from, he told my patient, was beyond his comprehension.

ARTICLE III.—*A Particular Alteration of the Natural Percussion Sound, observable in certain Cases of Pleurisy and Pneumonia.* By W. O. MARKHAM, M.D., Assistant Physician to St Mary's Hospital, London.

THERE is a peculiar character of the thoracic percussion sound, observable in certain cases of alteration of the lung's structure (whether this be brought about by the inflammatory depositions of pneumonia within the organ, or through its compression by pleuritic effusions), to which little attention has been paid by systematic writers upon auscultation. According to the views of those, however, who have made it a special subject of investigation, it should be ranked as one of the regular signs of pleuritic effusions, and as a not unfrequent accompaniment of pneumonic inflammation. To judge of it from my own experience, I should say that its occurrence was rather common than rare. If this be true, and I am convinced that further experience will prove it so, it follows that its recognition becomes a matter of considerable consequence; for, misinterpreted, it will be apt to misguide the opinion of the observer as to the nature of the disease he is investigating, and, overlooked, a valuable diagnostic aid is lost to him. I have therefore ventured here to call the attention of those interested in auscultation, to some particular consideration of the nature and history of the sign alluded to.

The percussion sound in question has been spoken of by authors, under the names of tympanitic, amphoric, tubular, tracheal, etc.; and has been noted as presenting itself in certain cases of pleurisy and of pneumonia. Many years ago, attention was directed to it by Dr Hudson and Dr Graves, in the early volumes of the "Dublin Journal," as a remarkable phenomenon appearing under peculiar conditions in pneumonia, and as one of the accidents of that disease. Neither has it escaped the notice of other authors subsequently; but Skoda, as far as I know, is the first systematic writer who has investigated its real nature, and ascribed to it its full and just value as an indicator of thoracic disease. Dr Williams speaks of it as occurring occasionally in some particular forms of pleurisy and pleuro-pneumonia, where the pleuritic effusion compresses the upper lobe of the lung against the anterior walls of the thorax, in the subclavicular region, so as to render solid the vesicular structure of the organ. Dr Walshe also notices its occurrence in pleurisy, and bears witness, moreover, to the correctness of Dr Hudson's observation, that the percussion sound may, "under certain circumstances of the locality of the inflammation in pneumonia, acquire a tubular or even an amphoric quality." "In much rarer cases," adds Dr Walshe, "the percussion sound may be purely tympanitic over the hepatized lung. I have found this at the anterior base on the *right* side." Dr Roger, in an excellent memoir published in the twenty-

ninth volume of the "Archives Générales de Medicine," has shown that a peculiar character in the percussion sound, "une résonnance plus grande, ou du moins toute particulière, du côté de l'épanchement dans la region sous-claviculaire," is almost constantly present in cases of pleurisy accompanied by effusion. This altered character of the sound he observed in forty-one out of fifty-one cases, and he remarks, that in the ten cases where it was not observed, their history and other circumstances tended to show that one of the conditions apparently requisite for its manifestation was wanting, *i. e.*, an amount of fluid in the pleura, sufficient to compress the lung upwards against the anterior wall of the thorax. Skoda gives us no reference as to the frequency of the presence of the tympanitic percussion sound (as he names it) in pleurisy, from his own observation of it; but we may gather from the account he gives, that he regards it as a constant attendant of the disease, whenever the fluid is present in such quantity as to compress the upper lobe of the lung against the walls of the thorax. His account, however, of the mode of its production, we may observe in passing, is very different from that given by Dr Williams. We will venture to add, as the result of our own of course much more limited experience, that pleuritic effusion, when the fluid exists in such quantity as to render dull to percussion the lower half of the thorax, and when the upper portion of the lung is the subject of no other morbid change, excepting that produced by the pressure of the fluid in the pleura, is almost invariably, if not invariably, associated with an alteration of the healthy sub-clavicular percussion sound; and that the alteration comprises several varieties of the abnormal percussion sound, being of an empty, tympanitic, tubular, metallic, or amphoric character; but the degree of distinctness with which the altered sound falls upon the ear varies much—varying, doubtless, with the changing condition of the lung producing it. At one time, it approaches the metallic chinking of the bruit de pôt fêlé, and at another, differs so little from the natural percussion sound of the lung, that it would be hard to signify the difference in words. The distinction, however, is readily appreciated by the careful ear of the observer, when he compares the abnormal sound with that produced by corresponding percussion over the healthy lung.

It becomes difficult, therefore, to assign to this altered sound any satisfactory definition, and it is impossible to represent it truly by any one term. Dr Williams speaks of the sub-clavicular percussion sound presenting in certain forms of pleurisy, "an uncommon development of the tubular or amphoric stroke-sound." "I have heard," he continues, "the stroke-sound under the clavicle in such a case, more tubular and clear than that of the trachea itself; in fact, it was quite a musical note. This has been mistaken for a tympanitic sound, and the case confounded with pneumo-thorax." Dr Walshe writes—"But in certain instances (of pleurisy), the

upper part of the chest, especially about the clavicle, first and second ribs, and adjoining part of the sternum, even to the other side of that bone, acquires a tubular, amphoric, or tympanitic resonance. or tympanitic and amphoric combined." These same sounds, as we have before remarked, have also been recognised by Dr Walshe, as the occasional but rare attendants of hepatization of the lung; and we would here observe, that we have spoken of this abnormal percussion sound indifferently, as occurring both in pneumonia and in pleuritic effusions, because we believe its characters (when it exists) are alike in each of these dissimilar affections. The experiments of Skoda, moreover, to which we shall presently have occasion to allude, leave little doubt that the pathological condition of the lung which gives rise to the sound, is the same in both cases. The reason of its being so much more frequently recognised in pleurisy than in pneumonia, will, we trust, become manifest enough upon further consideration of the subject.

Dr Hudson[1] writes, respecting the "tympanitic clearness on percussion over a solidified lung, without air being present in the pleura"—"I do not know any word to express this sound; it varies in degree, rather than in kind, from that produced by percussing over the empty stomach or cœcum, and is like the 'tintement metallique' of percussion of Martinet and Andral, sometimes heard over a tubercular cavity; it is also sometimes as clear as in a case of pneumo-thorax. It could not be confounded with the clearness of the healthy side, unless by a very careless observer; but it is still more different from the dull sound usually met with over hepatized lung." Dr Stokes[2] calls the abnormal sound "a tympanitic clearness over the diseased lung."

It is impossible that any one term should truly represent this abnormal sound, which presents so many different characters. Perhaps no one, however, is less objectionable than "tympanitic," used generically to include them all; this is the term employed by Skoda. Strictly speaking, "tympanitic" should represent an exaggeration of the healthy pulmonary percussion sound. "Le son tympanique de la poitrine," write MM. Barth and Roger, "est tout à fait analogue à celui qui rend à la percussion l'hypochondre gauche quand l'estomac est distendu par des gaz." In this sense, the tympanitic is a full, clear, and drum-like sound, having neither a tubular nor amphoric, nor metallic character. We do not believe, however, from our own experience, that it is used in this restricted sense by practitioners in general. Certainly it is, in common use, made to include those sounds having a metallic timbre (which sometimes, indeed, approach the bruit de pôt fêlé note), produced by percussion over the stomach when its walls are in a relaxed condition.

To prevent any misconception, however, on the subject, we shall

[1] Dublin Journal, Vol. vii., p. 401.
[2] Diseases of the Chest, p. 332.

employ the term "hollow," to designate *generically* the varieties of percussion sound which we are discussing, whether metallic, amphoric, tubular, tracheal, or drum-like. The term is used by Dr Walshe, and it certainly seems to represent a character of sound common to all these varieties. We must, however, expressly premise, that we do not thereby imply the existence of any abnormally hollow space within the thorax, for, as we shall presently have occasion to show, the amount of aëriform space within its walls, so far from being augmented, is, at least in the very great majority of cases, actually diminished whenever this sign appears.

The character of the altered percussion sound in the sub-clavicular region, as we have already said, varies much in cases of pleuritic effusion, and it is hardly necessary to add, that the nature of these variations, like every other auscultatory and percussion sign, can only be learnt at the bed-side of the patient; the organ of hearing readily appreciating delicate impressions, of which mere description can give no just idea. As the extremes of these variations in the percussion sound, which present themselves in the sub-clavicular region, when the lung is pressed upwards by pleuritic effusion, we may instance the full, clear, drum-like, exaggerated healthy pulmonary, in fact, tympanitic percussion sound (that word being used in its proper and restricted sense), and the hollow, empty, metallic bruit de pôt fêlé sound; an excellent idea of which sounds may be very readily gained, by filliping upon the cheek with the finger, when the mouth is closed and the cheek tensely inflated, and when it is open and the cheek lax; and I believe that every variety of the altered percussion sound referred to may be thus imitated, by varying the size of the opening of the mouth, and the condition of the cheek as to relaxation and tension.

Skoda ascribes to this altered sound the name of tympanitic, but he nowhere gives any verbal definition of the idea he attaches to the term, probably for the reason of the difficulty or impossibility of so doing; it is that which is obtained by percussion of the *collapsed* lungs or the *relaxed* stomach; it possesses a peculiar timbre, and varies in pitch, hollowness, emptiness, and metallic character, according to the amount of air which the collapsed lung or the relaxed stomach contains—facts of which every observer may very easily convince himself.

These tympanitic or "hollow sounds, as we have termed them, present themselves under very different degrees of distinctness, being at one time strikingly manifest, at another requiring the exercise of a practised ear for their recognition; always, however, they offer a marked distinction from the sound produced by comparative percussion of the healthy lung; and on this fact we lay particular stress, because the abnormal sound, though *per se* so slight as possibly to escape detection, becomes at once manifest when tested by comparison.[1]

[1] We would strongly advise the student to mark the variations of sound which may be obtained by percussion of the stomach, beginning with that

On the particular mode of production of these percussion sounds, whether as an attendant of pneumonia or of pleurisy we have no opinion to offer. Considerable difficulties surround its explanation: and it is perhaps not impossible that its origin is not the same in every case. But as different writers offer such very opposite accounts of the matter, it may be well perhaps to enter into a short consideration of them, and ascertain, as far as we are able, the actual pathological conditions of the organs which obtain when these abnormal sounds are elicited by percussion of the thorax.

Dr Graves and Dr Stokes have solved the difficulty, by supposing the cause of the sound, in the case of pneumonia, to be the sudden secretion of gas into the pleura over the hepatized lung. "It evidently proceeds," says Dr Stokes, "from an effusion of air by secretion into the serous cavity." Dr Walshe also refers to a case of pleurisy, in which he tells us that the presence of air was demonstrated by post-mortem examination, in the site where amphoric percussion sound had been observed a few hours before death. He does not, however, give any details of the case; but even admitting the correctness of this particular instance, few observers, we suspect, will receive the general solution of the question as given by Drs Graves and Stokes, for the presence of this amphoric sound is common, whereas the secretion of air into the pleural cavity during life appears a phenomenon of excessively rare occurrence, so rare indeed, that, from what we gather from authors on the subject, we should say that, as a fact, it still requires demonstration. Dr Stokes, indeed, quoting from one of Dr Hudson's cases, himself demonstrates the possibility of the incorrectness of his explanation,—"A remarkably muffled tympanitic percussion sound," he says, "with tracheal respiration and resonance of the voice, was heard, upon percussion, under the right clavicle; so that all who heard these phenomena expected to find a large tuberculous cavity. Dissection, however, showed the lung hard and solid throughout from solid pneumonia."

Dr Hudson fairly states, that his cases do not admit of the explanation given of the phenomenon by Dr Graves, and confesses "that he is quite unable to give one which would be considered more satisfactory. . . . . In some instances it has been accounted for by the transmission of the sound of the stomach through the solid lung. I think this explanation will apply to three of my cases, which occurred in the left side; the fourth will demand a different one, and it might perhaps be found in the facility with which the vibrations of the air in the bronchus and its larger divisions might be supposed to be communicated through a lung in that condition; *i.e.*, solid throughout, and therefore not permitting the loss in a mixed medium of solid and healthy lung, of such vibrations. This is mere conjecture, which may be readily denied and not easily confirmed. But of the fact, I am certain, that a sound, equal at times

derived from the organ when fully inflated, and following them down to that produced when its walls are very relaxed.

in clearness to that of pneumo-thorax, may be yielded by (or rather through) a solid lung under certain conditions; and since the researches of Drs Graves and Stokes, and M. Andral, have shown that absence of all sound of respiration may exist in the same circumstances, I submit that a great mistake is likely to arise from too hasty an explanation of these two phenomena, by supposing the existence of simple pneumo-thorax." The explanation given by Dr Williams, of the mode of origin of the sound, is much the same as that offered by Dr Hudson. The vesicular portions of the lung are consolidated around the large bronchial trunks, through pressure caused by the pleuritic effusion, and transmit their tubular sounds as though they had been directly percussed; but this account of the matter is very unsatisfactory, and if ever correct, is certainly only so exceptionally; for the case where the pleuritic fluid is so placed behind, as to compress the upper lobe of the lung against the anterior walls of the thorax, and to render its vesicular structure thereby solid, must be of very rare occurrence, whereas the sign itself is a common attendant of pleuritic effusion; and it certainly might be fairly asked, if this explanation were correct, how comes it that we do not oftener meet with these hollow percussion sounds in those frequent instances where the upper lobe, or great portions of it, are consolidated by tubercular and inflammatory depositions? That the transmission of the stomach sound through the consolidated lung is not the cause thereof in every case, we need hardly observe; for the hollow sound may exist in perfection beneath the *right* clavicle; and in a less degree (as tympanitic), has been observed at the base of the right lung, immediately above the liver. Moreover, when it occurs as the consequence of pleuritic effusion on the left side, several inches of dull percussion sound is generally to be noted between the line where it ceases, and the hollow tone of the stomach commences. Undoubtedly the stomach sounds are transmitted high up into the left thorax in certain cases of pneumonia, when the stomach is somewhat distended with air.

In our opinion, a most complete and satisfactory account of the condition of the lungs, which obtains where these sounds are heard, has been given by Skoda; and we cannot but express our surprise, that so little notice has been taken in this country of the very remarkable results which flow from his experiments on the subject, especially when we consider that a repetition of those experiments is in the power of every one.

For a fair elucidation of this interesting question, we must be permitted to enter here into a few short details of the statements and facts laid down or established by Skoda, affecting the subject.

His *tympanitic*—hollow we have termed it—sound is that produced by percussion of a collapsed lung or a relaxed stomach. His *non-tympanitic*, that of a healthy lung normally distended with air. These extremes comprise a distinct class of sounds, which pass gradually one into the other. Whatever cause operates in diminishing

the natural quantity of air in the lungs, tends to convert the non-tympanitic into the tympanitic sound, whether the cause act, as in pleuritic effusion, by pressure from without, or by the increase of fluid and solid constituents within the lungs, as in pneumonia. "When the lung is much reduced in volume by compression, but still contains air, its sound is invariably tympanitic." If, again, the walls of the chest be thin and yielding, the sound occasionally remains tympanitic in cases of pneumonia, even though the quantity of air in the lung be very small. On the other hand, a highly-inflated lung, or a firmly compressed or distended stomach, yields a dull *non-tympanitic* sound. From these facts results the conclusion, that "*lungs partially deprived of air yield a tympanitic, and when the quantity of air in them is increased, a non-tympanitic sound.*" Of the truth of this conclusion—never forgetting the sense in which the word tympanitic is employed—there can be no shadow of doubt, however little it be generally recognised, and however readily it may be tested.

Now, applying these facts to the conditions of the lungs, which obtain in pneumonia and in the course of pleuritic effusions, we have at once an explanation of the pathological states which give rise to these abnormal percussion sounds. A slight amount of pleuritic effusion does not seem to affect the natural subclavicular percussion sound; but whenever the fluid has collected in such quantity as to produce dulness of percussion sound in the lower parts of the thorax, its altered character becomes perceptible—we need scarcely premise that we are speaking of those cases where the lung is nowhere attached by its pleural surfaces—in the subclavicular region; the lung is pressed upwards by the fluid, and the amount of air in its upper lobe thereby diminished. The varying qualities of the abnormal percussion sound which it may render being evidently connected with the degree of pressure to which it is subjected, or to the interstitial changes within its structure, by which the natural proportion of its air is changed. When the whole of the lung is compressed by the fluid, so as to completely deprive it of air, the percussion sound becomes everywhere dull. In pneumonia a diminution of the due proportion of air likewise occurs; but it is brought about in a different manner,—viz., by inflammatory deposits within the lung itself. In this disease also the sound is doubtless also affected, by the emphysematous condition of some of the parts of the lung which often surrounds the partially condensed lung. When the lung is *completely* consolidated, its percussion sound is completely dull; *hepatized* lung therefore does not yield the particular class of sounds we are discussing, as it is generally said to do, by those authors who make reference to the subject. The proof of the fact may be most easily obtained by a few post-mortem experiments; a portion of hepatised lung,—*i.e.*, lung containing no air, yields to percussion a completely dull sound; but if the lung be highly congested, yet still contain air, its percussion note becomes very remarkable, taking on

a "hollow" nature, as amphoric, tubular, metallic, and of high pitch; it would almost seem indeed that the more the lung is congested short of its actual consolidation, the more metallic and high-pitched does its percussion sound become.

We have thought it useless to lengthen these remarks by the detail of cases, illustrative of the main points referred to; but we will here briefly allude to one very interesting illustration of the facts we are speaking of, presented by a patient lately in St Mary's Hospital, under the charge of Dr Sibson. In this man, for many consecutive days, a "cavernous resonance" (amphoric percussion sound) was recognised over the right lung, a little to the right of the middle of the sternum. On one occasion both Dr Sibson and myself observed distinctly the cracked-pot sound, on percussion, near the same spot. The post-mortem examination showed us that the right lung was gorged with blood and serum, but everywhere still retaining some portion of air. Portions of this lung, when percussed, exhibited striking examples of the metallic (hollow) sound.[1]

That the amphoric or tympanitic sound, when associated with pneumonia, may, in some rare cases, arise from the excretion or effusion of air into the pleura, has been admitted as a positive fact by Dr Stokes, and as possible by Dr Walshe, but the proof of it seems wanting. The production of gases in the pleura, from the decomposition of pleuritic fluids, in the very latter periods of life, when the vital influence is fast failing to counteract the decay of the animal compounds, is much more comprehensible, and such is probably the explanation which may be given of the case of Dr Walshe before alluded to, where amphoric percussion was observed, a few hours before death, at the inner and upper part of the chest, extending across the sternum, together with well-marked signs of copious pleuritic effusion. After death, air was found occupying the pleura there, where the abnormal percussion had been heard.

The altered percussion sound, as we have observed, is much more frequently noted in pleurisy than in pneumonia; and the reason of this is clear, when we consider the condition of the parts within the thorax requisite for its production, and the different circumstances under which these conditions present themselves in these two diseases. In pleurisy the sound manifests itself at the parts where the percussion stroke tells most forcibly, from the greater yielding and elasticity of the ribs. Pneumonia attacks, as its seat of election, the posterior part of the lungs, where fine percussion is difficult. In pleuritic effusions the sound may be generally elicited over the whole area of surface corresponding to the upper lobe of the lung anteriorly.

[1] "Tympanitic (sic) character of percussion sound remarkable over a portion of this lung, highly gorged with blood and serum, but still containing a certain amount of air."—*Writer's Note-Book.*

Another incidental fact seems illustrated by this case,—viz., that the existence of a pulmonary cavity is not necessary for the producing of a bruit de pôt fêlé.

In pneumonia it is seldom to be remarked, except around the border of the inflamed lung. In pleurisy, again, it is generally a sign of some duration, enduring like the effusion which is its cause; we have noticed it constantly present for days together. In pneumonia, it is for the most part fleeting and transitory, the lung, which is highly congested, but still contains air to-day, being to-morrow solidified, and so no longer able to produce the tympanitic sound. Still we would remark, that a practised ear will recognise its presence in pneumonia much more frequently than is generally supposed; and it may perhaps be doubted whether it is ever altogether absent in any case where the disease has attacked the lung lying immediately beneath the anterior walls of the thorax.

It is not necessary for us to enter into any detailed account of the value of this abnormal percussion sound. If observation should establish the fact, that it is almost constantly to be noted in a more or less marked degree of distinctness, in those cases of pleurisy where the lung is subjected to pressure upwards by the subjacent fluid, its semeiotic importance is at once established, and it must take rank as one of the ordinary indications of pleuritic effusions. Of great service will it also be in assisting the judgment of the physician in those obscure cases, which are perhaps less rare than is generally admitted, where there is difficulty in decyphering the meaning of a dull percussion sound observed over the lower parts of the thorax, whether indeed it represent the consequences of pleuritic or pneumonic inflammation. If the dulness depend upon pleuritic effusion, the subclavicular percussion sound should be altered from the normal, and more or less so, according to the amount of pressure to which the lung is subjected. If, upon consolidation of the lower parts of the lung from pneumonia, then will the percussion sound remain unchanged beneath the clavicle. But even if it be but an occasional accident of pleurisy, its recognition is well worthy of note, to protect the observer from error. Can any one doubt that it has been the cause of faulty diagnosis to those who were unacquainted with its real nature? Dr Hudson relates a case in which, from the hollow percussion sound obtained over the thorax, a pneumonic abscess was supposed to have burst into the pleura. The side was punctured *accordingly*, but no air escaped. Dissection afterwards showed the pleural surfaces extensively adherent, and the lung red and solid. The causes, as mentioned above, to which this abnormal sound has been attributed, sufficiently demonstrate the errors to which its untrue interpretation may lead observers.

The *cause* of the production of this abnormal percussion sound, thus presenting itself, has not been satisfactorily explained; neither has the difference existing between the sound produced by percussion of a laxly inflated, and that of a tensely inflated, stomach. It is possible, that in the case of the lungs (as above referred to), and in that of the stomach, when the abnormal sound occurs, the conditions of the parts are in one particular alike,—viz., as to the relaxation of

the tissues; that in pneumonia and in pleurisy the tissues lost their contractility, become lax, and thus resemble the known condition of the stomach, when it produces the sound. One fact, however, is certain, that the natural amount of air within the lungs is diminished when the abnormal sound presents itself beneath the clavicle in pleurisy, and over or around the inflamed portion of lung in pneumonia.

In conclusion, we would observe, that our main object in making the foregoing remarks has been to call more particular attention than has yet been generally bestowed upon it, to the fact, that an alteration of the normal percussion sound is observable in the subclavicular, and occasionally even in the mammary regions, in cases of pleurisy, where the fluid is present in such quantity as to compress the upper portions of the lung against the walls of the thorax. That the same character of altered percussion sound may be also observed, but much less frequently, in cases of pneumonia, over and around the parts inflamed. That in both cases, where the sound is produced, the pulmonary tissue contains less than its normal quantity of air; and that a correct knowledge of this sign cannot but be of considerable practical importance in the diagnosis of thoracic disease.

ARTICLE IV.—*On the Pathology of the Bronchio-Pulmonary Mucous Membrane.* By C. BLACK, M.D., Chesterfield, Bachelor of Medicine, and formerly Medical Scholar in Physiology and Comparative Anatomy in the University of London; Fellow of the Royal College of Surgeons of England, etc. etc.—(*Continued from page* 425.)

Specific Inflammation of the Bronchio-pulmonary Membrane.

THE specific inflammations, to which the above-named structure is liable, depend on the different poisons of scarlatina, rubeola, variola, erysipelas, continued fever, syphilis, and the morbific agents of gout and rheumatism. The general pathological conditions of the membrane, and consequently, the phenomena arising out of them, are so similar to those of the simple forms of inflammations, that it were unnecessary to review them in detail. It must, however, be observed of all, that they, like the simple forms of inflammation, assume the epithelial or the more severe variety of the disease—that the latter variety is sthenic or asthenic according to the particular type of the affection—and that it is also liable to assume a chronic character.

When specific inflammations of the bronchio-pulmonary membrane exist as the epithelial variety, they precede the development of the particular diseases of the nature of which they are significant, and they usually subside directly after such diseases have invaded

their accustomed locality. Thus, in many instances, at the very outset of rubeola, variola, scarlatina, erysipelas, and the cutaneous eruptions of syphilis, the pathological condition of the bronchio-pulmonary membrane is similar to that of the first stage of simple inflammation, for which it is very possible to mistake it, provided the concomitant symptoms and the history have not engaged the particular attention of the physician. But, at the time such diseases should appear on the surface of the body, the engorged condition of the bronchio-pulmonary membrane declines with the evolution of the cutaneous eruption; but it does not generally entirely subside for several days afterwards. This time is occupied by partial desquamation of the epithelium, and by a slightly increased development of mucus-corpuscles by those portions of it which have retained their vitality. Hence the constituents of the slightly increased sputa which now exist, consist of an undue proportion of epithelial patches and well developed mucus-cells.

The epithelial variety of arthritic and rheumatic inflammation of the bronchio-pulmonary membrane is always shifting in its character, and, contrary to the disposition of the other specific epithelial inflammations, it will recur again and again, but for a few hours only, during the course of the specific affections in their accustomed localities.

When the more severe form of inflammation of the bronchio-pulmonary membrane depends on the above morbific agents in the blood, the pathological condition of the membrane is similar, in every respect, to that which accompanies simple acute inflammation of that structure; but the products of the inflammation are occasionally modified by the particular poison or diathesis which influences the system at the time.

Thus the sputa discharged in variolous inflammation, invariably contain pus-cells during the period of the maturation of the pustules on the surface of the body; because, contemporaneously with the formation of the latter, pustules of a similar nature, but differing somewhat in structure, owing to the anatomical relation of the parts, are evolved in the basement structure of the bronchio-pulmonary membrane, and these, on bursting, yield their contents to the sputa, of which they form the chief constituent. Again, in erysipelatous, arthritic, and rheumatic inflammation of the membrane, I have frequently, although by no means in every case, found, in addition to the ordinary products of the more severe kind of simple inflammation, urate of ammonia; and more seldom, in the arthritic variety, the urates of soda and of lime. These salts I have as yet, when present, invariably observed as existing in a free state in the sputa, and likewise as a deposit in the mucus, but not in the exudation-cell. The reason of the immunity of the latter cell I have endeavoured to assign in my previous account of pulmonary cellulitis, to which the reader is, therefore, referred. In the sputa of rubeolous, scarlatenic, and syphilitic inflammation, as well as in that which accompanies

continued fever, I have observed no positive difference from the ordinary products of simple inflammation of the membrane. Nevertheless, in the sputa of syphilitic inflammation, I have, in addition to portions of the bronchial cartilages, which are seldom or never seen in other kinds of sputa, not unfrequently observed a peculiarity in some of the constituent cells, which may possibly hereafter prove to be a characteristic of syphilitic exudation, but which has not yet afforded me sufficient data to justify a positive declaration.

The indications of treatment, in the above specific inflammations of the bronchio-pulmonary membrane, are, in the epithelial variety,—

1. To solicit the eruption to the surface of the body, to relieve the engorged capillaries, and to subsequently excite and maintain their natural diameter by the means advised in the simple epithelial form of the disease.

2. To treat the specific disease in the general system, with a view of counteracting its injurious tendency, and of promoting its elimination from the blood, by remedies which experience has determined to be the most effectual.

In the more severe specific inflammations of the membrane, the indications are, in the main, similar to those of the simple severe inflammations, regard always being had to the second indication stated above.

How far are the different forms of Asthma dependent on Inflammation of the Bronchio-pulmonary Membrane?

Two distinct forms of asthma may be recognised—the one in which the paroxysm comes on suddenly, and is followed by an interval of perfect ease, during which there is neither the slightest difficulty of breathing, nor the least increase of the bronchio-pulmonary secretion; the other, in which the invasion of the paroxysm is more gradual, in which there is more or less straitened breathing during the interval, and in which the secretion of the membrane is somewhat greater than that of health, and is also changed, as to its physical appearance to the naked eye, as well as being microscopically and chemically different.

The history of the former kind of asthma shows it to be frequently connected with previously disordered general health, by which the nervous system is rendered morbidly sensitive, or with some local disease apart from the lungs, upon which this form of asthma supervenes. The morbid appearances are unconnected with the least trace of organic change in the bronchio-pulmonary membrane itself—the epithelium, basement structure, and areolar fibres, vessels and nerves being, so far as the microscope can determine, perfectly normal; but they (the morbid appearances) may show tumour of the vagus nerve, inter-thoracic tumour, diseased heart, or organic affection of one or other of the abdominal organs. Other cases again, falling under this category, show no organic change of any organ whatever. Such cases are invariably associated with a highly

sensitive or hysterical condition of the system, and frequently with a morbid sensibility of the bronchio-pulmonary membrane to particular odours. It is hence evident, that there is a form of asthma to which inflammation of the above membrane bears no pathological relation; and to this form, viewed in the light of the mode in which the attack is produced, the term "Nervous Asthma" is appropriate: but regarded in relation to its pathological cause, that of "Symptomatic Asthma" is the more applicable.

With this form I have nothing further to do.

There is, however, another form of the disease, which embraces by far the majority of cases, and which includes all the varieties of the true spasmodic asthma of authors.

In tracing the history of the occurrence and full establishment of such cases, it will invariably be found, that the spasmodic seizure is preceded by several marked attacks of inflammation of the bronchio-pulmonary membrane—that this inflammation is never entirely eradicated—that, even when the more marked symptoms of disease are absent, the breathing is not so free as in perfect health—that it is rendered more difficult by even moderate exercise—that there are more or less of cough and increased expectoration; and that the physical signs of the chest are indicative of a partial spasm of some of the bronchial tubes in one or both lungs, generally the latter. If, at this time, the sputum be microscopically examined, it is found to consist of the same structures as that of chronic bronchitis—namely, of exudation, mucus and pus-cells, basement patches, a few epithelial patches, bronchial casts, and portions of undeveloped exudation-plasma, together with an amorphous sediment of the sulphate and phosphate of lime. The condition, therefore, of the bronchio-pulmonary membrane, at this period, is one of chronic inflammation, which, left, as it were, to itself, as it frequently is, owing to the comparative mildness of the symptoms, is gradually progressive, as I have already shown when treating of chronic bronchitis. The existence of chronic inflammation of the membrane is not only thus demonstrated by the microscopy of the sputum, but also by the morbid appearances of that structure itself. Thus, on inspection, it is found to present rose-coloured patches throughout the bronchi, chiefly of the middle and lower lobes, which patches are owing to epithelial ulceration, as is shown by careful dissection of the part, and by subsequent microscopical examination. The same means of examination also demonstrates the existence of basement ulceration, together with a general thickening of the basement membrane throughout the affected bronchi and the cells to which they lead. This structure, instead of measuring $\frac{1}{18.000}$th of an inch in the affected bronchi, will not unfrequently be found of the diameter varying from $\frac{1}{8000}$th to $\frac{1}{12.000}$th of an inch; whilst that of the cells to which the morbid action has extended, varies from $\frac{1}{14.000}$th to $\frac{1}{18.000}$th of an inch. Besides these positive evidences of inflammatory disease in the membrane, we also find, that the fibres of

which that structure is in part composed, are hypertrophied, and that, in every case which manifested a distinctly spasmodic seizure before death, the fibres known as the bronchial muscles, have undergone a manifest increase in their diameter, and are as yet distinct in their outline. The coats of the capillary blood-vessels are likewise observed to be more or less thickened; and, in several cases, I have succeeded in tracing a degree of hypertrophy of the sentient nerves of the membrane.

Such, then, is the history of the gradual occurrence of nearly every case of true spasmodic asthma, and such are the morbid appearances of the bronchio-pulmonary membrane, and of the other bronchial tissues, with which spasmodic asthma is invariably associated, and on which it assuredly depends as its primary pathological cause. If the history of asthma is still further traced, we reach a period, at which distinct paroxysms of dyspnœa have ceased to occur. The respiratory function is now always performed within narrowed limits, which neither admit of that sudden extension, nor yet of that violent contraction which attends and characterises the remissions and invasions of spasm in the earlier stages of the disease. Still the respiration is somewhat improved under conditions which are known to be of benefit during those stages; whilst it is rendered more difficult by conditions of an opposite tendency. But the degree of respiration having been brought, as a permanent condition, below the standard of health, the bulk of the body has fallen in a corresponding ratio, and it now gives expression to the general system in a thin, cadaverous, and attenuated appearance, which attends the patient throughout the remainder of his life. Years elapse, after the full establishment of the spasmodic seizures, before the above permanently restricted limitation of the breathing supplants, as it were, intermittent paroxysms of dyspnœa, followed by ease and comparative freedom from disease; but the exact period required for such an occurrence cannot be correctly stated, inasmuch as it depends, to a very considerable extent, on the peculiarities of every individual case. It is, however, sufficient for my purpose to indicate that such an occurrence does sooner or later take place, and that, in addition to the reduction in the bulk of the body before noticed, it is accompanied by certain conditions of the sputum and of the bronchio-pulmonary membrane and other bronchial structures, to be next examined.

The sputum, to the naked eye, still presents most of the physical appearances which characterise that of an earlier chronic bronchitis; but it is not unfrequently of a more uniform consistence, less clotted in its appearance, and of a somewhat more muco-puriform colour. On close inspection, it is seen to contain the same yellowish-white, semi-flocculent, isolated portions, which, according to Lænnec, characterise chronic inflammation of the bronchio-pulmonary membrane, and which consist of exudation-plasma in different degrees of cell-growth.

This variety of sputum is seen, by the microscope, to consist of fully-formed mucus and pus-cells, the latter of which are proportionately more numerous than those of the sputa of the earlier stages of chronic bronchitis, and of exudation-cells, intermixed with a very few of the plastic kind, owing to the rapid conversion of the latter into pus-cells, after they have ceased to be living structures. Besides these, there are a few superficial basement patches, a few minute, opaque nodules of fibrine, and transparent, irregular layers of exudation-plasma, which seem to take the place of the large opaque masses of the same substance so common in the sputa of acute, and also of the earlier stages of chronic, bronchitis. But very few bronchial casts, and, as a general rule, quite as few epithelial patches, are observed in this kind of sputum.

On tracing the morbid appearances at this stage, the bronchio-pulmonary membrane is less turgid, and therefore of a less vivid colour than at the stage previously noticed. Often, indeed, the coloration is not deeper than that of health; but it is never uniform; because epithelial and basement ulceration is always present; and, at these points, the colour is more vivid than in those parts of the membrane which are yet protected by epithelium. The fibres of the membrane, together with the capillary vessels, lymphatics, and nerves, are so matted together, as to render their distinct definition a matter of some difficulty; whilst the circular and longitudinal fibres of the bronchi are hypertrophied and so amalgamated by inter-fibrous exudation, that their power of contracting is, in a great measure, abolished. Very frequently, indeed, the submucous tissue, the circular and longitudinal fibres of the bronchi are incorporated in one mass of organized exudation, in which also the outer surface of the capillary blood-vessels is imbedded—a condition which, in some measure, limits the degree of congestion to which such vessels are henceforth liable, and which can now only allow exudation to take place from that surface of them which is in direct contact with the basement membrane. This matting together of individual tissues increases the density of the bronchial walls, the rigidity of which is further increased, in the larger bronchi, by an increased thickness and hardness of the circular, and in the smaller tubes, by the same condition of the irregular, cartilages, as an invariable concomitant of such an amount of structural disease of the other bronchial tissues as has just been indicated. In addition to these results, we find more or less of bronchial occlusion, lobular collapse, and vesicular emphysema, often of the flaccid kind.

Thus, then, the characteristics of the sputa during life, and the pathological appearances after death, in every case of true spasmodic asthma, except those of the purely nervous or symptomatic kind, clearly demonstrate the existence of chronic inflammation of the bronchio-pulmonary membrane; and as this condition is recognised as invariably preceding, for a length of time, the development of the spasmodic seizure, it is evidently the pathological cause of the latter

—a fact, which will be further illustrated by considering the proximate cause of the attack, and the conditions under which the paroxysm is excited. To properly illustrate this point, it will be necessary to revert to the bronchial muscular fibre before named, and to show, by experiment, the properties with which it, as constituting a part of the healthy organism, is endowed, and from which a logical deduction is derived of its great influence in the production of the asthmatic paroxysm. These fibres, like the muscular fibres of the intestines, of the arteries, of the organic fibres of the heart, of the elongated tubular uteri of the inferior mammalian animals, of the crop of Hymenopterous, Lepidopterous, and Dipterous insects, and of similar structures in other classes of animals, are endowed with the power of contractility, which is excited by certain stimuli placed in direct contact with them, and, according to Volkmann, by galvanism applied to the vagus nerve of a decapitated animal. To ascertain how far the statements of Williams and Volkmann on this point could be substantiated by direct investigation, I repeated their inquiries in the following experiments:—

Experiment 1.—A young, healthy rabbit, six months old, was decapitated, the right vagus nerve was isolated, and the large end of a blow-pipe was introduced into the trachea, and there secured by ligature. The free extremity of this instrument was next directed towards the flame of a taper, and the poles of a two-celled electro-galvanic battery were brought into contact with the isolated nerve. Not the slightest movement in the flame was noticed. The sympathetic nerve was next isolated, and its course followed to the root of the neck; but, wishing to perform this experiment under precisely the same conditions as the previous one upon the vagus, the chest was not now opened. Having, by tracing its cervical route and connections, fully satisfied myself that I had isolated the sympathetic, the poles of the battery were applied to the communicating branch between the second and third ganglia, but no influence on the flame of the taper resulted from the application of the galvanic stimulus. A repetition of the same experiments on the left vagus and sympathetic, gave the same negative result.

The chest was now opened, the lungs were removed, and the poles of the battery applied to the large divisions of the bronchi, when the flame of the taper was, on several intermittent applications of the stimulus, thought to bear from the open extremity of the tracheal tube; but the inclination was certainly so slight, that I would not affirm it as an undoubted fact. Thus far, then, the contractility of the bronchial tubes, by direct stimulation of their muscular fibre, or by indirect stimulation through their nerves, was by no means proved.

Experiment 2.—A large, healthy, and strong dog, of the spaniel breed, was destroyed by hydrocyanic acid, immediately after which the right vagus nerve was isolated, the trachea divided, and a glass tube, tapering to a point at one extremity, was inserted in the latter,

and there secured by ligature. A lighted taper was now held, at a distance of a few lines only, from the open extremity of the tube; whilst the poles of a six-celled electro-galvanic battery, in powerful action, were applied to the isolated nerve; but no palpable movement of the flame was observed.

The sympathetic nerve was next isolated and submitted to the poles of the battery with the same result. During the application of galvanism to this nerve, rapid and snake-like movements of the œsophagus took place.

The chest was next opened; and as the lungs lay in situ, one pole of the battery was placed on the surface of the right lung, and the other at the lower part of the back of the trachea, between the extremities of the annular cartilages. The result of this was the same as that of the previous experiments. The open extremity of the tube was next directed beneath the surface of water in a vessel, and a pole of the battery was simultaneously applied to each of the lungs; but neither did the water move, in the slightest degree, within the tube, nor did the least bubble of air escape from its open extremity. The same negative result followed the application of the poles to the large bronchi before they enter the lungs. The latter organs were now removed from the thoracic cavity, and placed in a tepid solution of salt in water, after which a taper was held directly opposite to the free extremity of the tube, whilst the poles of the battery were applied to all parts of the lungs, to the large bronchi, and to the back of the trachea itself; but still the flame of the taper was not blown aside in anything like a satisfactory manner. Different portions of the lungs were next incised, and the poles of the battery were so placed, that a bronchus was included in the galvanic circuit. This experiment produced no palpable contraction of the larger bronchi; but in tubes from the third gradation in size downwards, evident contraction took place. In bronchi, from one to two lines in diameter, the contraction, as observed by the aid of a simple lens, was so great as to obliterate, for the time, their caliber, and thus to confirm the statement of Wedeymer on this point.

Experiment 3.—A large, full-grown rabbit was decapitated, after which the same proceedings as in the first experiment were adopted; but here, instead of a two-celled, a six-celled electro-galvanic battery was employed; and the taper, instead of being exposed as in the first experiment, was placed in a lantern, in order that slight currents of air might be shut out, and thus be prevented from vitiating the result. The flame was not, in the slightest degree, visibly affected on galvanizing the vagus or sympathetic nerve.

The lungs were now carefully removed, together with the trachea attached, and as they thus lay exposed, a pole of the battery was simultaneously applied to the lower lobe of each lung, and was gradually moved upwards towards the trachea; whilst the open extremity of the tracheal tube was directed beneath the surface of water at perfect rest in a shallow vessel. At intervals of about half a

minute, a single bubble of air escaped through the water from the open extremity of the tube; thus proving, beyond all doubt, the contractility of the bronchio-pulmonary fibre. When one pole of the battery was placed on the lower lobe of the lung, and thence gradually passed upwards, whilst the other pole was applied, for some short time together, to the fibrous coat, between the annular cartilages, at the very lowest point of the back-part of the trachea, the escape of single bubbles of air through the water took place at shorter intervals. In from five to ten minutes, the almost-continued application of the galvanic stimulus seemed to exhaust, for the time being, the contractility of the bronchio-pulmonary fibre, as was evidenced by the cessation of this intermittent escape of air from the tracheal tube; but after gentle artificial inflation of the lung, and after allowing these organs to collapse to the full extent of which they were capable under the pressure of the atmosphere alone, they were shown to have regained, at least to a certain extent, their power of contractility, as was manifested by the re-escape of isolated bubbles of air on the application of the poles of the battery to the tissue of the lungs, to the bronchi, and to the lowest point of the trachea. On incising the lungs, and directing the galvanic current through the smallest bronchi, these tubes were observed, by the aid of a lens, to gradually contract in this as in the previous experiment.

Experiment 4.—A full-grown rabbit was killed by a blow at the back of the head, its right vagus was isolated, the trachea divided, and a tube inserted in the latter as in the previous experiments. The open extremity of the tube was directed beneath the surface of water at rest (a mode of testing much more delicate, in my opinion, than that of the application of a taper to the open extremity of the tracheal tube), and the vagus, and subsequently the sympathetic nerve were galvanized, without the least manifestation of the escape of air from the lungs. These organs were next removed, a large bronchus was slit open, and a portion of its mucous lining was carefully dissected away. This was immersed, for a few minutes, in distilled water, at a temperature of 94° F., for the purpose of decolorising it, after which it was spread out, on a glass slide, with its lower surface uppermost, and on this surface a drop of warm distilled water was placed.

The microscope being adjusted, the poles of a one-celled electro-galvanic battery were brought into contact with the structure under observation, the fibres of which were seen to contract, and to agitate the fluid on its surface.

The result of these experiments shows that the muscular fibre of the bronchi, as also the fibrous tissue of the mucous lining, is endowed with the power of contractility—that this property is excited by stimuli directly applied to it—that it is highly questionable whether such property can be called into action by stimulation of the trunks of the bronchio-pulmonary nerves—that the mode of contrac-

tion is similar to that of the non-striated muscular fibre, commencing at the point of stimulation, and gradually propagating itself to neighbouring fibres—that the expulsion of air from the minute bronchi is, by virtue of their structure, more rapid than from the larger bronchi—that, in the latter tubes, the presence of their cartilages, the consequently limited action of their muscular fibres, and the greatly increased area of such tubes passively retard, rather than actively augment, the speed of the outward column of air set in motion by contraction of the smaller bronchi—that this negative opposition of forces is further increased in the trachea—and that, therefore, the expulsion of air from the lungs can never, under the above circumstances (*i.e.*, unaided by contraction of the expiratory muscles), take place in a forcible manner; but must, on the contrary, be a slow, continued, or intermittent oozing of air, according to the continued or intermittent action of the cause of the contraction of the bronchial and other fibres. On these grounds, I have no hesitation in stating my conviction, that some source of fallacy attended the experiment of Volkmann, which led him to assert that the galvanic stimulus, applied to the vagus nerve, produced such a sudden and forcible expulsion of air from the bronchi, that the flame of a taper was thereby extinguished.

The experiments above detailed rather lead me to the opinion, that we might in vain look through the whole range of the animal kingdom for such a result. Although my experiments tend to negative the opinion, that contraction of the bronchial fibres may be produced by galvanism applied to the vagus nerve; yet I had, during their conduction, sufficient proof that such an occurrence is readily produced by the direct application of the galvanic stimulus to the mucous lining of the tubes themselves.

Now, in order to arrive at a correct knowledge of the pathological cause of asthma, it was necessary to determine the capability of the bronchial fibre to diminish the caliber of the tubes; and having done this by direct experiment, as above stated, we have only to conceive this capability or power of contraction to be called into action, in an inordinate degree, to understand the proximate cause of the asthmatic paroxysm. I say, "in an inordinate degree," because I regard these fibres as taking part in the ordinary movements of respiration. It is evident, that the volume of the lungs, at the extreme point of inspiration, is considerably greater than at the extreme point of expiration; that this increase of volume is due to the distension of the pulmonary tissue with air; that this distension must have, as a necessary condition, a stretching or elongation of the pulmonary tissue; that, from the mode of termination of the minute bronchi, the distension of the pulmonary cells to which they lead must act upon the longitudinal fibres of these tubes; and that, from the arrangement of other pulmonary cells along the sides of such tubes, their distension must, in like manner, act upon the circular fibres. The minute bronchi, then, and consequently the muscular fibres

which they contain, undergo, during the inspiratory movement, a longitudinal and a lateral expansion, by which their power of contractility is excited, which, coming into play during the expiratory movement, enables them to regain the exact caliber which they had at the very commencement of the inspiratory act.

If, in opposition to this statement, it is urged, that the regaining of the normal caliber of the bronchi, after their expansion during the inspiratory act, is due to the property of elasticity with which their longitudinal fibres are said to be endowed, such an objection seems to be negatived by the following facts:—

1. That simply elastic bodies, by constant use, ultimately become permanently more or less lengthened; whereas the bronchial tubes of healthy lungs are neither widened nor lengthened by the incessant use of even four score years.

2. That, microscopically considered, there is no greater difference between the longitudinal and circular fibres than between the yellow and white fibrous tissues.

3. That the galvanic and other stimuli directly applied, prove that they both possess the power of contractility.

4. That striated and non-striated muscular fibres are capable of being elongated, as is proved by suspending a weight from a piece of voluntary muscle immediately after death, by the inordinate distension of the stomach by food, and of the bowels by a collection of flatus.

In reply to the first objection here stated, it may be remarked, that the molecular change, which is continually in operation in the bronchial fibres, as in all other living structures, prevents them from becoming old, in the common acceptation of that term, and therefore preserves intact their properties, and consequently their property of elasticity, simply considered as such, during the period of their ordinary functions in the organism. Such a statement is indeed true, in reference generally to the simply elastic structures; but, with respect to the bronchial fibres, it is somewhat otherwise; because, in advanced life, the number of respirations bears a greater ratio to the pulse than at an earlier date; consequently the nutrition of these fibres, which involves the idea of the molecular change above stated, is below the active ratio of the respiration; therefore such fibres do really become old, and in this condition they ought, if endowed only with the property of elasticity, to exhibit, in old lungs, the permanent elongation which constant use produces in all simply elastic bodies; but such is not the case. Hence the objection does not hold good; and as there are direct proofs of similarity of structure between the longitudinal and circular fibres of the bronchi, as these tubes have been shown to possess an inherent contractile power, and as both striated and non-striated muscle admits of elongation, we are, I conceive, justified in concluding, that whatever capability of expansion and power of contraction are manifested by the minute bronchi, are alike the conjoint properties of both the

circular and longitudinal fibres. To separate these properties in this particular instance; to assign, as some authors have done, the property of elasticity to the longitudinal fibres, and the power of contractility to the circular fibres, is opposed to the microscopic anatomy of such fibres, to their arrangement, and to all hitherto ascertained facts, relative to the physiology of organic muscle.

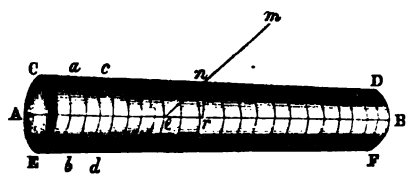
The similarity of structure and of function in these fibres, and their expansion during the inspiratory act, having been proved, it follows that the latter condition,—*i. e.*, their expansion by inspiration—becomes the exciting cause of their contraction during expiration, and that this contraction is a regular and an important auxiliary to the muscles of expiration. The cause of this contraction being the mechanical pressure of the air at the extreme point of inspiration, it is evident that the disposition to contraction exists at every point on which the pressure is made; but this disposition cannot be manifested by the minute bronchi until that of the pulmonary vesicles has been exerted; because the distension of these vesicles, in the inspiratory act, keeps in check the contractile power of the minute bronchi, which power, however, is immediately manifested after the restraining force of the cells has been removed by their contraction, and which may be said to commence at the point at which the contraction of the cells ceases, thus forming with the latter one continuous action during the expiratory movement. In the special anatomy and physiology of the bronchial tubes we thus find a wise adaptation of means to an end; inasmuch as the structure of the tubes is favourable to a considerable amount of contraction where such an effort is necessary; and to a very slight diminution of caliber where such a condition is required. Thus, in the very commencement of expiration, the expiratory muscles can exert but little influence in expelling the air from the pulmonary cells and minute bronchi, therefore considerable contractile power is required of these parts themselves; and we see, from their anatomy, that they are peculiarly adapted to such a purpose. On the contrary, as the expiratory muscles increase their force in the inverse ratio of their shortening during contraction, a less amount of contractile power is required of the bronchial tubes, which, consequently, become less and less mobile, and more and more patent, as we progress from the smallest bronchi to the trachea; in which latter, as also in its divisions, but little contractility is necessary, and to which, therefore, the means of contracting are much more sparingly dealt than to the minute bronchi, of which so much action during expiration is required.

We may thus give expression to the expiratory forces by the following formula:—

During the commencement of Expiration, . .	{ Vesicular and Bronchial Contraction, maximum. Muscular Contraction, minimum.
At the end of Expiration,	{ Bronchial Contraction, minimum. Muscular Contraction, maximum.

By the contraction of the pulmonary vesicles and minute bronchi, during each expiratory movement, the outward tendency of the natural secretion of the bronchio-pulmonary membrane is secured; but when this means fails to remove any unusually viscid secretion or other fluid, the co-operation of the sudden contraction of the expiratory muscles is excited; and, in this way, the cause of the obstruction is removed by cough, and the patency of the bronchi thereby maintained. The following diagram will illustrate the direction of the resultant of the component forces of the longitudinal and circular fibres of the minute bronchi, and of the important part which they play in the respiratory movement:—

Fig. 20.



Let A B represent the axis of a portion of the bronchial tube; C D, E F, two of its longitudinal fibres, directly opposite to each other, so that a plane passing through them would likewise pass through the axis A B; and let *a b*, *c d*, be circular fibres, and *n r* a radius of one of the circles. Now, if we suppose the expansive and contractile forces of the circular fibres to be in operation, it is evident that the circumferences of these circles, and likewise their radii, increase and decrease respectively. It is also evident, that if the forces of the circular fibres alone are in operation, motion of any point *n* takes place in the direction of the radius *n r*, because *n* is the extremity of that radius, which increases or decreases during the act of expansion or contraction. But we have likewise the expansive and contractile forces of the longitudinal fibres in operation, which tend to move the point *n* in the direction C D. Hence, by the principle of the parallelogram of forces, motion of that point takes place in the direction of a straight line, *m n e*, lying in the plane C D E F, and passing obliquely through the centre of the tube, with an angle, *m e r*, greater or less, according as the ratio of the expansive and contractile forces of the circular fibres to those of the longitudinal is greater or less.

From the above representation it is evident, that the expansion and contraction of the minute bronchi are in the diagonal of a parallelogram described on one of the longitudinal fibres, the sides of which parallelogram represent the two forces of the longitudinal and circular fibres; and we have only to conceive the maintenance of the contraction of these fibres at any and all points of their range of

action, to understand the proximate cause and the different degrees of the asthmatic paroxysm. The attack is, therefore, one of tonic spasm; and in proportion to the degree of contraction of the above fibres, so will be the urgency of the symptoms. When this power of contraction is destroyed by the confused amalgamation of the longitudinal and circular fibres by repeated exudations, the asthmatic paroxysm ceases to recur, as is almost invariably the case after the existence of the disease for a number of years; but the breathing is, nevertheless, laboured during the remainder of life, and liable to considerable aggravation on the slightest increase of the vascular condition of the bronchio-pulmonary membrane. As, in the progress of the disease to complete amalgamation of both the longitudinal and circular fibres of the affected bronchi, the fibres of one portion of a tube may be thus far advanced, whilst those of another part are as yet free, we thence conclude, that the inability of the former, and the ability of the latter, to contract, cause an unequal resistance to be offered, by the sides of such tube, to the entering column of air, during inspiration; and that, therefore, the equal pressure of the air, being unequally resisted, tends to the dilatation of those portions of the tube in which the action of the fibres is abolished.

Hence occurs saccular dilatation of the bronchi at the isolated points where the fibres of these tubes are imbedded in one mass of organised exudation; or general dilatation when the whole extent of their fibrous tissue is involved; and more particularly so, when with the above mentioned condition, the complete or partial obliteration of other bronchi causes more air to be forced into such tubes than they are accustomed to receive in the healthy action of the lungs. It is certain that, with an unequal pathological condition of a bronchus, there is a corresponding inequality of function, and hence the existence of the passiveness of one part with the active condition of another portion of the same tube. Hence also, as a dependent corollary, the transition from tubular to cavernous breathing, and from bronchophony to pectoriloquy, in the paralysed portion of the tube; whilst those parts of it, in which the fibrous tissue is yet free to act, produce various modifications of the sibilant and sonorous rhonchi, according to the degree of constriction which exists. This constriction is always more or less present even during the interval of the asthmatic paroxysm; because its seat corresponds with either epithelial or basement ulceration of the bronchial membrane, and with free, yet hypertrophied, bronchial fibre, which latter, prone to action, is directly excited by the entering column of air during each inspiratory movement. The breathing, therefore, owing to irregular and partial constriction of the bronchi, is wheezing even in the absence of an asthmatic paroxysm.

Now, the paroxysm has been shown to occur when the muscular fibres of the bronchi are free, and, therefore, able to contract; and to cease to recur when these fibres have been so amalgamated by inflammatory exudation, that they are no longer able to exercise an

individual contraction. The question then to be determined, is not the proximate cause of the paroxysm, which is proved to be an irregular contraction of the bronchial fibres; but rather, on what pathological condition does it depend, and in what way, or through what media, does the exciting cause operate. Does the contraction of these fibres depend on any influence of the nerves with which the bronchi are supplied? The experiments before detailed, fail to prove that the nerves exert the slightest influence over them; therefore, on this evidence, the direct cause of their contraction cannot exist at a remote part of the system. It must, then, have a local origin; and this statement is quite in harmony with the previously described result of the direct application of the galvanic stimulus to the bronchial fibre. Now, the local disease which precedes, accompanies, and, I may say, even outlives the recurrence of the asthmatic paroxysm, is that of chronic inflammation of the bronchio-pulmonary membrane; and as asthma is never truly established independently of bronchitis or of its results upon the bronchial tissues, we are justified in concluding, that its necessary pathological condition is, inflammation and its consequences of the bronchio-pulmonary membrane. This being admitted, the exciting cause resolves itself into any agent which is capable of irritating the bronchial fibres to contraction by direct application, or by inordinately increasing the vascular condition of the bronchial membrane. Hence a sudden reduction in the temperature of the atmosphere, and particularly so when combined with considerable humidity—the inhalation of air impregnated with irritating vapours or loaded with dust, together with the accumulation of mucus, are capable of exciting the spasmodic action of the above fibres, the contraction of which commences at those points of the bronchi, in which epithelial or basement ulceration of the membrane exists; inasmuch as these are the most sensitive parts, from which the contraction propagates itself, after the manner of that of organic muscular fibre, to the adjoining fibres, until the whole is more or less implicated. The paroxysm thus commencing, indicates its approach by minor degrees of the particular sensation to which the full development of the fit gives rise; but the duration of the premonitory symptoms depends, in a great measure, on the intensity of the exciting cause, and on the number of foci or points of epithelial or basement ulceration, from which the contraction simultaneously propagates itself. Clinical experience demonstrates the truth of this observation, and that the attack in such instances, depends on the direct action of one of the above mentioned causes. But many attacks commence during sleep, when none of the above causes are in operation; and, in such cases, the bronchial fibre is evidently excited to contraction by the vascular congestion of the bronchio-pulmonary membrane, the enlarged vessels of which exert unusual pressure on the bronchial fibres, the contraction of which may be further excited by the accumulation of mucus in the bronchi, and possibly, by the slight retention of carbonic acid

in the blood, as the result of the languid circulation and diminished respiratory movement produced by sleep. This congestion, however, may, and frequently does, take place during the waking state, when it never fails to excite more or less spasmodic action of the tubes in those in whom a confirmed paroxysm has once occurred.

From the views above detailed, it will be seen, that the pathological cause of true spasmodic asthma is inflammation and its consequences of the bronchio-pulmonary membrane, and other bronchial tissues—that the proximate cause of the attack is inordinate contraction of the bronchial fibres, which are hypertrophied—that the exciting cause must operate directly on these fibres, the mode of contraction of which is similar to that of organic muscle—that the contraction commences in the bronchial fibres, at the points of epithelial and basement ulceration of the membrane; or, when vascular congestion is the exciting cause, wherever the engorged blood-vessels produce unusual pressure upon such fibres—that such contraction cannot, in all probability, be excited by direct influence of the bronchial nerves—and that consequently, impressions, transmitted through these nerves, operate indirectly upon the bronchial fibre through the medium of the capillary blood-vessels, which are themselves influenced in either a primary or secondary manner. Hence the influence of a disordered stomach, which is so frequent a cause, and an accompaniment, of the spasmodic paroxysm, and which, in exciting it, is thought to do so by reflex action through the motor fibres of the vagus nerve; but which does not appear thus to manifest itself. On the contrary, the paroxysm, in such cases, is rather due to the upward pressure of the stomach when distended by flatus, which mechanically irritates the bronchial fibres directly, and also indirectly, by the vascular congestion of the bronchio-pulmonary membrane thereby occasioned; or the impression is conveyed from the stomach, either through the sensitive portions of the par vagum to the medulla oblongata, and thence through its motor fibres, which communicate with the sympathetic nerve, by which latter the influence is transmitted to the bronchial capillaries; or it reaches the latter destination directly through the route of the solar plexus, and its upward connection with those sympathetic fibres which are distributed to the bronchial capillaries.

The pathological cause, then, of true spasmodic asthma, being inflammation of the bronchio-pulmonary membrane, the indications of treatment, with a view of effecting a permanent benefit, are clearly those of chronic bronchitis; whilst the indications, during the paroxysm, have for their object,—

1. To relieve spasm of the bronchial fibres.

2. To withdraw the exciting cause, and to correct any condition of the system which may indirectly contribute to an attack.

The spasm is relieved by the exhibition of ether, opium and its different preparations; by lobelia inflata, camphor, conium, henbane; by extensive counter-irritation to the chest, and by the cautious in-

halation of chloroform when no cardiac disease exists. Benefit is likewise derived from the use of assafœtida and turpentine enemata; and, during the intervals of the paroxysm, from the exhibition of belladonna and stramonium, in conjunction with the remedies already indicated in the treatment of chronic bronchitis.

In withdrawing the exciting cause, regard must be had to its particular nature. If it depends on any irritating qualities of the air breathed, these must, as far as is practicable, be obviated, by removal to the more congenial air of another apartment. If, on suddenly increased vascular engorgement of the bronchio-pulmonary capillaries, derivation to the skin should be energetically solicited by counter irritation to the chest, the warm stimulating pediluvium, by warmth to the general surface, in the form of the warm water or vapour bath, and by the internal administration of the acetate of ammonia, camphor julep, coffee, and the different carminatives, followed immediately by a brisk purgative. If, on the accumulation of mucus in the bronchi, or the presence of indigested food in the stomach, an emetic of antimony, ipecacuanha, or the sulphate of zinc, must be exhibited, according to the particular features of the case,—antimony being the preferable where the patient manifests a moderate degree of constitutional vigour, and ipecacuanha or the sulphate of zinc in cases attended by marked debility.

To correct any condition of the system, which may indirectly contribute to an attack, it will be necessary to inquire into the state of the general functions, and particularly those of the digestive organs, and to treat any deflection from the standard of health according to the recognised principles established by experience.

(*End of Part First.*)

ARTICLE V.—*Reports and Observations in Surgical Practice.* By E. R. BICKERSTETH, Esq., Surgeon, Liverpool.

LARGE SUBCUTANEOUS NŒVUS IN PROCESS OF UNDERGOING A PECULIAR TRANSFORMATION.

M. E. C., a healthy little girl, æt. 18 months, was brought to me in the beginning of April last, with a tumour on the back and upper part of the left shoulder, which presented peculiar characters. It was of flattened oval form, about the size of a small fist, and had upon its summit a red mark as large as a shilling piece, possessing the ordinary appearance of a simple cutaneous nœvus. The tumour beneath was firm, minutely lobulated, and felt very like a fatty

growth that had become somewhat indurated from previous slight inflammatory action. Continued pressure had not the effect of diminishing its size in the smallest degree. It was well defined, and freely moveable upon the parts beneath. No pulsation could be felt. When viewed at a little distance, a slightly mottled bluish tinge was observed over the general surface. The skin was adherent, but could be pinched up and separated over the greater part. The parents informed me that the bright spot was first noticed a few days after the birth of the child, and was then nearly its present size, but not accompanied by any subcutaneous swelling. A few months afterwards, a slight thickening was discovered immediately beneath, and this had been increasing ever since—latterly more rapidly than at first.

They were desirous that the tumour should be removed; and, as it had already gained a large size for so young a child, I urged the operation without delay. I believed the tumour to be a fatty growth, and intended to excise it, but the small superficial nœvus, and the bluish cast over its surface, made me more doubtful of my diagnosis than I otherwise should have been. Lest it should prove more vascular than was supposed, I readily acceded to the proposal of my friend, Mr Hakes, that, as a precautionary measure, it would be prudent to place ligatures under the base of the tumour before commencing the operation of removal.

On the 6th of April, the child being fully under the influence of chloroform, the base of the tumour was transfixed, in both its long and short axis, by needles armed with a double ligature of strong hemp thread; and these being drawn through the ends, were cut, and left in readiness to be tied, if the hemorrhage should be found uncontrollable. A crucial incision was then made over the surface of the tumour, so as to unite the opposite points of exit of the threads, and in doing so care was taken to exclude the diseased portion of skin. The four flaps thus formed were then separated, and dissected downwards as far as the subjacent muscle. Dense adhesions rendered this part of the proceedings more tedious than was desirable; and although every care was taken not to cut into the morbid structure, a most profuse hemorrhage took place. Large vessels poured out blood with every stroke of the bistoury, and the child became blanched, and almost pulseless. While the flaps were held back, the ligatures were tied as tightly and as deeply as possible. The bleeding ceased at once, and the tumour appeared completely strangulated. An incision through the upper part of the growth displayed its structure. It consisted of blood-vessels of large size, imbedded in a dense fibro-cellular tissue, which surrounded and bound together the vascular formation. A portion was cut off for more minute examination, and the wound was dressed with wet lint and gutta percha tissue. The tumour separated on the fifth day, and the flaps brought together by straps of adhesive plaster had advanced considerably towards a perfect cicatrization, when, from a most im-

proper exposure on a cold and wet day, rigor and fever, with symptoms of pulmonary congestion, set in, and the child died exhausted twelve days after.

The examination of the small piece that was removed proved very interesting and important. It illustrated a remarkable pathological change of structure, to which subcutaneous nœvus, I believe, is occasionally liable,—*i. e.*, the development of cysts from partial or limited obliteration of the dilated capillaries. On making sections in different directions, and also by dissection, several complete cavities, containing blood in a fluid state, more or less decolorised, were observed. The greater number were little larger than a hemp seed, but some were the size of a large pea, and in these larger ones the contents partook much more of the character of coloured serum than of blood. They were perfectly round, and were formed by a very thin cyst-wall, lined with smooth shining serous membrane. The dilated vessels, which composed much the greater part of the tumour, admitted for the most part an ordinary probe, some were even larger. Their walls were thin, and they collapsed when cut across. It was impossible to ascertain by dissection any positive connection between this vascular network and the cysts; but that such was in reality the case, can hardly, I think, be doubted. The fibro-cellular structure, which gave firmness and consistence to the tumour, appeared to be the result of the organisation of plastic effusion from an originally simple subcutaneous nœvus—the consequence probably of chronic inflammatory action of the part. The continued exudation and organisation of plastic material would limit extension of the original disease, check the further dilitation of the aneurismal vessels, and at length, by encroaching gradually on their external walls, would contract, or altogether cause, the obliteration of their cavities. It is in this way that a spontaneous cure of nœvus is sometimes effected. A slight thickening or induration remains for a time in the site of the previous disease, but soon it also is removed. The condensed cellular tissue composing it has accomplished its purpose, and is now no longer required.

But suppose the cure to be imperfect, and that, instead of a complete obliteration of the sinuses and dilated vessels, limited portions remain unaffected, and are gradually shut out from the current of circulation,—they are then no longer capable of transmitting their contents, but form simple closed cavities, containing blood in a passive condition; disintegration and removal of the colouring matter is slowly effected; secretion from the internal walls is established; and consequent dilatation of the newly formed cysts completes the metamorphosis.

A similar alteration of structure was recently described by Mr Holmes Coote to take place in "venous tumours;"[1] an affection very distinct from nœvus maternus. The process by which the

[1] Medical Gazette, vol. x. p. 412.

transformation is accomplished appears to be the same in both cases, the difference merely existing in the nature of the structure affected.

And in the same way may be explained, I think, some of those peculiar subcutaneous, fibro-cellular, cystic tumours, which occupy a situation previously the seat of nœvus. Growths of this nature are not very uncommon. I have removed two such within the last eight months. Both of them I had supposed to be fatty tumours, and did not discover my error except by dissection; when, upon entering carefully into the history of their formation, I found that in infancy "mother marks" had occupied the same situations. In one of them, a girl eighteen years of age, upon whom I operated in December last, the tumour was situated over the deltoid muscle, and was so diffuse and flattened as scarcely to form any prominence. It moved freely upon the muscle, but could not be separated from the skin, which felt indurated and thickened over its surface, but did not possess any unusual vascularity. The tumour had existed since birth; but during the last year had increased considerably, and had given rise to shooting pains, extending down the arm. The nœvus mark had disappeared in early infancy. Very little bleeding accompanied the removal; but the operation was rendered troublesome from the tumour not possessing any distinct investing capsule, so that it was necessary to cut wide of it in every direction in order to secure its complete extirpation. It was of circular form, two inches in diameter, by about three-fourths of an inch in thickness; and was composed of a congeries of delicate cysts, bound together by tough fibro-cellular tissue. Some were as large as field beans, while others were not more than half that size. They contained thin transparent fluid. In some instances two or more contiguous cysts communicated, pressure appearing to have caused absorption of the adjacent walls. Some minute portions of fat were also observed among the uniting fibro-cellular structure.

In the other case, a gentleman about thirty years of age, the tumour smaller and more prominent, was on the right side of the chest, and, according to his account, was gradually getting larger. The skin over its centre was very vascular, though not sufficiently so to entitle it to the term of nœvus. The history of its formation, and the appearances on dissection, were exactly similar to those observed in the last case.

These tumours differ widely from the ordinary fibro-cystic growths. They do not possess the same distinct investing capsule, but appear rather to be continuous with the subcutaneous cellular tissue. The cysts are more delicate, their walls thinner, and their form less regular; and the uniting fibro-cellular structure, instead of possessing that freshness and vitality essentially characteristic of a new growth or tumour, properly so called, resembles rather ill developed adhesions resulting from the organisation of accidentally effused lymph. It will be observed that in each instance the tumour was said to have enlarged up to the time of operation; but if the explanation of their

origin be as I have stated, it is difficult to conceive how, the transformation once completed, any actual increase could take place, except by the individual dilatation of the existing cysts. No new ones could arise, for the original being themselves of accidental formation, would not possess the power of multiplying common to the true cystic tumours.

Exostosis from the Head of the Humerus; Accidental Fracture of its Pedicle; Excision of the Tumour; Recovery.

Henry V——, æt. 16, applied to me in December last, for advice about a tumour in front of the right shoulder, which he had discovered only four days before. He was lifting up a heavy bar of iron, and was in the act of fitting it into a window-sash, in order to secure the shutters of his master's shop, when suddenly he felt a severe pain in the shoulder and down the arm. The limb dropped powerless by his side, and he nearly fainted. On putting his hand to the part, he found a hard lump, which was painful, and very tender to the touch. Before this period he was not aware of any swelling or defect of any sort. One arm was as strong as the other; and he does not recollect ever to have suffered pains, or to have received an injury to the shoulder. Since the accident, the pain and helpless condition of the arm have continued.

On examination of the part, a slight prominence is observed on the inner and anterior aspect of the articulation. It is formed by a deep-seated tumour of bony hardness, and is most prominent about half an inch below, and to the outer side of the coracoid process. A few of the anterior fibres of the deltoid can be distinctly traced, passing over the external portion of the tumour, and the remainder of it appears to project in the interval between that muscle and the pectoralis-major. The soft parts move freely over its surface, which feels lobulated, and of extreme hardness. On grasping the body of the tumour, it can be felt passing deeply downwards towards the head of the humerus. A slight degree of motion is permitted to its superficial portion; but at its origin, which is evidently narrow and very deep, no movement can be ascertained with certainty. Once when endeavouring to discover its attachment, I felt a distinct crepitus, but by repeating the same manipulations, I could never again detect a similar feeling. The movements of the shoulder-joint were quite free, but pain prevented him raising the arm from his side, or from making any use of the articulation. There was no fracture of the neck or head of the humerus, which was plainly felt, occupying its natural situation. The movements of the tumour corresponded exactly with those of the humerus, both during flexion and rotation.

From the examination there could be no doubt that the tumour was an exostosis, arising from the immediate neighbourhood of the head of the humerus; but whether fixed or broken off, it was impossible to

ascertain. The latter appeared most probable, from the history and from the crepitation that I had felt upon one occasion; but, on the other hand, my failure to experience a second time the same sensation, and the remarkable precision with which the tumour followed all the movements of the humerus, rendered the diagnosis of this point very uncertain. However, as it was agreed in consultation to remove the tumour, I was prepared with strong bone pliers, should their assistance be found necessary.

December 15th.—Chloroform was fully given; but even with this aid to diagnosis, a careful examination failed to decide the point at issue. Having made an incision about four inches in length, parallel with the anterior fibres of the deltoid, and cut through a thin layer of muscular tissue, the fibrous capsule which enveloped the tumour, was immediately exposed. When this was freely divided, the two halves of the sac were turned aside; and as no adhesions had formed, the fingers were passed without difficulty round the body of the tumour. Then by lifting it up, I found it already detached. A little dissection from the dense fibrous tissue, which adhered closely round its neck, was all that was required to turn the tumour fairly out from its bed. Scarcely any bleeding occurred, and that little was checked by the application of cold. On putting the finger into the cavity, a narrow elongated fractured surface was readily felt. It was on a level with the general surface, and was very close to the head of the humerus. The rough portion had not the sharpness of newly fractured bone, but felt as if covered with thick lymph. A few sutures were introduced, and the water-dressing placed over the wound.

On the 29th of December the wound had closed completely, and he could use the arm perfectly.

On examination of the tumour, of which fig. 1 is an accurate representation of the natural size, it was impossible to overlook its

Fig. 1.

remarkable resemblance to the head of the humerus. It was of

similar shape, and possessed all the essential structures of an articular extremity, developed in a perfect but less regular form. It consisted of an elongated neck or pedicle, composed of compact bone, and expanded into a globular-shaped body or head, which was crested with a thin layer of glistening semitransparent cartilage. To complete the resemblance, the periosteum, which was firmly attached round the neck of the tumour, and constituted its capsule by enveloping the entire body, became reflected near the margin of the cartilaginous crest, and lined by a smooth serous membrane. In this way a shut sac, having all the characters of a synovial capsule, was formed for the pseudo-articulating extremity.

On making a section (fig. 2), the body of the tumour was seen to be composed of cancellated bone, and separated from the super-

Fig. 2

imposed cartilage by a thin and irregular stratum of compact structure.

Exostosis near the head of the humerus is a very rare affection. On the lower end of the femur, and on the head of the tibia, it is comparatively common. At whatever situation it arises, the characters I have described are commonly observed; but I am not aware that the synovial capsule, which invests the cartilaginous extremity, has attracted that share of attention which, considered either pathologically or physiologically, it manifestly deserves. After an exostosis has gained an inconvenient size, it is not unfrequently broken off by an accident—as a fall or blow. Its further increase is thus stopped, and the fractured extremities becoming rounded off, are reunited by a species of false joint. I remember to have seen two cases where a complete socket had formed at the lower end of the femur, in which the rounded end of the pedicle was received, and revolved as in a joint. The same would probably have occurred in the foregoing case had a longer time been allowed to pass.

ARTICLE VI.—*Observations on the Fevers of Jamaica.* By J. MAGRATH, Senior Medical Officer of the Public Hospital, Kingston.

DURING a residence of thirty-five years in this island, I have never met with a single circumstance that for a moment induced me to believe that the fevers which occasionally prevail here epidemically, or arise sporadically, are ever of a contagious nature.

The intermittent, remittent, and yellow fevers appear to me to be modifications of the same disease, influenced by particular circumstances, and to arise from the same cause,—viz., a deleterious principle (generated during the decomposition of certain substances, principally vegetable), the production of which is materially influenced by the degree of heat and moisture present, and by the existing state of the atmospheric and telluric electricity.

In 1819, during the prevalence of the fatal epidemic of yellow fever among the troops at Up Park Camp, it repeatedly happened that, on going to the hospital, I passed over spots of small diameter, from whence a hot moist air seemed to issue. Upon afterwards examining these situations, I could not, however, discover any cause to account for the circumstance.

I have known patients to be admitted into hospital who, after having had two distinct febrile paroxysms, have lapsed into yellow fever, and died with black vomit. And again, I have known others who were admitted with the muddy injected eye, and drunken suffused appearance of the countenance, deep-seated pain in the orbits, and other pathognomonic symptoms of yellow fever, lasting for two or three days, have afterwards two or more paroxysms before they became convalescent. Cases of this kind are rare, but still sufficient, I think, to establish the connection of yellow with intermittent fever. Sir William Pym attempts to distinguish what he calls Bulam from aggravated miasmatic fever; but every symptom he describes as diagnostic of the one I have repeatedly seen in the other.

Yellow fever has not for several years prevailed as an epidemic in Kingston; but every now and then we receive patients suffering from it into the public hospital, and the most of these cases come from coal ships.

In 1848, the crew of a vessel, that had a few months previously been employed in carrying guano, suffered most severely; and in 1849, the persons on board three coal ships, which, after discharging their cargo, took in some impure ballast, were attacked with this disease in its most virulent form, whilst the other vessels in the harbour were comparatively but little affected. The sick from these ships were placed amongst the other patients in the hospital; but the disease did not spread to them.

Unacclimated individuals of every race are liable to attacks of yellow fever, although I believe the negro will often have remittent fever when the white man would be subject to the more aggravated

form. I have known persons of colour, natives of the mountain districts, and who had never been off the island, to be attacked on coming to Kingston, and die on the third or fourth day, with black vomit, hæmorrhage, etc.

Yellow fever never attacks the thoroughly acclimated, although persons who have had it once are not invariably exempt from a return of it. I have myself attended more than one patient in two well-marked attacks of the disease; and in 1827, shortly after I engaged in practice in Kingston, I was called to see a gentleman, who died on the third day of fever, and who, I was assured by Dr Adolphus, deputy-inspector of hospitals, and by his family, had, about six months previously, a similar attack with well-marked black vomit. Persons have been known to die of remittent fever after a residence of upwards of twenty years in the island.

We have lately been receiving into the public hospital several persons in the advanced stage of remittent fever, or suffering from its sequelæ, who contracted the disease at Chagres. In them the spleen and liver are always congested, and the lungs occasionally so. The liver and lungs are occasionally disorganised. The greater number are dropsical, and suffering from subacute inflammation of the alimentary canal, accompanied by diarrhœa; in several a marked change is observed to have taken place in the blood.

I have seen a few instances of the algide form of fever mentioned by Dr Barclay, and believe that when it occurs a paroxysm has taken place, which from some cause does not proceed beyond the cold stage, and in which the patient dies or gradually recovers. In 1823, I was called to see a lady in St Elizabeths, who had a severe paroxysm of fever, and whom I found in the hot stage. At daylight next morning she was freer from fever, and feeling comfortable. At nine o'clock A.M. she complained of being weak, and as I was in the house I saw her immediately, and found her in a state of collapse, which gradually increased until she became deadly cold, like a person in the last stage of cholera; in this state she remained for upwards of thirty-six hours, when, by the continued use of stimulants, and keeping her wrapped in blankets, she gradually emerged from it, and recovered without any return of fever. Quinine was not in use in this island at that time.

As these different forms of fever arise from the same cause, their treatment is based on similar principles, modified by the intensity of the disease. The indications appear to be, "to relieve such viscera as become the seat of congestion, or are threatened with it; to overcome the fever, and to effect a counterchange in the state of the blood."

General blood-letting in young and otherwise healthy subjects is frequently most useful in many epidemics, when the patient is seen within the first few hours of the attack; but the indiscriminate practice of it has done a great deal of mischief, particularly when it

has been had recourse to after the first day. Topical bleeding is a most valuable remedy, and can be employed at a later period than general blood-letting. Where there is tenderness of the epigastrium, with vomiting and ardent fever, it should rarely be omitted; sinapisms and blisters are also often useful.

The American practice of giving large doses of calomel and quinine is, I think, an excellent one. It is that which we have for the last few years followed in the public hospital, and which I adopt in private practice. The first dose of calomel (a scruple) will often of itself allay the irritation of the stomach, and enable it to retain the scruple of quinine which is given immediately afterwards. The quinine is then given in ten-grain doses, and the calomel in four-grain doses, every third or fourth hour, until an impression is made on the disease, or the quinine is found to be affecting the system, when the dose of it is diminished. Sometimes only the first dose of calomel is found necessary, in other cases several are required. It generally acts on the bowels, and supersedes the necessity of giving those large doses of purgative medicines that were formally deemed indispensable.

When it is evident that a change is taking place in the state of the blood, the saline medicines recommended by Dr Stevens are administered, in conjunction with or without quinine, as may be found advisable; and a perseverance in this treatment has saved the lives of many, who without it would have fallen victims to the disease. When the powers begin to fail, stimulants (particularly brandy) are given in moderate quantities.

Kreosote, hydrocyanic acid, and nitrate of silver have at times been found useful in arresting the vomiting; but in general the oxide of bismuth is found the most effectual remedy.

Black vomit was at one time believed to consist of vitiated bile; but it is now known to be disorganised blood, mixed with the secretions of the stomach. The bile, found in the gall-bladder of patients who die with this symptom, mixes readily with water; whilst a mixture of black vomit and water is turbid, and always deposits a sediment.

The yellow colour of the skin, in most instances, arises from the diffusion of bile in the system, and is then first noticed in the eye. In other cases it has its origin in the decomposition of the blood, and in these is first observed in streaks about the neck. When arising from the latter cause, it indicates a most formidable disease.

When an epidemic breaks out, the removal of the people (when it is practicable) should be one of the first steps taken. The good effects arising from this measure were strongly exemplified in the 60th regiment, a few years ago. It was suffering most severely from fever at Fort Augusta; but, on being removed to the mountains, soon got rid of the disease. Had they remained at the Fort their loss would, I have little doubt, have been enormous.

Kingston, Jamaica, 1852.

ARTICLE VII.—*Analysis of the Fluid from a Cyst in the Thyroid Body.* By DOUGLAS MACLAGAN, M.D., F.R.S.E.

I WAS consulted about eighteen months ago by a young lady, for a swelling on the front of the neck. It was evidently situated in the thyroid body, and confined to the left side of that organ. In shape and bulk it resembled a moderate sized pear; it was free from all pain or tenderness, and had attracted attention only by its increase in volume. It felt firmer than an ordinary goitre, and both from this circumstance, and from its being confined to one lobe of the thyroid body, I stated my suspicion that it was a cyst; but as this could not be positively affirmed, I ordered it to be counter-irritated, by painting it with tincture of iodine, and prescribed a solution of iodide of potassium. Under this treatment the tumour diminished in size, and at the end of a month had become so small as not to be readily discernible by the eye, although by the fingers it could be felt not to have disappeared entirely. Gradually, however, the tumour reappeared, and again obtained nearly its former magnitude; but a repetition of the treatment by iodine, externally and internally, again reduced it. As it gave the patient little inconvenience, and as the deformity, from her not being thin, was not very great, I did not see it again till a short time ago, when my attention was directed to it, during my attendance on another member of the family. I now learned that the tumour had several times been reduced in size, by repetition of the iodine, though never entirely removed, and this had on one occasion occurred spontaneously, when she was residing on the west coast, and using no iodine. It was now as large as it had ever been.

At my request Professor Syme saw the patient. He agreed with me in regarding it as a cyst in the thyroid gland—in advising that it should be punctured with a trochar, and if it proved to be of this nature, that it should be injected with tincture of iodine. This was accordingly done; rather more than two ounces of fluid were drawn off, about three drams of tincture of iodine injected and left in the cyst.

The result has been most satisfactory. The patient suffered little or none from the operation, only to a small extent for two days from the inflammatory action excited by the injection, and in the course of a week was able to go about as usual. The tumour has entirely disappeared.

My present object is to state the results of the analysis of the fluid which was drawn from the cyst, but at the same time I gladly avail myself of this opportunity of recording an additional testimony to the simplicity and efficacy of the iodine injection, as the remedy for "hydrocele of the neck," as well as hydrocele of the tunica vaginalis.

When Mr Syme withdrew his trochar, after puncturing the cyst, and the fluid flowed from the canula into a clean tea cup, we simul-

taneously expressed our surprise at the total want of colour, and extreme clearness of the fluid; and as neither of us had ever seen such a liquid removed by tapping, from any part of the body, I subjected it to analysis.

It was clear and colourless, and when drawn had the appearance of the purest spring water, but a small quantity of it which was left in the bottle for two days, became slightly turbid at the end of that time. Its specific gravity was 1009·47. It deposited a small quantity of albumen when boiled. Microscopic examination revealed nothing but one or two transparent epithelial scales of an irregularly triangular form, and without visible nuclei or granules.

500 grains of the fluid, evaporated and dried, left of total solid residue 5.28 grains.

The solid residue, which was white, with a very faint yellowish tinge, was agitated with successive portions of ether, and this, on evaporation, left 0·22 of fatty matter, which, contrary to my expectation, did not show any crystals of cholesterine, but was a liquid fat. The residuum, on being now treated with cold water, separated a quantity of coagulated albumen, weighing 0·71; and this when incinerated, left of phosphate of lime 0·13; the real albumen therefore was 0·58.

The watery solution, separated from the coagulated albumen, was evaporated. It left an abundant crystalline residue, mostly in masses of irregular cubes, and obviously was chloride of sodium, tinged yellow by adherent animal matter. But on the side of the small beaker glass, where the evaporation had left a thin crystalline layer, the crystals had the appearance of the crosslets and daggers, so often seen when chloride of sodium crystallizes from a fluid containing urea. The crystalline mass was treated with boiling absolute alcohol, which, when decanted off and evaporated, left a translucent solid residue, in which a few minute obscurely prismatic crystals could be observed. I endeavoured to determine whether or not this was urea, by dissolving it in a few drops of distilled water, and adding an excess of nitric acid. The water, however, took up only a small portion of the alcoholic residue, separating the greater part of it in opaque flakes, and the watery solution did not yield any crystals of nitrate of urea. I made other trials with reagents, with a view to determine the real nature of the matter dissolved by the alcohol, but the results obtained do not enable me to put a specific name upon it. Hot nitric acid acted upon the opaque flakes as it does upon the fats. I am, therefore, inclined to regard it as a solid fat, which the albumen and other matters had protected from the solvent action of the ether previously used. The crystalline appearance may have been due to a trace of urea. It did not look like chloride of sodium, as the quantity was too minute to enable me to determine this positively. The total dissolved by the alcohol weighed 0·17.

To determine the nature of the residue left undissolved by the alcohol, it was taken up with water, and divided into two equal por-

tions. One of them was evaporated to dryness, and incinerated, when it left a perfectly white saline ash, weighing 1·69. This, when deducted from the weight of the residuum remaining from the former treatment, gave, of soluble animal matter destroyed by the ignition, 0·37, or, in the 500 grains of fluid analysed, 0·74.

In this saline ash I found chlorine in large quantity, sulphuric acid to a very small amount, lime and magnesia of each a trace. There was no iodine (the patient had used none for a length of time). Phosphoric acid was tested for, but could not be detected.

The other half of the solution of the saline residue was used to determine the alkalies. For this purpose it was mixed with baryta water, and heated. No ammonia was evolved, so that there were no ammoniacal salts present. It was then filtered, precipitated with carbonate of ammonia, refiltered, evaporated to dryness, and ignited. The residuum gave, of alkaline chlorides, 1·53,—and from this, by the ordinary proceeding with chloride of platinum, I obtained 0·28 of platino-chloride of potassium, which is equivalent to 0·085 of chloride of potassium. In the 500 grains of fluid, therefore, there were of chloride of sodium 2·90, chloride of potassium 0·16, and of other saline matters, including sulphuric acid, lime, and magnesia, 0·32.

Calculated for 1000 parts, therefore, the above analysis gives the following results:—

Water,	989·44
Total solids,	10·56
Albumen,	1·16
Liquid fat,	0·22
Solid fat, with urea (?),	0·34
Soluble animal matter,	1·48
Phosphate of lime,	0·26
Chloride of sodium,	5·80
Chloride of potassium,	0·32
Sulphuric acid, lime, magnesia, etc.,	0·64
	10·22
Loss,	0·34

This fluid differs from most of those, the analyses of which are recorded, by the absence of colouring matter, and by the small amount of solids which it contains. The fluid, removed from ordinary hydrocele of the *tunica vaginalis*, contains from 6 to 17 per cent. of solid matter, of which from 4 to 6 is albumen, whilst here the total solids were almost exactly 1 per cent., and the albumen little more than 1 per thousand. It is evident, from the variations in size of this tumour from time to time, that absorption and reaccumulation of fluid took place in it much more readily than is commonly observed in such cysts, and this may have been connected with the tenuity of the fluid which it contained.

28, Heriot Row, Edinburgh,
May 1853.

ARTICLE VIII.—*On Excision of the Knee-Joint.* By R. J. MACKENZIE, F.R.C.S.Ed., etc.

HITHERTO an impression most unfavourable to the performance of the operation of excision of the knee-joint appears to have been entertained generally by the profession; and severe censures have been passed on those who have, within the last three years, revived the operation, as well by those who practically are quite ignorant on the subject, as by those whose experience and professional standing entitles their opinion to attentive consideration.

My own experience on the subject has as yet been too limited to warrant my speaking with any degree of confidence as to the result of the operation. I have performed the operation in two cases, but although the results of these cases hitherto have fully realised my expectations, yet the number is too small, and the time which has elapsed since the operation was performed is too short, to allow much weight to be attached to my personal knowledge of the operation. I have been induced, however, to write the following short notice of the subject by the conviction, from what I have lately seen of the results of the operation in several cases where it has been less recently performed, that the operation is one which has been rejected from surgical practice on insufficient grounds, and that it will yet come to supersede, in many cases, the severe operation of amputation of the thigh.

In order to enable us to arrive at a rather more definite conclusion than hitherto, as to the propriety of the operation, I propose to give a short account of the few cases in which, as far as I know, the operation has been performed.

The operation was first brought prominently before the profession by Mr Park of Liverpool, in 1783. It had been previously performed successfully by Mr Filkin of Northwich, but no satisfactory record of Mr Filkin's case exists. Mr Park, in speaking of the result of his first case, says that a year after the operation his patient "was able to walk with great ease and firmness, without even the assistance of a stick, or of any kind of splint to support the union," "the foot being three quarters of an inch shorter than the other one." Seven years later Mr Park gives this additional account of the same case:—"To the history of the case of Hector M'Caghen, I have now to add, that he afterwards made several voyages to sea, in which he was able to go aloft with considerable agility, and to perform all the duties of a seaman; that he was twice shipwrecked, and suffered great hardships, without feeling any further complaint in that limb; but was at last unfortunately drowned by the upsetting of a flat in the river Mersey." Mr Park repeated the operation, but his second case proved less fortunate, the patient, a very unfavourable subject for the operation, having died about four months after its performance.

Moreau, some time afterwards performed the operation in three cases. Of his first case he says:—"At the end of the third month the consolidation of the bones was such that I left the limb at liberty in bed; the patient moved it about at his pleasure; in short, I flattered myself that I should be able to make him walk upon crutches in a month or six weeks; but an event, with which my operation had nothing to do, deprived me of that satisfaction. Epidemic dysentery of a fatal kind appeared in the hospital, by which the patient was attacked, and from which he sank three months and a half after the operation. "This unfortunate accident," says Moreau, "deprived me of the pleasure of enjoying the fruits of my care; but I remain convinced of the utility of the operation, and persuaded of the propriety and necessity of performing it in similar cases. I looked on my patient as cured, for I had no relapse to dread." In Moreau's second case the operation proved fatal. In a third case by the younger Moreau, the result was more fortunate, the patient recovering with a serviceable limb.

In 1809, Mülder excised the knee-joint of a pregnant female. Two months after the operation she was delivered of twins, and some time afterwards died from tetanus.

The next notice of the operation is by Mr Crampton, in 1823. The operation in Mr Crampton's first case can scarcely be said to bear on the question of excision of the knee-joint; not only were the patella and part of the tibia removed, but upwards of six inches of the thigh bone were taken away. The patient, however, lived upwards of three years after the operation, and then died from phthisis. Mr Crampton, in his remarks on the case, says, that it "was one to which the operation of excision was not applicable." The disease had proceeded too far; for, even had it been possible to have removed the whole of the diseased bone, and that union had taken place between the femur and tibia, the limb, from its shortness, would have been useless. The operation was performed in the same year in a second case by Mr Crampton. The girl, although a most unmanageable patient, made an excellent recovery. Six months after the operation, the femur and tibia were consolidated by a firm bony union. Eight months later, she is reported as being able to walk a distance of nearly five miles at once; and upwards of three years after the operation, Mr Crampton says:—"I have examined the limb, and find that the femur and tibia are firmly consolidated; the leg and thigh are not in the slightest degree wasted, but the limb is considerably bowed outwards; she wears a shoe with a cork sole four inches thick; and, to use her own expression, 'is able to stand or walk the length of a day.'"[1]

[1] I quote this from a work published eight years after the performance of the operation (Syme on "Excision of Diseased Joints," 1831). I understand, however, that the bones in this case yielded so much afterwards, that ultimately they were anchylosed at a right angle, a result which ordinary precautions, on the part of either patient or surgeon, might certainly have averted.

The operation was next performed by Mr Syme, who, in 1829 and 1830, excised the knee-joints of two children, of seven and eight years of age. Up to this time the operation had only been performed in adults. The result in the second case was unsuccessful, the child having died about ten days after the performance of the operation. Writing in 1831, Mr Syme gives the following satisfactory account of his first case:—"In the course of four weeks after the operation, the wound was all but healed, and the limb, before the expiration of three months, had regained so much strength, that the patient could make some use of it in walking. It has been progressively improving since, and is still doing well. I have no doubt that ultimately it will be nearly as useful to him as ever; but even at present he would be very sorry to exchange it for a wooden one. He can walk and run, though with a halt, without the constrained appearance of a person with an artificial leg, and merely requires the *heel* of the shoe to be two inches higher than the other. The limb is stout and well nourished, and though slightly bowed outwards, does not occasion any disagreeable deformity; it allows a slight degree of flexion and extension." Seventeen years later, however, a much less satisfactory account of the same patient is given. In speaking of excision of the knee-joint, in 1848, Mr Syme says:—"I tried the operation nearly twenty years ago on a boy, who recovered perfectly from it, and seemed at first to possess a limb little inferior to its fellow, except in so far as it was stiff at the knee. But in the course of time it was found that the growth of the two limbs was not equal, and that the one which had been the subject of operation, gradually diminished in respective length, until it wanted several inches of reaching the ground, when the patient stood erect."

After Mr Syme's experience in 1830, the operation seems, as far as I can ascertain, to have been abandoned till within the last three years. The results of the operation on the whole had been decidedly unsatisfactory. Of eleven cases in which it had been performed, six had died, and five had recovered. Of the fatal cases, however, some at least may be said to bear little on the merits of the question of excision, Mülder's case for instance, where the patient was far advanced in pregnancy when the operation was performed, and died of tetanus after parturition; and Mr Crampton's case, in which, although between six and seven inches of the thigh bone were removed, and the disease not yet eradicated, the patient lived for more than three years, and ultimately died from tubercular phthisis. The *epidemic* dysentery, however, which carried off Moreau's first case, cannot, I think, be altogether looked upon as an accidental occurrence, diarrhœa and gastro-enteritis being, as is well-known, a common cause of death after all severe operations. The result in the remaining three fatal cases was attributable in each case to the direct effects of the operation. Of the five cases which had recovered, the limb appears to have been thoroughly serviceable in three; the

amount of usefulness of the limb in the fourth is unknown, and in the fifth case (Mr Syme's) the limb was ultimately useless.

In 1850, the operation was revived by Mr Fergusson; and within the last three years the operation has, as far as I can ascertain, been performed thirteen times: in three cases by Mr Fergusson, in six cases by Mr Jones of the Island of Jersey, in one case by Mr Page of Carlisle, in one case by Dr H. Stewart of Belfast, and in two cases by myself. Of the results of these cases I shall give some details immediately. Of the thirteen, three have died, two directly from the effects of the operation, and one from dysentery. The remaining ten are, as far as I know, at present alive. In five the limb is already used freely in progression; in one the result, as regards the usefulness of the limb, is yet uncertain; in one, I am ignorant as to the result; in the remaining three, sufficient time has not yet elapsed to render the result certain, but of each the most favourable expectations may be entertained.

Such is a brief outline of the history of all the cases in which, as far as I can ascertain, the operation has hitherto been performed, and I think I am justified in saying, that an impartial review of these cases, especially of the later series, at least proves that the propriety of the operation is yet an open question.

In order to contribute to the solution of this question, I shall now give a short account of the cases in which the operation has been performed within the last three years, and of the present condition of the cases in which the operation has proved successful; and I shall point out one circumstance which, in my opinion, has hitherto been erroneously allowed, in most cases, to interfere with the success of the operation. The following account of these cases, I should mention, is given, with three exceptions, from my own personal observation of them. To satisfy myself more fully as to the results of the operation than could have been done by a written description of the cases, I lately crossed the channel to the Island of Jersey, where, by the kindness of Mr Jones, I had an opportunity of fully examining four of the patients, on whom the operation had been successfully performed; and I cannot omit this opportunity of expressing my admiration of the skill and enterprise with which Mr Jones, in a small provincial hospital, has prosecuted this branch of surgery. In addition to the cases of excision of the knee-joint, I saw in Mr Jones' wards two or three admirable results from the same operation on the elbow-joint, and one of excision of the head of the femur, which (apart from the question of the propriety of this mode of practice) would convince the most sceptical as to the usefulness of the limb after this operation.

Mr Fergusson has performed the operation three times in King's College Hospital. The subjects of all these operations were adults.

Case I.—A male, æt. 21. The operation was performed, July

20th, 1850, and proved fatal on the ninth day from acute necrosis of the femur, the same cause of death apparently as occurred in the child operated on in 1830 by Mr Syme.

CASE II.—A female, æt. 21. The operation was performed October 30, 1852. Considerable constitutional disturbance ensued, which subsided in a few days. Convalescence, however, was protracted, at a subsequent period, by a severe attack of erysipelas. I had an opportunity of examining this patient a short time ago (six months after the performance of the operation). The wound was entirely healed, but one or two superficial sinuses discharging a little matter still existed in its neighbourhood. The limb was shorter than its fellow by about three inches, and slightly bowed outwards at the knee. Slight motion (chiefly antero-posterior) existed between the bones. The limb was still œdematous, from the effects of the erysipelatous attack. The patient moved freely about the ward on crutches. She could bear strong pressure of the tibia upwards, but as yet did not bear any weight on the limb when walking. Her general health appeared to be good.

CASE III.—A female, æt. 28. The operation was performed, April 2, 1853. This case terminated fatally with all the symptoms of pyæmia, sixteen days after the operation.

Of the six operations performed by Mr Jones, four have made excellent permanent recoveries; the fifth is convalescent, and promises an admirable result; the sixth case terminated fatally.

CASE I.—A female, æt. 25. The operation was performed January 19, 1851. Unfortunately this patient was residing at a distant part of the island from St Heliers, and during my short stay of a few hours I was unable to see her. Mr Jones has furnished me with the following account of her present condition:—She is in perfect health, the parts about the knee having been long entirely healed. The shortening of the limb is a little over three inches. Complete anchylosis has not taken place, so that she requires a support on the inner side of the bone, with which she can move about freely, and can stand at her washing-tub for hours together. She is perfectly satisfied with her condition, and, with a more secure support for the knee, which is being made for her at present, the limb will be rendered still much more serviceable than it now is.

CASE II.—A boy of 11 years of age, when the operation was performed, April 27, 1851. This case appears to have been a peculiarly unfavourable one for the excision. Before the operation was performed, the leg (the left) was fixedly flexed on the thigh, so that the heel nearly touched the hip. The boy was emaciated and worn

by hectic, and numerous sinuses existed in the leg and thigh communicating with the diseased bones. The extent of disease necessitated the removal of the femur and tibia to the extent of nearly five inches. His recovery was tedious. The condition of this patient, when I examined him, April 21, 1853, was as follows:—He appeared to be in perfect health. The limb was sound and whole, although numerous cicatrices gave ample evidence of the extent of the previous disease. Complete anchylosis had taken place at the seat of operation, the limb being a little bowed outwards, and slightly flexed at the knee. The shortening of the limb, when compared with the other, was rather under four inches. The muscles of the limb were well-developed. He could walk and run quickly without any aid from a stick, could stand on the limb alone, and, to show me his confidence in it as a support, he *pirouetted* and hopped two or three yards without putting the right leg to the ground. No measurement of the limb had been made subsequently to the performance of the operation; but a careful examination of the apparatus in which it had been placed, and the conviction of Mr Jones and of the nurse who had attended the boy, satisfied me that the limb had grown upwards of two inches since the operation had been performed.

Case III.—A lady, æt. 30. The operation was performed, September 4, 1851. Everything progressed as favourably as could have been wished, for eight days after the operation. At the end of this time, she was attacked by dysentery, from which she sank fourteen days after the performance of the operation. With regard to the cause of death in this case, Mr Jones writes to me, that "at this time affections of the bowels prevailed everywhere, and more particularly in the vicinity where this lady resided, indeed it was the epidemic of the day. The mortality at this period was very great, and very few persons laboured under or died of any other disease. I feel fully persuaded that the operation was not the immediate cause of death. Those who saw the patient with me were of the same opinion; still I am unwilling even to attempt to prove this."

Case IV.—A boy, æt. 7. The operation was performed fifteen months ago. The condition of the patient and of the limb previously to the performance of the operation, was very similar to that of Case I., the leg being fixedly flexed to the full extent on the thigh. Rather more than four inches of the femur and tibia were removed in the operation. The condition of this patient, when I examined him, April 21, 1853, was as follows:—The boy is in perfect health, the parts about the knee having been for long entirely healed. Complete anchylosis has taken place, the limb being slightly bent forwards at the knee. The limb is $2\frac{3}{4}$ inches shorter than its fellow. He stands equally securely on either foot, walks and runs quickly without any support, and, to show me the use he could

make of his limb, he ran up and down the ward, kicking his cap like a football with the foot of the anchylosed limb. I need scarcely say that the halt, with which this and the other boy (Case I.) walked, was very considerable and unseemly. Both, however, were provided with shoes, with a firm but light support under the sole, with which they walked steadily and easily, and with but a slight appearance of lameness.

CASE V.—A male, æt. 20. The operation was performed six months ago, previously to which the limb was flexed at an acute angle, as in Cases I. and IV. When I saw this patient (April 21, 1853), he appeared to be in good health. The parts about the knee were firmly cicatrised, with the exception of a superficial sinus, which still continued to discharge a little matter. All swelling had disappeared, and the limb, with the exception of this trifling sinus, appeared perfectly sound, and presented less trace of the operation which had been performed than I could have conceived possible. There was complete anchylosis in the straight position. The extent of shortening of the limb was rather under an inch and a quarter. He walked about the ward with crutches, the limb not yet having acquired sufficient strength to allow of his walking easily without support. He laid aside his crutches, however, and showed me that already he could walk without them, and with but a very slight halt. I have since heard from Mr Jones that this patient is progressing rapidly. As far as I could judge, this case promises the most perfect result which can be reasonably expected, after the removal of the patella, and articulating ends of the femur and tibia.

CASE VI.—A boy, æt. 9. The operation was performed, April 17, 1853. In this case the patella was not removed, its carious surface only having been taken away by the gouge. The head of the tibia and condyles of the femur were removed without division of either the tendinous or ligamentous attachments of the patella. Four days only having elapsed since the performance of the operation, when I saw this boy, I can, of course, say nothing as to the result. He was suffering little or nothing when I saw him; and Mr Jones writes to me four weeks after the operation, that the boy progresses most favourably, and gives promise of a still more satisfactory result than in any of his former cases.

The next two cases which I have to mention are those in which I have myself performed the operation. The following brief outline of these cases is taken from the hospital reports of my house-surgeon, Dr Thom, under whose immediate care the patients were, and whose care and attention has contributed materially to the hitherto satisfactory result of these cases.

CASE I.—William Harrison, æt. 42, an hostler from Carlisle, was

admitted under my care in the Infirmary by the recommendation of Mr Macdonald of Leith, September 28, 1852, suffering under disease of the left knee-joint of five years' standing. The symptoms were those which are usually attributed to ulceration of the cartilages, uniform thickening of the parts around the knee, severe gnawing pain, especially during the night, and much aggravated by the slightest motion of the joint. Considerable relief was obtained by the joint being kept immoveable in leather splints, and by the repeated application of the actual cautery. The thickening of the soft parts became diminished, and I hoped the patient was to recover with a stiff joint. A painful spot, however, remained over the inner side of the head of the tibia, on account of which the counter-irritation was continued, and the joint retained for a long time fixed by the leather splints. He continued in this condition till about the middle of January, when, without assignable cause, the pain in the knee, especially on each side of the head of the tibia, became much aggravated. The pain was now so severe and constant that, in spite of the free and frequently repeated use of morphia, he scarcely slept night nor day. The swelling of the joint again increased, and the foot and leg became œdematous. On the 26th of January I called a consultation of my colleagues on the case, as it appeared to me that the removal of the disease by operation was alone likely to save my patient. It was thought, however, that a repetition of the same measures as had been already adopted, might still save the joint. The leather splints were accordingly readjusted, and the cautery once more applied in front of the joint. No relief, however, was obtained, and after ten days' further delay I yielded (with the consent of my colleagues, who saw the case with me) to the earnest solicitation of the patient, to remove the disease by operation.

The operation was performed, February 5, 1853. A straight incision was made across the front of the joint, a little below the level of the patella, and extending to rather less than half the circumference of the limb, and at right angles to each end of this transverse incision, a longitudinal incision of about two inches in length gave the wound somewhat the form of the letter H. The operation was performed quite according to the plan of Moreau, the patella being first removed, then the condyles of the femur, and lastly the articular surface of the head of the tibia. It was accompanied by smart hemorrhage from the articular arteries, which was at once arrested by applying ligatures to them. On dividing the tibia with the saw, the cavities of two abscesses in the cancellated texture of the bone, were laid open, each of a size capable of containing a grape. I carefully removed the remaining portion of the walls of these abscesses with the gouge, and then, finding that the diseased portions of bone had been entirely removed, I placed the ends of the tibia and femur in apposition, united the wound by sutures, and fixed the limb in the straight position, by applying a splint on its posterior surface. (The articular surfaces of all these

bones were found almost entirely deprived of cartilage; and both the cancellous and laminated structure of the bones, at various points, were in a state of suppuration and caries. The primary disease evidently consisted of suppuration of the cancellous texture of the head of the tibia. The extent of bone removed in the operation amounted to rather more than two inches—an inch and a half of the femur, and a little more than half an inch of the tibia).

The progress of the patient for the first two days following the operation, was most satisfactory. He slept well, which he had not done for weeks before; his pulse never rose above 80; he expressed himself as entirely relieved from the severe pain which he had been previously suffering. On the evening of the third day he began to suffer from hiccough, which very soon became constant, and continued unmitigated by all the remedies which were employed to relieve it, for six successive days and nights. The spasm of the diaphragm at length yielded on the seventh day under the continued pressure of a seven pound weight over the epigastrium, and the pretty free use of the tincture of Indian hemp, and everything went on favourably till about Feb. 23, when he began to complain of pain in the region of the diaphragm, and on pressure over the right hypochondrium. Obstinate diarrhœa succeeded, the evacuations presenting no appearance of bile. He lost all appetite; became desponding as to his recovery, and lost flesh rapidly. The wound, however, never presented, during this time, any very unfavourable appearance. Primary union had failed in the greater part of its extent, but the discharge was moderate, and the entire surface was covered by healthy granulations. Considerable difficulty, however, was experienced from the unfavourable condition of the patient, in keeping the bones in proper position, the end of the thigh-bone having a great tendency to project forwards and outwards. The diarrhœa continued, with short intervals, for about a month, and then gradually ceased under the use of mercurial alteratives, and the repeated application of blisters over the region of the liver. He then began to improve, and in a short time I considered him out of danger, when he was attacked by acute pleuro-pneumonia of the right side, the inflammation extending apparently over a large surface of the diaphragmatic pleura. Under Dr Douglas' care he again recovered from this attack; and he has now for the last six weeks gone on improving steadily, and is now, I am glad to say, quite convalescent. With regard to the state of the limb during this time, I may mention shortly, that in spite of all the serious constitutional derangement, the wound progressed slowly but steadily. The long-continued diarrhœa, and the emaciated condition of the patient, rendered it no easy matter to keep the bones in good position; but under Dr Thom's careful management, this was satisfactorily accomplished. At the end of six weeks after the operation, the motion at the knee was very limited, and since this time it has progressively diminished.

The present condition of this patient (three months and a half

after the operation) is as follows:—His general health is tolerably good, and is improving rapidly; he sleeps and eats well, and has no complaint of uneasiness of any kind. He is still confined to bed, but this is a measure of precaution more than of necessity, to prevent any motion at the seat of operation till the bones are firmly consolidated. The limb, which is supported by a leather splint behind the knee, and a starched bandage, is slightly bowed outwards at the knee, and is one inch and a quarter shorter than its fellow. The wound is healed, with the exception of a granulating surface on the outer side, which may be covered by the point of the finger. On the inner side, too, there is a small discharging surface, where a superficial abscess of trifling extent was opened about a week ago. There is still a little œdema of the whole limb, but this has diminished steadily for some time, and is now nearly gone. The bones at the knee are immoveably fixed as regards lateral motion, but on using a little force, slight motion backwards and forwards can still be perceived. Two or three weeks more, I have no doubt, will serve to complete the osseous anchylosis. He can move the limb freely in all directions, but is directed to keep it in as quiescent a state as possible.

The points in the progress of this case to which I would wish to direct attention, are—1st, That the patient was suffering under a high degree of irritative fever at the time of the operation; 2d, That he suffered, from the third to the tenth day following the operation, from severe and incessant hiccough, which, as the subsequent symptoms proved, was referable to a local source of irritation on the surface of the diaphragm; 3d, That, from the end of the second to the seventh week succeeding the operation, he suffered from nearly constant diarrhœa, evidently dependent on hepatic derangement of a serious character, and which, Mr Macdonald informs me, was of old standing; 4th, That, two months after the operation, he passed safely through a severe attack of pleuro-pneumonia; and lastly, that, in spite of all these untoward complications, the wound, at the end of three months after the operation, was, with the trifling exception I have mentioned, entirely healed and the bones consolidated, though not quite immoveable, in a nearly straight position.

I think I do not misstate the case, when I say that the serious constitutional symptoms which presented themselves were wholly independent of the peculiarity of the operation, and that this patient survived and made a good recovery, in spite of complications under which he must almost inevitably have sunk, had he suffered amputation of the thigh.

Case II.—Isaac Johnston, æt. 28, a fisherman from Shetland, was admitted under my care in the Infirmary, February 14, 1853, suffering from disease of the right knee-joint, of about ten months' standing. The affection had commenced, without assignable cause,

by pains and swelling of the joint, which gradually increased; and, for two or three months previously to his admission into the hospital, had entirely disabled him from moving about. The condition of the joint was similar to that of Harrison, but the symptoms were less severe. The pain was considerably mitigated by the joint being kept immoveable in leather splints, and by the repeated application of blisters. About a month after his admission into the hospital, the pain again became more severe, and was referred chiefly to the outer side of the head of the tibia. The actual cautery was freely applied, but without giving any relief, while increasing deformity of the joint, and pain on slight motion, gave evidence of the morbid changes which were in progress in and around the articulation. Shortly after this, he had an attack of modified small-pox, from which he speedily recovered. No improvement, however, took place in the condition of the joint. He continued to lose flesh; the pain and the deformity of the joint continued to increase; and symptoms denoting suppuration of the joint presented themselves.

Sir George Ballingall, Dr Dunsmure, and Dr Gillespie, who saw the patient with me in consultation on the 2d of May, agreed with me in the necessity of performing amputation, or excision of the joint, to either of which measures the patient willingly gave his consent.

I performed the operation of excision of the joint on the 5th of May 1853. Having found the disadvantages of the H-shaped incision in my first case, I now exposed the interior of the joint by a semi-lunar incision, extending from the inner side of the inner condyle of the femur to a corresponding point over the outer condyle, the incision passing in front of the joint nearly as low as the tuberosity of the tibia. The flap thus found was dissected back, the *ligamentum patellæ* being divided, and the patella itself left in the substance of the flap. The rest of the operation was completed, as I have already described, with the important exception, that the patella was left in its place, and its immediate attachment left undisturbed. The cartilage, however, which remained on its surface was removed by the gouge, as well as the rough surface of bare bone around its articular margin. The amount of bleeding was very trifling: one of the articular branches and several small vessels were secured by ligature, and the wound was dressed and the limb supported as I have already described in my former case. (The interior of the joint presented the usual appearances accompanying advanced strumous disease of the synovial membrane, suppuration of the joint, universal thickening and degeneration of the membrane, and ulceration of the margins of the cartilage in nearly their entire extent. Great part of the cartilage covering the articular end of the bone was as yet unaltered. Three-fourths of an inch of the tibia, and fully an inch and a half of the femur, were removed.) With regard to the progress of this case since the performance of the operation, I need only say that the patient has suffered as little local and con-

stitutional disturbance as usually follows excision of the elbow-joint, and certainly very much less than usually follows amputation of the thigh. He has been almost entirely free from pain, and has slept and eaten well, whilst his general appearance has been such that no one, who was unaware of the operation which had been performed, would, on seeing him, have imagined that he had undergone an operation which was dangerous to life. Nearly a half of the wound is already healed, and the remainder is covered by healthy granulations. The discharge, which has never been great, is already diminishing in quantity. There has not been the slightest tendency to displacement of the bones from the straight position,—a circumstance which I attribute, in a considerable measure, to the patella and its attachments having been left undisturbed. Little more than a fortnight has elapsed since the operation was performed, and I am, therefore, unwilling to say more of the case at present than that it promises, as far as it has gone, a most favourable result.

Whilst writing this notice of the above cases, I have received, by the kindness of Mr Page of Carlisle, an account of another case, in which that gentleman performed the operation about a year ago on a lad of seventeen years of age. The operation was performed, June 7, 1852. The patella, and two and a half inches of the femur and tibia, were removed. At no time after the operation was there any important amount of constitutional disturbance. Mr Page gives the following account of his present condition:—"He is now able to walk quite firmly, and without a stick, for a short distance, and is daily gaining greater control over the limb. He wears a shoe, the sole of which is about three inches thickened, that being the amount of shortening. The thigh and leg bones are firmly united, forming a firm and perfectly straight limb. The size in the situation of the excised joint is about that of the opposite knee."

The only remaining case in which I am aware of the operation having been performed within the last few years, is one in which Dr H. Stewart of Belfast was the operator. Unfortunately I have been able to obtain no further information from Dr Stewart regarding this case than that he considers the result "very encouraging."

Such is a short summary of the cases, of which I have been able to find any record, or to get any account, in which excision of the knee-joint has been practised. It is possible that other recorded cases may have escaped my observation, and I am not without hope that this communication may elicit further information relating to the practical results of the operation.

Although my chief object has been to give information on the more recent results of the operation, I may be permitted to add a few words in defence of those who have been censured for reviving a mode of practice which, for the last twenty years, appears to have been universally abandoned.

The propriety of attempting to save a limb by excision of the

knee-joint seems to depend on the three following considerations:—1st. Is the operation of excision attended by greater or less danger to life than amputation of the thigh? or, may the dangers of the two operations be considered equal? 2d. In the event of recovery after excision of the knee-joint, is the limb more or less useful and seemly than a wooden leg? 3d. Does the long confinement to the horizontal posture, which is necessary after excision of the knee-joint, in any measure counterbalance the benefits of the operation (if such benefits are proved) as compared with amputation of the limb?

A careful consideration of these questions and of the recorded results of the operation, led me several years ago to doubt the propriety of the operation having been rejected from surgical practice. I satisfied myself, by repeated trials on the dead body, that the operation could be performed without difficulty, and without the risk of wounding any important parts; and, as a teacher of surgery, I have been in the habit of mentioning to my pupils my impression that the operation had been rejected from practice on insufficient grounds. Knowing, however, the objections of my hospital colleagues to the operation, I never requested their sanction in carrying it into execution, till the late experience of Mr Fergusson and Mr Jones appeared to me to warrant my urging the question more closely on their attention, and my performing the operation in the cases which I have related.

With regard to the considerations which I have suggested as principally bearing on the merits of the operation, I would submit,—1st, That the danger of the operation may be reasonably expected to be less than that attending amputation of the thigh. The wound necessary for the removal of the diseased bones is less extensive than the wound of amputation of the thigh, whether performed by the circular or flap operation. The large vessels and nerves are not divided in the operation, the parts involved in the incisions being principally the integuments and ligamentous apparatus of the joint; the medullary canal of the bone is not laid open, a point which has of late years been suggested as of some consequence in diminishing the dangers of amputation; the shock, which always attends, to a greater or less degree, the sudden removal of a large part of the body, is avoided.

Time and experience, however, can alone determine the comparative danger of the two operations. In the mean time, the results of the limited number of cases in which the operation has been recently practised, prove, as far as they go, that the danger to life is not great;[1] and unless some unforeseen accident should occur to the cases, which are at present in progress of convalescence, that the danger is inferior to that of amputation.

2d, With regard to the usefulness and the seemliness of the limb,

[1] Of the last *eleven* cases in which the operation of excision has been performed, *seven* have recovered and *two* are in progress of convalescence, at the periods of six and three weeks after the operation; *two* only have died, one from dysentery and the other from pyæmia.

I need only refer to the account I have given of the results of the operation in Mr Jones' hands, to show that the limb may retain a degree of usefulness which can never be attained by the wooden leg in common use, nor yet by the most expensive and efficient artificial limb which I have ever seen; and if, in the days of Mr Park, such a result could be obtained as that a man, whose knee-joint had been excised, could run up the rigging and perform all the duties of a seaman, surely, with all the resources of modern surgery, we ought to obtain an equally satisfactory result now.

3d, The remaining objection to the operation is the tediousness of the convalescence. This, I think, unquestionably detracts, to a certain extent, from the benefits of the operation. I am doubtful, however, after all, whether the recovery can be proved to be on the whole more tedious than the recovery from compound fracture of the leg; in some cases even the limb seems to have been restored to usefulness as soon as the patient would have been able to walk on a wooden leg, had the limb been amputated. In Mr Syme's case, for instance, "in the course of four weeks after the operation the wound was all but healed, and the limb, before the expiration of three months, had regained so much strength, that the patient could make some use of it in walking." [1]

Mr Syme, in another place, says, "It ought to be recollected, too, that, though recovery from amputation of the thigh is usually completed in three or four weeks, it is generally *at least as many months* before the patient can rest the weight of his body on the face of the stump, so as to use it in standing or walking." [2]

Another objection has been brought forward against the operation which requires to be noticed, viz., that, when the operation has been performed on children, the growth of the limb has been checked, and that, consequently, from the unequal growth of the two limbs, that on which the operation has been performed becomes so disproportioned to the other as to be ultimately useless. The only practical proof of this being so, which I can find, is Mr Syme's case; and I think it is possible that it might be attributed in this instance to anchylosis not having taken place. Experiments have been performed on animals to show that bones do not grow when deprived of their heads, but the results do not appear to be conclusive. A few years will be required to confirm this as a valid objection to the operation. Mr Jones has, in the meantime, made accurate measurements of the limbs of the three boys on whom the operation has been performed, and will have an opportunity of giving further information on the subject hereafter. I have, in an earlier part of this paper, mentioned the fact, that the limb in one of Mr Jones' cases had already, since the operation, grown considerably, and presented now no appearance of checked growth. As bearing on this question, I would be glad to know if the growth of the upper ex-

[1] Syme on Excision of Diseased Joints. 1831. [2] Ibid.

tremity is checked by excision of the elbow-joint. It is so, as far as I remember to have observed, only in a slight degree. Surely the numerous children who have undergone this operation at the age of six or seven, have not grown to adult age with the stunted arm and hand of little more than an infant. Supposing, however, that the fact is so—that the limb is so much *respectively* shortened in after years—the objection applies only to the operation being performed on children, and has nothing to do with the question of the propriety of excision in the adult.

I have suggested that an error has, till lately, been committed in the performance of the operation, which has been allowed to interfere most seriously with its result. I refer to *the removal of the patella.* It is said that the patella is generally extensively diseased in affections of the knee-joint demanding excision or amputation: this, I have no doubt, is an error. The amount of disease to which it is liable is very limited,—in the great majority of cases consisting only in its being more or less deprived of its cartilage, and of a rough, or perhaps carious condition of its articular aspect. The patella is rarely, if ever, the primary seat of the disease in affections of the knee-joint. When the disease commences in the bones, it invariably does so in the cancellated texture of the head of the tibia, or condyles of the femur. It is extremely rare that there is such extent of disease in the patella as to prevent its easy and effectual removal by the gouge, the body of the bone being left uninterfered with. The advantage of leaving the patella cannot fail, I think, to be very great. The natural form of the joint is preserved; the attachment of the extensor muscles is left undivided; the wound is less extensive; the annoying tendency to displacement forwards of the end of the femur seems, in a great measure, to be done away with; and there is every reason to believe that the consolidation of the bones will proceed more rapidly, and the limb ultimately be found both more useful and more seemly than when that bone is taken away. This is a point, however, which remains to be proved. In the two last cases, however, in which the operation has been performed by Mr Jones and myself, the patella has been left, and the advantages of its having been retained are already apparent in each case.

I shall not lengthen this paper by describing what appears to me the best mode of performing the operation, but merely mention that the semilunar incision seems to me the most advantageous, as being the smallest by which the joint can be satisfactorily exposed, and as giving rise to less bleeding than the H-shaped incision. There is one point, however, to which I wish to direct attention. It has been recommended that a considerable portion of integuments should be removed by a double lunated incision, to prevent the redundancy of skin which might be expected from the large amount of bone removed in the operation. I believe that this advice, if followed, will lead to great annoyance. I have not removed any portion of in-

teguments in the cases in which I have performed the operation, notwithstanding which, the retraction of the skin was such as to cause, in each case, more or less gaping of part of the wound. Mr Fergusson mentioned to me that he had removed a small portion of integuments in the operation, in the case now in King's College Hospital, and that, during the patient's convalescence, he had much reason to regret having done so, as the retraction of the integuments was such as to leave part of the end of the femur uncovered, and a thin cicatrix only now covers this point of bone.

I shall not add any further details as to the performance of the operation or the after-treatment of the patient. My chief object at present is to present a summary of the cases in which the operation has been recently performed, and the results of these cases as far as they have gone, and I have endeavoured to do this faithfully and impartially. From what I have myself seen, I am satisfied as to the utility of the operation, and I think that the details which I have given of these cases prove that the operation, till its recent revival, had been banished from surgical practice on insufficient grounds, and that the propriety of practising it in certain cases, is still at least an open question.

Part Second.

REVIEWS.

Illustrazione di tutti gli Strumenti Chirurgici scavati in Ercolano e in Pompei e che ora conservansi nel Real Museo Borbonico di Napoli. Compresa in Sette Memorie lette all' Academia Ercolanese. Dal Cavaliere BENEDETTO VULPES, Professore di Clinica-Medica nell' Università di Napoli. Napoli: 1847. 4to. Pp. 94.

Description of all the Surgical Instruments excavated at Herculaneum and Pompeii, and presently preserved in the Royal Bourbon Museum of Naples. Comprised in Seven Memoirs read to the Herculanean Academy. By BENEDETTO VULPES, Cavalier, etc. etc. Naples: 1847.—(*Continued from p.* 468.)

REFERRING to the illustrations of the speculum magnum, given in our last number, we now proceed to describe a little more fully this remarkable instrument, and its mode of use as apprehended by our author. In Fig. 1, which represents the entire instrument, and in Fig. 2, which gives a side view, and therefore includes only two of its three branches or blades, the external aspect of each blade is

observed to be convex, and the internal to be of the form of a triangular prism.

From this peculiarity of construction, it follows that, when the three blades are closed, their surfaces lie in exact contact. By the union of the blades, a kind of cylinder is formed, half an inch in diameter, and as at the distance of six and a-half lines from the top of each blade, there exists an enlargement of the width of eight lines, the entire cylinder terminates in an olivary form. Professor Vulpes remarks that the distance between the points of the extended blades varies considerably, thus the measurement from the points of the two superior blades is 3 inches and 3 lines, while, from the point of the right superior blade to the point of the third or lower blade, it is 3 inches and 4½ lines, and from the left superior blade to the same, 3 inches and 5½ lines. These differences in actual measurement our author attributes to the oxidation of the bronze, a change which, though it has to a certain extent affected the whole metal, is not unlikely to have operated on some parts of the instrument more than upon others. The blades at their greatest divergence enclose a circular space, whose diameter is equal to 3 inches and 11 lines. A few words remain to be said concerning the other portions of the speculum. Firstly, regarding Fig. 1, it will be seen that the two superior blades are supported by two bars, which form the sides of the instrument, and whose shape is double curved, each having below its middle point an immoveable joint; and the extremity of their handles being rather neatly turned. The third or lower blade again, is affixed to a bar which crosses and plays over both supporters of the superior blades, and is attached to a finely-wrought screw, upon which (see Fig. 1) a button-like prominence is seen; and whose handle is most elegantly turned. The screw is furnished at its summit with a neat cap, which is fixed by a small iron hinge. Now, regarding Fig. 2, the screw is seen to pass through a cylinder corresponding to the button-like process seen in Fig. 1. This description of the speculum might easily be made more minute, but probably the best idea of it will be had from referring to the two figures. In using the speculum, the handles are taken with the left hand, while, with the thumb and index finger of the right hand, the manœuvring of the screw is conducted. If the screw is moved from right to left, the cross bar is elevated, and the lower blade of the speculum attached to it is caused to approach the two superior blades, which are at the same time being approximated by means of the handles. If, on the other hand, the screw is turned from left to right, the cross bar descends, and with it the third or lower blade of the speculum. The mode of using the instrument is thus apparent, and by its means Professor Vulpes adds, that the morbid states of the os and neck of the uterus can be seen. Professor Vulpes appears to imply that the speculum was so used by the Pompeian physicians—for ourselves, we are scarcely satisfied so to conclude, and we still think it a question of doubt, whether this "speculum magnum"

was used as a means of seeing the morbid states of the uterus at its neck and mouth, or simply as a dilator, perhaps in certain cases of difficult parturition from the os uteri not being sufficiently open.

Having discussed the subjects of the fourth, we now revert, as proposed, to the other memoirs, taking these according to our author's own arrangement. The *first* memoir, then, relates to the forceps with bent or curved claws. This instrument, which, like the speculum, is made entirely of bronze, and in its general appearance is not unlike the modern bone pliers, is composed of two portions, placed the one upon the other, and united by means of a hinge. The blades are of a double curved form, and are not equally long. Each, for a little less than half its length, is marked apparently by parallel indentations as in a file, but, in reality, having a prismatic form, and being smooth. For the greater part of their course, the indented surfaces can be placed in contact, but towards the point of both claws a space of a line intervenes. The handles of this forceps are most elegantly turned and fluted; they also terminate in a neat head. When the blades are closed, the handles are distant from each other at their heads 1 inch $5\frac{3}{4}$ of a line. The greatest extent to which the handles can be separated is seven inches and five lines, while the claws can be expanded 3 inches and 9 lines. Such is a short description of the forceps with the curved claws. As to its probable use there has been a difference of opinion. Our author believes that the idea entertained by previous writers, whether physicians or archæologists, was erroneous. That idea was, in the words of Dr P. Savenko, of St Petersburg, writing in the Annali Universali, edited by Omodei, that the forceps were similar to those used in the time of Cornelius Celsus for the extraction of loose teeth. This, adds Professor Vulpes, seems to me incompatible with the size, solidity, and form of the discovered instrument. His own opinion is, that this forceps was used for seizing and removing from wounds, small, loose, or detached portions of bone, and for removing any foreign body from almost any cavity into which the forceps could be introduced. Farther, and very important, that this instrument served the purpose of seizing an artery before the application of the ligature—was, in short, an artery forceps. In supporting his argument as to the improbability of this instrument having been used for the extraction of loose teeth, our author observes that the ancients were in the habit of employing no instrument for that purpose, but of accomplishing it with the unaided hand—farther, that the forceps alluded to by Celsus was of a different nature, admirably adapted for the purpose of extracting teeth which were not loose. The forceps of Celsus was in reality the rizagra (ῥίζαγρα) of the Greeks, an instrument varying in important particulars, of which the marked inequality of the fangs is most noticeable, from the Pompeian forceps. Castelli, in his Greek and Latin Lexicon, appears to corroborate the view taken by Professor Vulpes. In regard to our author's argument for the second and very important

use of the forceps which he has suggested, viz., for the seizure of arteries before the application of the ligature, we would willingly follow him, the more so, as he has advanced some most interesting particulars, and to us appears to have made out his case. We must, however, pass on to the *second* memoir. The first instrument described in this memoir is a forceps of very peculiar form. It is made of bronze, and, like the forceps described in the first memoir, is composed of two portions united in the middle by a moving hinge. It has two handles which terminate in elegantly turned cylinders; while its blades are at their termination half spoon-shaped, and of a semi-elliptical form. The internal margin of each blade is provided with a set of small triangular teeth, those on the one side fitting into those of the other when the blades are closed. At the inferior extremity of each indented margin is a small circular opening—when closed, a space is left, of an elliptical form, between the shoulders of the instrument. Unless this instrument is the one referred to by Bayardi in his Catalogue, under the title of "Circino escisorio fatto a foggia di uno de' nostri compassi," then no writer on the instruments discovered in the ill-fated cities has taken notice of it, far less of its probable use. Professor Vulpes evidently thinks that it is the instrument so referred to by Bayardi, and it certainly has not a little of the appearance of modern compasses. Of its use, our author declines giving an opinion, but advances a simple conjecture that it was suitable for introducing into any long internal cavity, and for removing excrescences from such, or for retaining them in order that the knife might be applied for their removal.

The second intrument described in this memoir is a canula, for the removal, according to our author, of the dropsical fluid from the abdomen in cases of ascites. It consists of a small pipe divided unequally by a plate of bronze, of which metal the entire instrument is made, the lower portion of the canula terminating in a point like that of a pen for writing. The transverse plate apparently served the purpose of limiting the introduction of the canula into the abdominal cavity. The other three canulæ preserved in the Bourbon Museum, and one of which is figured in Professor Vulpes' work, were, he thinks, for the purpose of removing pus or other morbid fluids from any deep seated locality, as well as for introducing medicated fluids into deeply situated parts. That this plan of procedure was adopted by the ancients is evident from a passage in Hippocrates,[1] the quotation of which terminates our author's second memoir.

Forty-five probes have been found in Herculaneum and Pompeii, and are now preserved in the Bourbon Museum. Of these, and of certain instruments of a somewhat similar nature, Professor Vulpes speaks in his *third* memoir. Many of the discovered probes are in fragments—some are broken, and others acted on by the progress of

[1] See Hippocrates de Morbis Mulierum, Lib. ii.

oxidation are cemented together with the pincers, and the cases in which they are held. The probes found in Herculaneum and Pompeii present some differences in their form, and chiefly in that of their extremities. A reference to the list already given will illustrate the varieties at a glance, as well as the number of each. They are all made of bronze, two of the number are striped in a spiral manner with silver. Of the twenty-one which, in the catalogue, are marked *ciatischi*, fifteen were together in a small open box, and six, along with some pincers, within the regular surgical cases. The latter are long and round, marked by equidistant circular lines with lids, and resemble the metallic cases now used for holding different kinds of plaisters. On the history of the probe, Professor Vulpes makes some interesting remarks. It was called by the Greeks μηλη, by the Latins *specillum*, and is a most venerable weapon. Its invention was attributed to Esculapius. Cicero, in the "de Natura Deorum," writes, "Æsculapiorum primus, Apollinis, quem Arcades colunt, qui *specillum* invenisse, primusque vulnus dicitur obligavisse." The use of the probe in making examinations is expressed by its Latin name, and the word from which it is derived, *speculor*. The *specilli* or probes were designed just as they still are, to take the place of the eyes; and where sight could not avail they were called into use—they served to explore the wounds, the fistulas, etc., where the finger of the surgeon could not reach, nor his eye penetrate. Celsus was acquainted with the probe. "Id quoque vitii (caries)," he says in his eighth book, "specillo significatur," so also Paulus Ægineta, in speaking of the extraction of weapons, says, "If the weapon has a tail, which is ascertained by examination with the probe."[1]

The third memoir concludes with the description of the catheter of bronze. This most interesting instrument is ten inches long, and of a diameter of three lines, and possesses a double curve. It appears that, in the writings of all the older authors, when allusion is made to the catheter, it is, with one exception, to that with the single curve. Galen seems, however, to have been acquainted with a catheter similar to that found in Pompeii, for in the "Introductio seu medicus," attributed, at least, to him, reference is made to it in the following words:—"Porro huic instrumento ad romanae literae S. figuram facto, quod in urinae iter usque ad vesicam demittitur, inditur filum, quod in summo habens lanae paululum procumbens, urina madefactum, deinde foras extrahitur, quod urina tanquam ducem viae sequitur." Celsus never alludes to the double curvature, though he speaks of different kinds of catheters, according to age and sex, of the patient. On this point, the learned Paulus Ægineta also speaks, but he nowhere shows his acquaintance with the instrument of Pompeii. Petit, the inventor of the double curved catheter in modern

[1] Adam's Edition, Book vi., chap. 88.

times, is in all probability entitled to the full credit which he has always enjoyed. He could not have seen the instrument at Pompeii; and Bayardi's Catalogue was published four years after his life closed on the banks of the Seine. It is just possible that, in reading the works of Galen, he may have hit upon the passage already quoted; but, like our learned author, we much prefer to give the distinguished French surgeon the benefit of the doubt—and "de mortuis nil nisi bonum."

(To be concluded in next Number.)

On Lithotrity and Lithotomy. By WILLIAM COULSON, Surgeon to St Mary's Hospital, etc. 8vo. Pp. 388. 1853.

THE literature of lithotrity and lithotomy is already so extensive, and embraces so much that is worthless, and so little that is truly excellent, that a new treatise on the subject is not likely to find much favour in the eyes of the critic, unless it contains something to atone for the faults of its predecessors. If, then, we were to regard Mr Coulson's goodly octavo as an original work, and as a further contribution to this now very hackneyed literature, we might feel disposed to complain of the paucity of information which it conveys. But the book is in reality a review—a judicious and elaborate commentary upon former writers, executed, moreover, by a surgeon of considerable personal experience, who states his views on practical points where there is room for difference of opinion, modestly, yet with becoming confidence. Several chapters of the work originally appeared as Reports of Lectures in the pages of the "Lancet," from which inauspicious medium of publication Mr Coulson has, in our opinion, acted well in now promoting them. They have been considerably expanded, and are submitted to the public with all the advantages of thorough revision, profuse woodcut illustration, and readable type.

The subject of lithotrity is first discussed, and its history, practice, indications, and counter-indications successively passed in review. Those who are curious in the armamentum of surgery will, in the first chapters of the volume, find representations of many strange forms of lithotritic apparatus, some of which are both efficacious and ingenious, others ingenious but not efficacious, and many neither efficacious nor ingenious. It is right that some more enduring record than their own trumpery publications should be found for the inventions of the modern surgeon-cutlers; and by consulting that which Mr Coulson has supplied, we may learn how the idea of Ammon of Alexandria, who, according to Hippocrates, broke up calculi within the urethra "with a statuary's chisel" (see p. 2), has been improved upon by these gentlemen within the last thirty years.

Lithotomy is next considered in a somewhat similar manner; and here, in the history of the exploded methods of performing the operation, the surgical archæologist will find much to interest him.

The parallel between the operations by crushing and extraction is next drawn; and the volume ends with a chapter of statistics.

On one very important and practical point we are glad to observe that Mr Coulson expresses an opinion similar to that which has been maintained in former numbers of this Journal. We allude to the expediency of administering chloroform as a measure preliminary to the operation of lithotrity. We need not now recapitulate the reasons which Mr Syme has urged against this practice. In fact, they will almost suggest themselves to any one who reflects upon the subject. So we presume, thought Mr Coulson, for at p. 77 we find chloroform in lithotrity disposed of as follows:

"This perhaps might be the proper place to notice the applicability of chloroform to lithotrity, but various considerations induce me not to advocate the use of anæsthetics in that operation."

This doctrine, we regret to think, has not yet become universal; and we fear that nothing short of repeated accidents, clearly referable to the insensibility of the patient during the delicate manipulations of lithotrity, will secure its more general adoption.

COLLOQUIA DE OMNIBUS REBUS.

COLL. VI.—*On Incantations and Recantations, Piracy, and Quackery, etc.* COLL. PERSONAE *ut antea.*

Editor. The London weekly journals are slow to notice our cartel. Need we hang on any longer, waiting for them?

Chemicus. If you wait till they notice our cartel, you will have to wait long enough. They cannot take up the charge against them, without themselves giving currency to it, and so suggesting to their subscribers to try other reading besides the weekly medical press alone. Therefore, being wise in their calling, they have digested the affront, and lie still,—like a hooked salmon sulking at the bottom of a pool.

Chirurgus. Have you not a contrivance for poking him up?

Editor. Run a ring down the line over his nose.

Chemicus. Or pepper him with stones. An unsafe plan, however; as you may chance to snap the line.

Editor. In the present case, the Dublin "Weekly" has tried stone-flinging for us, without success.

Obstetricus. Is there no way of enchanting, mesmerising, or poisoning him outright in his pool?

Chirurgus. Medicus there will tell you

HOW TO POISON WHALES; which, one would think, a much more difficult matter.

Medicus. That is an old story now, and a long one. But it is worth telling; because, though the contrivance did not benefit the inventors, it may be of use to somebody.

One and twenty years ago, a Leith merchant, concerned with the Davis' Straits Fishery, called to consult me about the poisoning of his whales. I laughed heartily at the idea; and so did he. But when our mirth was over—and he had fairly tabled his proposal—it appeared to me a grave possibility. The whale, it seems, when struck near the ice, invariably makes for it; and if not first overtaken by the boats, it dives far under the field, and rising at some blow-hole, rests or dies there, inaccessible to its persecutors, who are forced to break away the line and abandon it. Ten per-cent are actually lost in that way. Now, if the whale could be even stupified only, so that the crew might overtake it with their lances, they would have it safe enough, although it should not actually die of the poison. By a rough calculation I found that two ounces of pure hydrocyanic acid, well administered, would probably kill, and at least stupify, any whale of the largest ordinary size. The ship's captain was therefore sent for, with his harpoon; and, after full deliberation, we fixed on the following device:—The poison was to be enclosed in two glass tubes of moderate strength, each capable of holding a fluid ounce,—the tubes to be placed on each side of the iron shank, close to the blade, and protected by its flukes, and by a slight hollow for them on the shank. The tubes were held in their place by a strong copper wire, which was passed through holes in the shank, twisted over the tubes like the figure 8, and then fastened to a bight in the rope. We ascertained that, with our full force, we could shatter the tubes by pulling the wire. We thus made sure that a slight accidental tug would not break them; and that the strain of the animal could not bear on the rope till the wire broke, and consequently the tubes also in the first instance. The captain, an experienced harpooner, told me they always try to strike the creature in the chest, and to drive the weapon into the cavity, and, at all events, through the blubber, deep into the flesh. Hence the tubes would break exactly where the poison would be most deadly.

The next thing was to set about making the poison,—at that period not so easy a matter as now. My laboratory assistant and the ship's surgeon made, under my direction, 200 ounces of acid, of 50 per-cent strength, which we reckoned would be enough when concentrated for charging fifty harpoons.

Obstetricus. Let me see. One hundred ounces of pure acid, or 43,750 grains, ought to do for 40,000 human beings; so that you counted a whale worth 800 men—a safe calculation. Had you any accidents in your perilous factory?

Medicus. One day, the ship's doctor having, in the confidence of

familiarity, sniffed too freely the vapour issuing from the safety-tube of the apparatus, he staggered back, fell into the corner of the room, and lay there like a bundle of rags; but his companion soon brought him round with hartshorn.

The acid was preserved in ice as it was made; and the surgeon took with him a quantity of fused chloride of calcium, to concentrate it when required for use.

Chemicus. The poison could be supplied more easily now-a-days, since we know that pure hydrocyanic acid, condensed in glass tubes, and hermetically sealed in them, will keep almost indefinitely.

Medicus. True; I have a specimen seven years old, which is still perfect. Besides, we are now acquainted also with other equally active poisons, which are safer to work with, such as conia, aconitina, nicotina, and even strychnia, when pure.

Ere long the good ship reached the whale latitudes. The surgeon set about the concentration of the acid. His laboratory was the cabin, his assistant the mate; and he took the wise precaution of lifting off the deck window, and warning the mate to bolt at once for the companion-stair, should any breakage happen. In no long time an eight-ounce phial of the 50 per-cents slipped through his fingers, and was smashed on the cabin floor. The mate, true to orders, instantly started for the stair, and had just time to mount and call out "Fly to the doctor," when he tumbled senseless on the deck. The captain, a man of nerve and promptitude, in a moment seized a boat-hook, hooked up the doctor by the waist through the window, dashed sea water over his head, and revived him, as well as the mate, by the same means.

Chemicus. Bravo, captain! First-rate practitioner! *Vide hujus Menstruarii nostri, tom.* x. 97, *anno* MDCCCL.

Medicus. The surgeon and mate, nothing daunted, fell to again next day, and got through the job without further mishap.

Towards nightfall their ears were gladdened by the cheerful call of the look-out at the mast-head, "There she blows!" The boats were then got in order for a start at early dawn.

But, alas, for the fickleness of all human hopes! In the middle of the night two huge ice-fields came down upon the ship, nipped off its bottom between them, crushed the boats, sunk the harpoons, and forced the crew to fly for life to the ice.

Chemicus. And was this the end of the experiment? What became of the crew?

Medicus. Messrs Young, the spirited contrivers, were afterwards led to abandon the Davis' Straits trade, and had no further interest in the matter. The crew were taken off the ice by a friendly whaler. They, of course, had got an inkling of what was intended; and some of the outlandish harpoons, picked up by others on the ice, set many a-guessing. Accordingly, for several years afterwards, notices appeared in the newspapers of projects for poisoning whales. But I suspect these were merely successive editions of Messrs

Youngs' adventure, and that no experiment has hitherto been actually tried.

Chirurgus. As nothing more, then, can be made of whales for the present, let us try our hands in the mean time on some other species. Here, for example, is a shark worth capturing—

DR G. T. GREAM ON THE EMPLOYMENT OF CHLOROFORM IN PARTURITION. The medical world has hitherto assumed by common consent, that etherisation in surgery originated in America, and obstetrical anæsthesia in Edinburgh. But here is an individual, inhabiting London in 1853, who assigns the former discovery to a London surgeon, and ignores the latter fact altogether. He does not actually claim the obstetrical use of chloroform for himself. A cautious and sceptical reader may surmise from the pamphlet, that in the art of using chloroform there may possibly have been valiant men before this Agamemnon. But what is to be said of a fashionable London practitioner who, professing to write for the enlightenment of his benighted neighbours, as to the safety and comfort of chloroform in female labour, contrives to suppress every allusion, however slight, to the man to whom alone the world is indebted not only for the discovery of this blessing, but likewise for the sound and established precepts as to its right employment in surgery.

Medicus. *A priori*, I should have offered a reward for ingenuity to the successful author of such a literary curiosity.

Chirurgus. Dr Gream has discovered, it seems, by his own unaided faculties, that, when duly administered, without the necessity of causing complete insensibility, chloroform—1. "Procures more or less perfect immunity from the pains of labour;" 2. "May be so managed as to prevent the progress of labour being interfered with;" 3. "Does not interfere with the voluntary expulsive efforts of the patient;" 4. "Acts beneficially in allaying after-pains;" 5. "Materially facilitates the manipulations of the operator in turning;" and 6. "Is not injurious to the child." But all these principles were acknowledged here five years ago at least, and have been published here and elsewhere over and over again; and the only original idea in the whole pamphlet is a childish conceit about whiffing chloroform out of a scent-bottle.

Medicus. Is the author, Dr Gream, any family connection of Dr Simpson's more ancient adversary, Mr Gream, on "The Misapplication of Anæsthesia in Childbirth?"

Editor. There seems a strong family resemblance between them in their hostility to Dr Simpson, but much the reverse in their obstetrical principles, for *Mr* Gream had an overwhelming horror of chloroform. It was in his eye "so dangerous," and led to such "a frightful extent of casualties," as to be "not within the limits of sound medical practice." Etherisation he regarded as merely "another word for intoxication," and etherisation by chloroform "a gross immorality"—"a state of gross drunkenness, with

the accompaniment of obscene expressions and actions, into which no woman above the level of the lowest would consent to be put"—"a state bordering on death, and giving rise to sensations and actions of so revolting a nature, that men actually shudder at the very thought of its being induced in their wives"—a state which "brings a chaste woman to a condition debauched even to the extent of a prostitute." Besides, he says chloroform does "not produce immunity from the pain of labour," and "women have been merely made to imagine they had no pain." It retards labour by "lessening uterine contraction"—"increases the fatality of childbed"—renders "all more or less predisposed to subsequent puerperal disease"—and "is likely to be very injurious to the child." In the mother, he says, it occasions loss of milk, emaciation, vital depression, destruction of personal beauty, premature old age, protracted giddiness and loss of memory, bronchitis, hæmoptysis and pneumonia, mania, and coma, and epilepsy, and paralysis, and apoplexy, and threatenings of death by asphyxia. In the case of the child it causes "acceleration of the pulse, struggles, and convulsions" in the fœtal state, "very painful to the mother;" and after birth "peritonitis," "asphyxia," "intoxication," "a smell of ether;" and "time alone will tell whether Idiocy may not follow."

Chemicus. Mr Gream's "Misapplication," and Dr Gream's "Employment" of chloroform, constitute as magnificent an instance as I ever encountered of contrariety and contradiction in medicine. Are the two gentlemen on terms?

Obstetricus. You may rely upon it, that Dr Gream is on the very best of terms with Mr Gream. Will it surprise you to learn that Mr Gream of 1848, and Dr Gream of 1853, are one and the same person?

Omnes. Impossible!—Impossible!

Obstetricus. And yet true. There is only one indivisible Gream in London obstetrics—the original of the name having become Doctor lately by grace of the University of King's College, Aberdeen.

Editor. So that, in casting his skin, he cast out his principles.

Obstetricus. Not he. He got his new designation in 1850, but his new ideas only a few weeks ago. They are the effect of a much more recent and potential cause. The fact is, her Majesty, as all the world now knows, having lately taken advantage of chloroform in her confinement, it was easy to foresee that every fashionable lady in London, who, in consequence of the strong medical denunciations of Mr Gream and others, *would* not take it formerly, *will* have it now. Therefore it became a sheer necessity for Dr Gream to advertise by a new pamphlet his own readiness to gulp it also; and this flatulent explosion is the natural consequence.

Editor. But the "Lancet" says her Majesty did not use chloroform, as the newspapers allege.

Obstetricus. Depend upon it, Dr Gream knows better.

Editor. The "Lancet" of 14th May lifts its immaculate hands in

consternation at the profane possibility of any responsible being advising or allowing the monarch of these realms to put herself in such imminent peril, declares that all her medical advisers hold the use of chloroform during labour in abhorrence; and proclaims, as if from authority, that her Majesty did not take it on a late occasion.

Medicus. And, nevertheless, I can assure you, in defiance of "Lancet," and all other tools of our profession, and upon authority not to be gainsaid, that her Majesty did make use of chloroform, and that she profited greatly by it, and in more ways than in the mere saving of pain.

Chirurgus. I should like to know what Dr Snow had to do in the Queen's bed-chamber, if her Majesty did not get chloroform? [*Hear! Hear!*] But I dare say *Obstetricus* there can tell us all about it, if decorum do not withhold him.

Obstetricus. If what *Medicus* has told us be denied in any respectable quarter, I have no doubt that evidence of its truth will be forthcoming. Meanwhile, be assured that Dr Gream has not been one hour too soon in eating in his [*Yawns!*]— But I am weary of the subject. His pamphlet has made such a jumble of my ideas about chloroform, that I feel in some doubt who can have discovered its action and uses. I am much in the same case with a certain lecturer on natural philosophy in this city, when he went in great perplexity to Dr * * to ask him who first found out that the density of water was 1000.

Medicus. Then let us return to

THE LONDON HEBDOMADARIES; and tell us, *Chirurgus*, some of your adventures, when you went to visit the prisoners who were left in possession of the pirates, when we last met together. *How did* you get in to see them?

Chirurgus. Our conversation the other evening about the sad plight of these poor people, and some things respecting them which had come to my knowledge since, made me anxious to ascertain, from my own inspection, whether the state of matters was really so bad as it had been represented. As to getting in, there was no difficulty, since they are allowed on stated days to receive visits from a few particular friends.

Chemicus. Well. I am impatient to know what sort of reception you met with.

Chirurgus. Nothing could be more pleasant, apart from the melancholy reflection suggested by the helpless state of so many people obviously intended for better things. I was told that visitors were not allowed to carry a light with them. But thinking that such a rarity might not be unacceptable, I smuggled with me a pound of candles and a tinder-box; and you can have no idea how happy they looked when my luminary began to twinkle.

Medicus. Were all the prisoners there?

Chirurgus. Some of the old officers, who fought on deck when the

good ship was boarded by the pirates, and have ever since been in close custody, were not allowed to appear; and others, as I was told, fearing that the light might hurt their eyes after they had been so long in the dark, had shut themselves up in their cabins. But these weather-beaten, broken-hearted victims of oppression heard very well through the boards what was going on; and the younger men, who only require a feeling of honourable independence to break their galling chains, entered freely and agreeably into conversation,—so much so indeed, that we exhausted the hour of license, and had to send up a petition to the quarter-deck for extension of leave.

Editor. And did you really meet with nothing personally unpleasant?

Chirurgus. With nothing that could exactly be called unpleasant. But a rather amusing scene interrupted for an instant our quiet chat about old times. You must know the old pirate chief has a son, whom he has put into the midshipmen's berth, and dressed in a uniform made out of his schooner's old sails, and bits of bloody bunting for gold lace. This precious youth having been smuggled in by one of the prisoners, he began to cock his hat at me and look wicked; when all at once a gallant old quarter-master, who had been appointed to keep order on such occasions, came down upon him with such a whack with a rope's end that the mischievous imp was glad to fly up the ladder to the deck with all the speed which the condition of his stern permitted.

Medicus. This is all very well. But do you think that any permanent good will result from your expedition; which I assure you was the cause of no little anxiety to your friends here?

Chirurgus. As to that I don't know. My pound of candles will of course soon burn out. But I venture to hope that the glimpse of light they have had will create a desire for more, and that visions of freedom, which they have had during my visit, may make their present state of bondage too intolerable for much longer endurance. Indeed I shall not be surprised if the rocket manufactory at Rotherhithe, which has never yet been satisfactorily accounted for, should turn out to be connected with some plan for blowing up the old pirate with all his crew.

Physiologus. Did you see or hear any thing further of the friendly craft which *Medicus* thought he discerned looming in the distance,—the rasée with the blue flag, supposed to be the Provincial?

Chirurgus. Yes! But she is still in the offing, standing off and on, and making signals which nobody understands. And most people think we need not trust to much assistance from that quarter in effecting the deliverance of our friends from bondage.

Editor. Did you pick up any news of the globule-dealers in London?

Chirurgus. Very little. They seem as much countenanced as other quacks; and that is all.

Editor. I see poor Lord Robert Grosvenor, at the annual meeting the other day of the supporters of the Hahnemannic Hospital, had to give a very lachrymose account of its resources. He complains that last year's expenditure of only L.1900 exceeds the income by L.780.

Medicus. A very beggarly account of the wealth and influence of all homœopathic London! Honest men may now expect to get their own by and by. I think they will get it here, if right use be made of a late legal discovery, which threatens to place

QUACKERY IN JEOPARDY. It now ranks as an offence in the criminal code of Scotland, and is punishable with imprisonment.

Editor. Indeed! We shall have to build another wing to our new prison in that case. How did this come about?

Medicus. Very simply. For twenty years past and more, our English neighbours have been pursuing the phantom Medical Reform. Houses of Parliament, Select Committees, Secretaries of State, Royal Colleges, Medical Associations, Metropolitan, Provincial, and National,—have all been called into requisition. And the result is, that they are now just exactly where they were twenty years ago: they are not even any nearer the simplest of all their objects, the abatement of the quack-nuisance. But, what this prodigious armament has been unable so much as to approach, a local judge in Scotland has successfully commenced, and without feeling any occasion to call upon Hercules for help.

Editor. Who?—Where?—How? This is wonderful news! Hercules himself perhaps ——?

Medicus. The Sheriff of Edinburgh, John Thomson Gordon, has discovered that quackery is "Falsehood, Fraud, Wilful Imposition, and Swindling," and has treated it accordingly.

Chirurgus. A very simple discovery, truly! Is it possible that the law was ignorant of it till now?

Chemicus. Every great discovery seems simple enough, as soon as it is made. In law there is much philosophy lying hid in secret nooks, for occasion to bring into light. And yet it does appear very strange that no benevolent sheriff in this country ever before observed an axiom lying so near the surface, as that quacks are fraudulent liars and swindling impostors,—or so self-evident a conclusion as that this is a grievous public wrong, requiring a vigorous public remedy. But what was the case which occasioned the discovery?

Medicus. Two speculators had of late distributed widely a handbill in and around Edinburgh, bearing that "Professors Curtis and Campbell, two celebrated botanical physicians from the Royal Botanic College of Health," "recently arrived on a professional tour from America," undertake to cure "deafness, and all diseases of the eye, provided the sight is not gone, or the drum of the ear broken," —also "ruptures permanently without a truss, provided the patient is not too old,"—besides "ague, asthma, bilious complaints, blotches

on the skin, bowel complaints, colics, constipation in the bowels, consumption, debility, dysentery, erysipelas, female irregularities, fevers of all kinds, fits, gout, headaches, indigestion, inflammation, jaundice, liver complaints, lumbago, piles, rheumatism, retention of urine, sore throats, scrofula, stone and gravel, secondary symptoms, tic douloureux, tumours, ulcers, venereal affections, worms of all kinds, and weakness, from whatever cause,"—furthermore, "Fits (again), deafness, hooping-cough, inflammation of the kidneys, bladder, or liver, flux, dropsy, gravel, and stone;" and lastly, "corns," which are "carefully extracted with instruments properly adapted for the purpose." So the Procurator-Fiscal contrived to lay hands on this pair of scamps, and caused them to be indicted, in name of the Sheriff of the county, "for having contrived a wicked and felonious scheme to cheat, impose upon, and defraud and swindle the lieges, by fraudulently obtaining from them money by means of false pretences, and in particular, by assuming a false name, title, or character, and by falsely pretending and representing themselves to be celebrated botanical physicians, or physicians from some pretended college, and properly qualified to prescribe and administer medicines for the cure of paralysis, etc. etc., thereby inducing the lieges to pay to them, or one or other of them, money for advice and treatment, and for furnishings of pretended drugs or medical liquids." The indictment then goes on to charge them more specifically with having, on certain dates, "imposed upon and deceived John Robertson" in Fisherrow, and so induced him to purchase, for "the sum of five shillings sterling or thereby," in order "to cure him of paralysis," "a quart or thereby of some deleterious or at least useless liquid, and a quantity of pills;"—likewise "Catherine Watson in Fisherrow," who got "a pint or thereby of some deleterious or at least useless liquid, to cure her of rheumatism, for the sum of two shillings and ninepence;"—in the next place, "Jean Leitch or Doig in Fisherrow," who, for the cure of her granddaughter, Jean Doig's fits, got "a small quantity of some deleterious or at least useless liquid," for which she "was charged the sum of sixpence sterling or thereby;"—next, "Robert Archibald, and Jane King, his wife, in Musselburgh," who were charged "the sum of five shillings and sixpence for a similar quart of stuff for curing a liver complaint, which they pretended that he, the said Robert Archibald, was labouring under;"—and lastly, "Marion Craig or Gibson, who, for one shilling, got a quart of some deleterious or at least useless liquid," which "by false and fraudulent pretences and representations," they "made her believe" was "calculated to cure her of toothache." In these several instances, they were charged with obtaining the said monies "fraudulently and on false pretences, and with cheating, cozening, and swindling" the several lieges. Their stock in trade too was produced against them, consisting apparently of ninety-seven handbills, a box with wax-cloth cover and strap, seventeen bottles containing liquid, and three boxes of pills.

On this indictment they were tried on the 17th May, before the Sheriff as judge, and a jury of fifteen members. They were defended by counsel; who could say no more for them than that their pretended drugs were not dangerous,—an alternative which the indictment expressly includes. And the result was that the jury unanimously found them guilty; and Christopher Curtis or Brooks, and George Campbell, were condemned by the judge to nine months of imprisonment with hard labour.

Chemicus. This is most satisfactory. These men are accused of taking a false title or designation,—of pretending to be qualified to prescribe medicines,—of prescribing pretended medicines,—and of raising money on these pretences; and a very important element of the charge is that, in order to sustain it, there is no absolute necessity that the pretended medicines shall be deleterious; it is sufficient that they be useless,—that is, for the purpose for which they are given. Now it is evident, that, under this plain common-sense view of law and justice, a great amount of provoking, injurious, and abominable quackery is within reach of the summary process of indictment before the sheriffs of counties; and it is to be fervently hoped that the Sheriff of Edinburgh will follow up promptly the blow he has struck so effectively. It is not enough to put down the miserable fry of petty empirics who prey upon small tradesmen and operatives, in the shape of botanical physicians, herbalists, and the like. These are great scoundrels, no doubt, and may do an infinity of mischief. But the injury they do to the community is light as a feather compared with the damage effected by the "Falsehood, fraud, wilful imposition, and swindling" of the aristocrats in quackery, who practise on the wealthier members of society in this country. The starving mountebank who circulates his handbill, or affixes it to gate-pillars and the posts of toll-bars, is a much less mischievous animal than the bloated empiric who expends his hundreds of pounds on newspaper advertisements, and on monster-placards in our streets and railway-stations. The latter, however, come quite as smoothly and inevitably under the definitions of the Sheriff's indictment as the former. There can be surely no insuperable legal technicalities to distinguish, from such "celebrated botanical physicians" as Christopher Curtis and George Campbell, the bigger and more thriving dealers in "Manhood," "Secret Infirmities," "The Silent Friend," "Morison's Pills," "Parr's Life Pills," "Holloway's Ointment," and fifty other greedy beasts of prey, who fatten upon the vitals of the wealthier dupes in the community. I trust that the success of their imposture is not to operate as a protection against the terrors of the law which are now opening up to their view. For it is plain that a public-spirited magistrate, who is not to be deterred by a little clamour on the part of a few deluded victims and hired advocates, has the whole race of these gentry within his grasp, great and small.

Editor. It is much to be deplored that the ignorance of the com-

munity, as to the relation of the members of the regular profession to quack practitioners, may prove a hindrance to the administration of justice on this occasion. The detestation in which we hold quackery is too apt to be put down by the ill-informed public, not to zeal for the purity of a noble profession, or sympathy with the sufferings and errors of humanity, but to the dictates of a direct, selfish, pecuniary interest. But the real truth is, that the great mass of the victims of quackery are not in ill-health at all, if they could only be persuaded to believe it. Many more have trifling habitual ailments which they themselves do not regard as of such consequence as to need interference on the part of the physician, and yet they must needs devour mountains of Life Pills for them. Others, who are seriously ill, would find that, in the hands of a medical practitioner, their cure might be accomplished in a third of the time, and for a tenth of the sum, which they do not hesitate to give to an empiric. And I verily believe, that if a statistical account of the matter could be obtained, it would turn out that, were pecuniary interest to be the motive, medical men ought rather to foster than decry quackery; and that more professional emolument is brought to them for repairing its damages, than is withdrawn from them in the first instance by its seductions.

Obstetricus. A time will come when all these things will be well understood. But in the meantime our true position is not seen through by the public. And therefore my opinion is, that medical men would exercise a wise discretion in not pressing the authorities too keenly in the present crisis. The movement has begun quite irrespective of all medical interference. And I have a strong persuasion that the judge and magistrate, with whom it originated, had sufficient knowledge and forethought to discern the path upon which the movement would lead him; and that he possesses sufficient public spirit and determination to follow it.

Chemicus. Will yonr indictment also reach the emperors of quackery, who pursue it under the ensigns of science?

Medicus. I don't feel so confident of that. How would it do to commence,—"Whereas fraud, falsehood, etc. Yet true it is, that A. B. did *truly* represent himself to be a doctor of medicine, from a celebrated college, and qualified to prescribe and administer medicines?"

Chemicus. But would you not come down upon him in the end with the charge, that "The said A. B. did, by fraud and falsehood, impose upon and deceive C. D., by falsely pretending that certain globules would cure her, the said C. D., of gripes, or some other ailment or ailments, and did administer to her one globule of useless material or materials, and did charge and receive from the said C. D. the sum of one sovereign and one shilling sterling, or thereby, well knowing that the same was good current coin of this realm, and that the said globule was a useless globule, and by no means worth the said sum of one sovereign and one shilling sterling or thereby?"

Medicus. I fear, however, you might encounter among the "Productions" libelled on in the indictment, as having been seized on his premises, "one secret drawer containing one paper packet of one pound or thereby of tartar-emetic, one sack containing one hundredweight or thereby of Epsom salt, one glass bottle containing one imperial pint or thereby of croton oil, one box with ten square yards or thereby of blistering plaster, and one shagreen case of four lancets or thereby;" and that the prisoner's counsel would justly argue that these "Auxiliaries" removed the case from the statute.

Chirurgus. Then let us make the most of what we have, and leave A. B. and his brotherhood alone, to taste what pleasure immunity can impart amidst the stings of conscience. But how the mouths of our English friends will water when they learn what Scotch law has given us!

Chemicus. A raid across the Border by our Sheriff, with the Scotch law of quackery at his back, would make welcome havoc in these days of fraternity.

Part Third.

CLINICAL REPORTS, LECTURES, ETC.

CLINICAL MEDICINE.—DR SKAE.

CLINICAL LECTURES ON INSANITY IN THE ROYAL EDINBURGH ASYLUM. BY DAVID SKAE, M.D., F.R.C.S.—(DELIVERED 7TH MAY 1853.)

Introductory Lecture.

Upwards of fifty years ago, when it was proposed to commence this asylum for the insane, one object anticipated by its founder, Dr Andrew Duncan, sen., and held forth among the other claims which it had upon the liberality of the public, was the opportunity which such an institution would afford to the medical profession for the *study* of insanity and its mode of treatment. Upwards of 3000 patients have enjoyed the advantages of the asylum since it was opened; and although in some respects it may be true that the medical profession has derived indirectly from it an increased knowledge of mental diseases and their treatment, yet in strictness I think we may affirm, that the anticipations of its founder in this respect begin only to be fully verified *this day* by the commencement of a course of clinical visits and lectures within the asylum.

For the institution of this course of lectures, and the opening up of the asylum for clinical observation and study, you are indebted, in a great measure, to the Royal College of Physicians, at the request of which body, influenced by the same views as those which animated their president fifty years ago, the managers of the asylum have agreed to the present arrangements, by which it is proposed to have two courses of lectures and clinical visits annually, each of three months' duration; one during the spring, and the other during the summer session.

It is not more than ten years since the first course of lectures on insanity was delivered in an asylum in this country. The first asylum opened for clinical instruction was St Luke's Hospital, London, where Dr Sutherland delivered a

course of lectures. And very soon afterwards Bethlem Hospital was made available for the same purpose. The fine asylum at Hanwell next afforded still further opportunities to the medical profession and students of the London Hospital schools for the study of insanity, and an eloquent and attractive series of lectures was delivered there annually by Dr Conolly, until lately the physician of that hospital. These three asylums are, as far as I know, the only institutions in this country which have hitherto permitted of clinical instruction within their walls. In France and Belgium the public asylums have for many years been open to students; and the lectures of Esquirol, Baillarger, Falret, Guislain, Morel, and many others, have long attracted students from every part of Europe. The importance of this department of study begins, however, to be more and more recognised by the examining boards of this country. This is apparent from the fact, that a course of lectures on insanity is now required by the Medical Examining Board of the Hon. East India Company, and attendance on such lectures is recommended by most of the other licensing boards. Indeed, the importance of the subject can scarcely be overrated. I do not speak of its importance to those who are to take charge of asylums, for to them it must necessarily form a special object of study. But I mean its importance to the physician in the routine of general practice. If it is considered requisite that the physician should be thoroughly acquainted with the natural history of phthisis, for example, in order that he may watch over the families entrusted to his care, and, in those whom he knows to be predisposed to this disease by a strong hereditary tendency, to guard them anxiously at particular ages, as at the development of puberty, and to adopt such precautions as to clothing, and diet, and changes of climate, etc., as may prevent his patients falling victims to this disastrous malady. If all this is considered a necessary part of your education in relation to this, and many other bodily diseases, is it not equally imperative that you should be acquainted with the predisposing causes, and incipient signs, and natural progress of the most melancholy of all the diseases we are heir to? There is perhaps no disease more certainly and uniformly hereditary than insanity; so much so, that special and distinct forms of insanity are transmitted through successive generations with remarkable constancy. The tendency to convulsions, to epilepsy, to idiocy, to puerperal mania, and to meloncholia, and indeed to all forms of mental disease, is transmitted with as much certainty through families as the predisposition to phthisis, gout, or any bodily disease. Nothing, I believe, is more certainly hereditary than the predisposition to excess in the use of stimulants. I have known many instances in which (in two or three successive generations) one or more members of the same family have terminated their career in fatal attacks of *delirium tremens*, brought on by this excess. But, perhaps, of all facts of this kind, the most striking is the constancy with which the suicidal tendency is propagated from father to son. It has been frequently observed that suicide has been committed at nearly the same age, and very often in the same manner, by father and son through several successive generations. Such predispositions can only be removed by early and careful training, by teaching and enforcing habits of self-control, by developing new tastes and resources, and directing the trains of thought, and ultimately the pursuits and business of life, into channels remote from those exciting causes which might develope the latent predisposition to disease.

There are many other aspects in which a knowledge of the early signs of mental disease, and its appropriate treatment, are of more importance to the general practitioner than they are to the physician of an asylum. The diagnosis of the particular form of insanity is of great importance. There are many cases of transient but acute hysterical excitement, which very closely resemble maniacal attacks. How distressing it must be for a young female to be hurried into a lunatic asylum, while labouring under such an attack, and to recover after a few days, to find her prospects for life perhaps destroyed, by the stamp of insanity having been irrevocably attached to her name.

The opposite error is one, however, of much more frequent occurrence. In many cases the development of insanity is preceded by a long period of incubation. An individual becomes altered in his natural disposition and habits; from being cheerful, gentle, and patient, he becomes sullen, irritable, and impatient, displays peculiarities of conduct which startle and alarm his friends, but yet too often fail to alarm them sufficiently; they cannot and will not believe that he is ill. How much might be done in many such cases by judicious advice and firmness, on the part of the medical adviser, to ward off the coming illness (for it is in practice this occurs, not in asylums). Change of scene, and of occupation, and other remedial means appropriate to the case, and adopted at the proper time, might save from insanity and from all the misery and ruined hopes which so often accompany it.

How much more frequently it happens that, in consequence of the prejudices of friends, cases of insanity are too long of being removed from home and placed in asylums; and how hopeless such cases become when the morbid fears, or delusions, or suspicions of the patients have been allowed to take a fixed hold on their minds by the ill-judged attempts of friends to reason them out of them, are facts now established by a long array of statistics; but these, too, are errors which can be corrected only by an enlightened acquaintance with the subject on the part of those to whom the friends look for advice in such cases,—namely, the general practitioners of medicine.

The experience of a large hospital such as this affords very frequent evidence of the little attention which has hitherto been paid to insanity by medical men *at large*, of how often they make mistakes as to cases, and how often they fall into grave errors in their treatment of the disease. It has occurred to me to see two cases of typhus fever sent into this asylum as insane; and not a few cases of acute delirium and hysterical mania, which ought never to have been sent here at all.

It is a well-ascertained fact, that in a vast majority of cases of insanity—perhaps in all—you cannot venture on general blood-letting to any extent without extreme risk of the case passing into one of imbecility or even terminating fatally. In not a few cases of acute mania brought here, from country districts in particular, I have found that the medical attendants, deceived probably by the violence of the symptoms, had bled largely, and in some instances repeatedly. One or two such cases died soon after admission, and others lapsed into hopeless fatuity.

There is a disease very familiar in asylums, known and easily recognised at a glance, very often even by the attendants, and yet almost entirely unknown in general practice—the general paralysis of the insane. Such cases almost never come under treatment in asylums until they have made considerable progress, and they are considered, I may say, in all asylums to be incurable. If they were recognised in their earliest stages by the medical attendant, who shall say that they could not, by prompt and judicious treatment, be cured.

Such are a few of the considerations which appear to me most strongly to illustrate the importance of this subject to the general practitioner. I might easily enlarge on this topic, and dwell upon the difficulties you would have to encounter in private practice, when first called upon to give advice amidst all the alarm and confusion incident to attacks of this disease; and I might draw a vivid picture of the delicate and trying position you would be placed in when called on to give evidence in a court of justice as to a case of insanity; but I forbear. I take your presence here to be a sufficient evidence that you think the subject deserving of your attention.

Let me describe to you briefly the plan of this course of lectures and clinical visits. It is intended, that for the present the number of pupils attending each class shall be limited to twelve,[1] and that there shall be three visits each week, and a clinical lecture on the Saturdays. I propose to have the clinical visits

[1] The class has since been extended to a larger number.

on Mondays and Wednesdays at three o'clock, and on Saturdays at two. The class shall be divided into three parties, each of which shall be accompanied, in going through the wards, by one of the medical officers of the asylum. By him the attention of the pupils will be directed to such cases, and to such features in each case, as are likely to afford illustrations for the subject under review at the weekly lecture. Immediately after the visits on Saturdays, which are to commence at two o'clock, shall have been finished, the lecture will be delivered in this room.

In making your visits round the wards of the asylum, I feel that, in addressing you, I need scarcely bespeak your sympathies towards the subject of your study, or fear, for one moment, that you may so far forget the scientific object you have in view, as to give license to the spirit of frolic or humour which may be tempted into exercise by the grotesque and singular persons and incidents you may encounter. Permit me, however, to offer you one or two hints on your conduct in the wards, which may be of use to you until experience gives you the *tactus eruditus*. In the first place, if you take your cue from your conductor, who has already acquired that tact, and knows the individuals with whom he is conversing, you can hardly be misled in your mode of addressing the patients. In the second place, always speak to them as you would to persons in the full possession of all their faculties—frankly, truthfully, and with that deference, in regard to their delusions or peculiarities, which you would display towards *differences of opinion* between you and any *sane* man. Although the insane perceive the absurdities involved in the delusions of each other, they have a firm persuasion of the truth of their own fancies. They are, however, generally aware that their opinions are *peculiar*, or *incredible*, or the grounds of their bearing accounted insane; and while you may therefore generally tell them, temperately and frankly, that they are mistaken, or that you cannot see things as they do, they resent indignantly any attempt to ridicule or scoff at their peculiarities.

For the same reasons, the insane do not consider themselves objects of *commiseration* or *pity*, and resent anything like an attempt to extend such feelings towards them. Their confidence, however, is easily won, in general by a frank and friendly sympathy with their situation and wishes. They are, at the same time, keen observers, and readily distinguish between what is spurious and affected, and what is genuine and spontaneous. They are mostly prone to be suspicious, and consequently the more alive to the least indication of insincerity. It is much better therefore, in your intercourse with the insane, to be silent than to offer a sympathy which you do not feel. They are keenly sensitive to ridicule, partiality, and any apparent injustice; and, on the other hand, remarkably grateful for kindness, and susceptible to its influence, so much so, that very often, even when under violent excitement, every kind look and act has been gratefully remembered and appreciated.

As a general rule, it will be better for you to put very few questions to the patients, beyond the ordinary interchange of civilities, until you become acquainted with them, but to listen to the conversation which passes between them and the medical officer accompanying you, and which will suffice, in general, to elucidate the peculiarities of each case.

And now, gentlemen, permit me to indulge myself by wondering what your feelings and reflections may be on first visiting this large institution. A large and well-regulated asylum appears to me to be one of the most wonderful and interesting sights, to a reflective mind, which this wonder-working age can exhibit. It is not yet forty years since a parliamentary committee of inquiry into the state of lunatics in England laid before the public such a picture of the mode in which insane patients were kept and treated, as makes one shudder to think of it. They were confined in jails, workhouses, and asylums. In the former, they were chained in the lowest dungeons, or made the butts and menials of the most debased criminals. In the latter, they were generally

confined in low and damp cellars, sometimes isolated in cages if violent, or chained to the floor or to the wall; if harmless, huddled together without regard to their habits, in cells not capable of containing properly a tithe of the numbers immured in them. In the best asylums of the metropolis, the medical treatment consisted of a general bleeding in the month of June annually, and four emetics during the year. The whip was systematically and regularly used. They were exhibited like wild beasts to the public for money, and their passions irritated to gratify a morbid and vulgar curiosity. They were often killed by the ignorance and brutality of their keepers, sometimes during rough methods of forcing meat into them, sometimes by barbarous and violent beating. Here is a picture of an asylum in 1815, drawn from the report of the committee by Dr Browne:—

"Let us pass a few minutes in an asylum as formerly regulated, and from the impression made by so brief a visit, let us judge of the effects which years or a lifetime spent amid such scenes was calculated to produce. The building was gloomy, placed in some low confined situation, without windows to the front, every chink barred and grated—a perfect gaol. As you enter, the creak of bolts and the clank of chains are scarcely distinguishable amid the wild chorus of shrieks and sobs which issue from every apartment. The passages are narrow, dark, damp, exhale a noxious effluvia, and are provided with a door at every two or three yards. Your conductor has the head and visage of a Charib; carries, fit accompaniment, a whip and a bunch of keys, and speaks in harsh monosyllables. The first common room you examine, measuring twelve feet long by seven wide, with a window which does not open, is perhaps for females. Ten of them, with no other covering than a rag round the waist, are chained to the wall, loathsome and hideous, but when addressed, evidently retaining some of the intelligence, and much of the feeling, which in other days ennobled their nature. In shame or sorrow, one of them perhaps utters a cry; a blow, which brings the blood from the temple, the tear from the eye, an additional chain, a gag, an indecent or contemptuous expression produces silence. And if you ask where these creatures sleep, you are led to a kennel, *eight feet* square, with an unglazed air-hole *eight inches* in diameter; in this you are told five women sleep. The floor is covered, the walls bedaubed, with filth and excrement; no bedding but wet decayed straw is allowed; and the stench is so unsupportable, that you turn away, and hasten from the scene. Each of the sombre colours of this picture is a fact. And those facts are but a fraction of the evils which have been brought home to asylums as they were."[1]

Let us turn from this picture and look upon such a one as that which you are now about to contemplate. You approach a large and cheerful building, situated upon a fine southern slope, surrounded by gardens and extensive pleasure-grounds, laid out with order and taste,—a magnificent amphitheatre of hills bounds the view in the distance. These lawns, and shrubs, and ornamental walks, and finely trained wall-trees—the green fields and kitchen garden—are all the work of patients, many of whom you may see labouring cheerfully, or walking in orderly bands about the grounds. You enter the building, and, instead of low damp cells and dungeons, you see cheerful and spacious galleries—large airy and orderly dormitories—beds covered with clean white sheets—sitting parlours, whose inmates are busily and cheerfully employed in needlework and knitting. Instead of shrieks and sobs, you may hear the cheerful and unrestrained laugh or song, and the sound of busy hands engaged in useful trades; instead of the lash and intimidating threat, the law of kindness appears to prevail, and officers and attendants are greeted with confidence, and looked up to as guardians and friends. You may look in vain for the filth, and bedaubed floors and walls, and the chairs and leg-locks of former days. The wildest and most ungovernable appear to enjoy unrestrained the impulses of their disease, or to succumb to the general influence of order, disci-

[1] Asylums as they were, are, and ought to be, p. 132-3. By Dr W. A. F. Browne.

pline, and example, and fall naturally into habits of self-control and industry. Such a community in some respects resembles a highly civilised people in this view, that individualities of character are lost sight of, and peculiarities of disposition are masked, and disappear under the general influence of polite habits, and a current and established code of manners and conduct. In your old bedlam kings crowned themselves with straw, and were surrounded by obedient courtiers and hangers-on. Your divinities were continually engaged in directing the clouds, giving rain and thunder and sunshine to the world. Your generals were issuing commands to their armies, and directing the evolutions of their troops—storming citadels and devastating empires. Bankers, millionaires, and politicians were last busily engaged in financial, speculative, and diplomatic arrangements, upon which the fates of millions were dependant Here, however, it is all changed. Kings, princes, and generals are divested of the insignia and trappings of their office,—and, like the kings and warriors of the opera, when they pass into the green-room, walk about like ordinary mortals. Under the omnipotent influence of order, discipline, and example, the fool forgets his cap-and-bells, the king his sceptre and his people, the potentate his supernatural powers, the warrior his soldiers, and the millionaire his wealth; and, stript of all the emblems and signs of their fancied eminence, with a singular inconsistency of character, work industriously at the most menial offices of the house, or pursue with interest and industry the most common-place craft or occupation. Two women who believe themselves to be our Divine Saviour, work hard at the washing-tub; and a third, who asserts she is the Almighty, peels potatoes and cleans dishes in the scullery. A venerable-looking, but hale and active, man, who claims to be Almighty God, and tells us that we are all in the flames of hell, mows the lawn all day with his scythe, and rolls it carefully for the game of cricket which is to take place. He watches the entire premises, and should an unfortunate being make his escape he will leap the fence and pursue him through the whole town and country, and bring him back to the asylum. A king, who believes he is made of solid gold, and that the marks of small-pox on his person have been produced by predatory incursions upon his royal skin, sits all day in the tailors' shop mending breeches and making flannel drawers. In an opposite room sits the commander-in-chief making shoes, and professing to believe that it is an excellent pastime. H. R. H. Prince George of Cambridge, after having driven triumphantly through Fife in a carriage-and-four, and horsewhipped a 'pike-keeper for offering to charge him with the toll, now tends the dirty, violent, and refractory inmates with the dignity, patience, and firmness of a stoic. Prince Charlie trundles stones in a wheel-barrow; and the lineal descendant of Ethelbert the I. of England, who hopes ere long to trample every crown in Europe in the dust, writes and prints his own poems, paints scenes, prepares dresses, and superintends rehearsals for the drama—forgetful alike of his descent and his destiny. "Thunderbolt," who is heir to several Scottish Dukedoms, and wields supernatural power, and who, in the persuasion of it, constructed a boat without a single iron bolt, in which he launched himself in the Forth during a storm, and was cast ashore after sailing many miles, pursues industriously his craft, and constructs fences for the gardener. The Pope, who, by the way, declaims against celibacy, and boasts of having thirty wives, forgetful of his high calling and solemn functions, works at the carpenters' bench. Empresses, queens, and princesses, are not ashamed here to make shirts and knit stockings, and bind shoes; and you will find Nebuchadnezzar, king of Babylon, no longer eating grass like an ox, or burning Jews in a fiery furnace, seated quietly on the end of a bench enjoying a pinch of snuff, and sewing with an industry and perseverance truly exemplary. Such are a few of the distinguished individuals who figure in the little world you are about to enter; but so changed from the dignitaries and potentates of the bedlam of former days, that you may look for them in vain as you walk round the wards, and imagine you are talking to very rational persons. Perhaps you may take

them for servants or officers of the institution. The mistake has often occurred. If you are assured that they are patients, you may wonder why they are detained here, unless some one, knowing the secret, may touch the string that jars, and then the apparent harmony will at once be succeeded by discord.

So much for system and discipline; but I should mislead you greatly if I led you to anticipate that within these walls you would see nothing but harmony, industry, and cheerfulness. You shall see mental derangement in every stage and form. In one part of the house you may find some of the inmates secluded in separate rooms, talking and vociferating, tearing their clothes and their bedding, wandering incessantly about their rooms, beating the walls, and fighting with some unseen and imaginary foe. Some will assail you with abusive epithets and obscene language. Others will talk loudly, garrulously, and incessantly. Others again will greet you with a continuous and quiet stream of incoherent babble. Not a few will claim your aid in delivering them from false imprisonment, and will endeavour to impress you with a sense of the injustice they suffer from retention in a madhouse. Perhaps in them the only indication of disorder you can trace is the ill-concealed anger, or malignity, or suspicion which lurks in the eye. Others you may meet, who will greet you with excessive politeness, tell you of their comforts and the kindness of every one, of their enormous wealth, their carriages, their titles and dignities, their splendid mansions, their projects for diffusing universal happiness, and converting the world into a paradise of luxury and love.

There are other groups which cannot fail at once to arrest your attention, and fix themselves on your memories,—figures standing in motionless attitudes, gazing intently on the vacant air—statues; others stooping, staring on the floor, unmindful of all around them. Some, again, you will find rolled up and crouched—sitting in corners or on benches, with their heads between their knees; and others wandering up and down, eying you with suspicion or disdainful glances.

Amid such an endless variety of morbid mental manifestations as will thus meet your eyes, all may appear to you at first confusion and disorder, and you may imagine it impossible to reduce such diversified phenomena into any intelligible classification. It is not so, however. They readily arrange themselves under a very few simple and distinct heads. The classification does admit, doubtless, of minute divisions and subdivisions, founded upon psychological and metaphysical distinctions. I shall not, however, burden your memories, or tax your attention with refined speculations in this course of lectures—which, as they must be brief, and as they are intended to be strictly clinical, must be, therefore, necessarily simple and practical.

In the vast majority of cases which you will be called upon to see in general practice (and it is with an especial reference to practice, and practical purposes, that I should wish to arrange my subject) there will be either an *undue and general excitement* of all the mental faculties, or a *partial* perturbation and undue exercise of one particular faculty or train of thought. The cases, therefore, at first sight, naturally arrange themselves under *two* great divisions,—(I.) *Mania*, or *general perturbation*, or *raving madness;* (II.) *Monomania*, or partial perturbation—one fixed persuasion of an irrational kind—one delusion—one fatal error—that pervades the mind, and poisons life, and destroys happiness or usefulness. This table indicates these two great divisions, along with the general outline of the classification I intend to adopt.

Classification for Clinical Lectures, 1853.

MANIA.

Acute. Chronic.	Affecting the Emotions and Passions only:—MORAL INSANITY.
Periodic. Remittent.	Affecting the Intellectual Faculties, and Emotions and Passions generally:—MANIA, or RAVING MADNESS.

Puerperal Mania.

Delirium Tremens (Mania a Potu:—Brain Fever).

MONOMANIA.

Melancholia.	Affecting the Desires, Emotions, and Passions only, without delusion:—MORAL INSANITY.
Suicidal Mania.	
Homicidal Mania.	
Kleptomania.	
Pyromania.	
Dipsomania.	
Satyriasis.	Affecting the Intellect or Understanding, accompanied with Delusions, Hallucinations, etc.—INTELLECTUAL INSANITY.
Nymphomania, etc.	
Monomania of Pride.	
... ... Fear.	
... ... Suspicion.	
... ... Unseen Agency.	
Demonomania, etc.	

DEMENTIA, or Incoherence.

1st Stage:—Forgetfulness—Loss of Memory—Senile Dementia.
2d ... Irrationality—Loss of Reason—Marked Incoherence.
3d ... Incomprehension—Instinctive Stage—Fatuous.
4th ... Inappetency—Loss of Instinctive Action—Total Fatuity.

GENERAL PARALYSIS, with INSANITY.

EPILEPSY, with INSANITY.

IDIOTCY (Idiotism).

Imbecility.
Idiotcy.

Cretenism.

Under the 1st class, *Mania*, you have all those cases where there is a general perturbation and excitement of the mental faculties. There is a loss of all control over the operations of the mind; the thoughts are, therefore, incoherent,—that is, they succeed each other without any regulation on the part of the individual himself; and they are carried on with inordinate activity. Objects of imagination and perception are mistaken for realities. Ideal forms and terrors have all the reality of dreams. The loss of self-control extends itself from the succession of ideas to the voluntary acts, which become instinctive, incontrollable, restless, incessant, or violent. This excitement may be either *acute*, and of comparatively short duration, or it may become *chronic* and protracted; or lastly, it may occur at regular or irregular periods, with intervals of sanity—constituting *periodic* or *remittent* insanity.

In some cases, along with inordinate *activity* of *thought* and *excitement* of the *feelings* and *passions*, there appears to be no *delirium*, no delusions, no perversion of the understanding,—these come properly under the denomination of *moral insanity*.

II. The second great division includes those cases where the derangement is partial—where the individual reasons, and observes and feels correctly in relation to some objects, or in certain trains of thought, but erroneously in regard to others. This derangement may affect the understanding or intellect—or it may affect the emotions and passions alone. In the former case you have delusions or hallucinations,—the individual fancies he is a king; that he is persecuted by imaginary foes; that he is about to be poisoned or executed; that he is tortured by some unseen demon, or possessed, and, according to the supposed organ or faculty of the mind, believed to be diseased in its operations. In such cases the malady receives the distinctive title of monomania of pride—fear—suspicion—unseen agency—avarice, and so forth. Such delusions or hallucinations may act as motives, and lead to melancholy, to suicide, to

revengeful acts of homicide. But there are, it is now well known, many cases of mental disease where there are no such delusions, but a morbid excitement of the passions and emotions only. You may thus have deep melancholy, without any cause real, imaginary, or assigned—a simple abstract, but overpowering melancholy. You may have a strong loathing at life, and craving for death,—a strong, and sometimes uncontrollable, impulse to destroy life in others, without any reason, or any imagined wrong received; an impulse to burn or destroy property, to steal, or to indulge in inordinate drinking. In such cases the intellect is often unimpaired, and the reason revolts against the morbid impulse, and struggles to subdue it. These constitute examples of *moral insanity* again, in which one emotion or passion only is in a state of morbid excitement, and are named, according to the particular passion or emotion affected, satyriasis, melancholia, suicidal or homicidal mania, *pyromania*, kleptomania, dipsomania or oinomania, etc. etc.

III. The third great division comprises all those cases where the mental faculties are impaired, and where mental operations are carried on with diminished activity, or the faculties are nearly or altogether obliterated in their exercise. The general term dementia, or incoherence, is applied to all such cases where they are either primary affections or, as more frequently happens, the sequelæ of other diseases, or other forms of mental derangement. *Dementia* presents a great number of phases, but they are differences in degrees only, beginning with the smallest appreciable imbecility or weakness, and terminating with complete fatuity or loss of all mental power, and total inappetency.

Dementia is the natural termination of insanity. *General paralysis* and *epilepsy* (the fourth and fifth divisions which I have adopted) are two of its most important complications, and present a most interesting field of study.

The last division comprises cases of congenital weakness of mind of every degree, from partial imbecility down to absolute and complete idiocy. This subject has of late years become one of great interest, from the philanthropic and laudable experiments which have been made for the education, and even for the cure of idiocy. In France, and more recently in England, establishments have been instituted for training idiots; and it is deeply interesting to know that, by a systematic method of physical and mental training, the helpless motionless, dumb, and insensate idiot has been raised to the enjoyments of a rational and voluntary being. In some mountainous countries you are aware that idiotism prevails as an endemic disease, and is called Cretinism. In Switzerland in particular it has long been familiar by its prevalence. There Dr Guggenbühl, an enthusiastic and enlightened philanthropist, has established an institution for the *cure* of this disease, and, receiving infants at a very early age, succeeds, in a considerable number of cases, in curing this apparently most hopeless, and certainly most deplorable and loathsome, of human maladies.

Such, gentlemen, is the arrangement which I propose to adopt, and such, in brief outline, the character of the diseases we are about to examine. When we have completed the inquiry, we shall turn to the consideration of those special means and appliances which a lunatic asylum offers for their amelioration or cure. The effects of isolation, of removal from the exciting causes of disease, of the influence of the discipline, and of the amusements and occupations which an asylum affords. In this survey we shall have to consider the best construction and arrangement for an asylum; the mode of classifying the patients; the various methods of employing them; the sources of instruction, exercise, and amusement for them; the duties and qualifications of attendants; the mode of managing such an establishment; the responsibilities, powers, qualifications, and duties of its officers and governors.

"A perfect asylum," says Dr Browne, the accomplished and *well-known* physician of the Crichton Asylum, near Dumfries, "A perfect asylum may appear to be a Utopia—'a thing to dream of, not to see.'" Nevertheless he has lived to see, both under his own management and under that of many of

the most enlightened *Alienistes* (excuse the expression—I prefer it to Mad-doctor), both of this country and America, a nearer approach to that Utopia than he dreamt of seeing when those words were uttered (1837). Such a Utopia we think we see when we find in a large and orderly institution an air of industry and contentment; every one who can work working cheerfully and busily; shoemakers, tailors, and sempstresses furnishing clothing to the inmates; blacksmiths, builders, carpenters, upholsterers, tinsmiths, plumbers, plasterers, and painters, actively engaged in repairs, or new designs and additions for the extension, comfort, or adornment of the house; others, ambitious of a more honourable calling, pursuing the lighter and more graceful occupations of music, sketching, or literary composition; gardeners tastefully tending the flowers or fruit trees, or raising vegetables for the kitchen. These hours of occupation, agreeably interspersed with reading, for which an extensive and well-selected library, and a liberal supply of periodicals, afford ample means. This more grave occupation, relieved at intervals by music, and dancing, and singing; by games at cricket, or quoits, or billiards, cards, chess, or backgammon. While at stated or occasional intervals, the inmates assemble to witness some exhibition —some histrionic efforts on the part of the stage-struck inmates, or to listen to a concert or lecture; or, at other times, issue forth on some piscatorial expedition; or wandering to some glen in the neighbouring hills, where they can indulge in unrestrained exercise and mirth. When we see all this I say, and consider the character and condition of the persons who perform in this drama, we have before us a picture which to me appears such a Utopia as half a century ago could not even have been dreamt of, far less seen.

Part Fifth.

MEDICAL NEWS.

EDINBURGH MEDICO-CHIRURGICAL SOCIETY.

MEETING VI.—*Wednesday, 16th February* 1853.—Dr SIMPSON, President, in the Chair.

1. *Dr Simpson* exhibited a preparation illustrating the spontaneous dislocation of the heads of both femurs.

Professor Syme had seen such cases; they were in general apt to be at first overlooked, the lesion not being noticed till the child begins to walk.

2. *Mr Spence* showed a mamma affected with cancer. The preparation was mainly interesting from the unusual situation in which suppuration had occurred, namely, on the deep surface, near the pectoral muscle.

3. *Mr Struthers'* communication, entitled "Considerations on local blood-letting in affections of the internal viscera," was read in his absence by the secretary. The object of the paper was to show, that as the circulation of the abdominal and thoracic walls formed a complete and independent system, only communicating with that of the internal organs by the great systemic trunks, or by the centre of the circulation, the ordinary means of local blood-letting could not produce any really deviant effect on the vessels of the lungs, liver, kidneys, etc. In the case of the brain, Mr Struthers admitted, that such an effect was possible, and also in the case of the portal system by means of the hemorrhoidal veins.

Dr Bennett agreed with the author in many of his remarks, but not in all. Local blood-letting often does produce greater effect than a general abstraction of blood. In some cases he felt inclined to attribute the good effect which followed the application of leeches, not so much to the loss of blood as to the continued warm fomentation which was so uniformly employed after their removal. In severe headaches, experience had shown that the application of warmth was successful after the use of cold bath, as water and ice had failed; the reason of this he was as yet unable to explain.

Mr Miller remarked, that very probably the headache, when dependent on vascular excitement, would be relieved by warm applications; and when on nervous impressions, by cold; in this he thought the solution of the varied experience of Dr Bennett lay.

Professor Syme said, that in painful affections of the joints, though both hot and cold applications were employed, yet it was only from the latter that permanent relief was obtained.

Dr Lowe remembered, some years ago, to have seen warm and cold applications to the head in the delirium of fever used at the same time in the practice of different physicians. While both seemed to afford relief, he thought the experience of the warm applications was the most favourable.

Dr W. T. Gairdner argued, that it was not surprising that hot and cold applications should have a similarly beneficial effect in various painful affections, inasmuch as the effect of both of them upon the healthy nervous system was, locally, to diminish the sensibility to external impressions in the parts to which they are applied; and, generally, to produce a sedative effect when suddenly applied to a large surface. After several illustrations of this principle, Dr Gairdner expressed his general concurrence with the views of Mr Struthers, which he thought to be quite consistent with the ordinary experience and practice of physicians, who, not perhaps in theory, but in fact, were in the habit of looking for relief from local blood-letting, only in the description of cases to which Mr Struthers referred.

A very desultory discussion then followed, including further illustrations of the application of heat and cold in disease, by Dr Andrew Wood, Dr Alexander Wood, Dr Simpson, Dr Husband, and Dr W. T. Gairdner.

MEETING VII.—*Wednesday, 2d March* 1853.—Dr SIMPSON, President, in the Chair.

Dr Andrew Wood, at the request of the Rev. Mr Nisbet, exhibited an apparatus to prevent the inhalation of dusty particles in the case of masons and other operatives. The apparatus consisted of a skeleton of leather, to which was attached a plate of bone, perforated either by holes or formed as a comb, this again being covered externally by a portion of merino cloth; both nose and mouth were thus protected. One great advantage was the cheapness of the respirator,—not more than from 1s. to 1s. 9d. being required for its preparation.

Dr W. T. Gairdner thought the nose-piece in the apparatus exhibited imperfect; and the integrity of it was of greater consequence than that of the mouth-piece.

Professor Miller adverted to the extreme difficulty of inducing artisans to wear such apparatus, even though convinced of their utility.

Dr Dumbreck and other members, and the Rev. Mr Nisbet, spoke on this subject.

Dr James Sidey exhibited a preparation showing the presence of six well-developed teeth immediately under the mucous membrane, in a fœtus born at the eighth month.

Dr Husband read a paper "On the Method of Preserving Vaccine Lymph in Capillary Tubes." In proof of the efficiency of this method, Dr Husband stated, as the result of a large number of vaccinations performed by him, that

in vaccinating with lymph which had been preserved in tubes for periods varying from six months to nearly three years, he had found the chance of success to be to that of failure as 9 to 1. Additional and very extended observation enabled Dr Husband to confirm his former observations on the same subject.

Professor Syme read a communication "On the Improvements which have been introduced into the Practice of Surgery in Great Britain within the last Thirty Years." The object of the paper was to contrast the method of treatment presently adopted with that previously enforced in certain cases in which the points of advantage, which distinguish the existing practice, had been well ascertained, and would be readily admitted. Attention was directed particularly to the following affections and their respective means of cure:—Ulcers, indolent or callous, varicose and syphilitic; the disease of the shin bone; simple incised wounds; gangrena senilis; aneurism; loose cartilaginous bodies in the joints, especially that of the knee; deformities proceeding from a contracted state of the muscles, particularly wry neck and club foot; morbid growths affecting the upper and lower jaws; tumours of the nasal cavity; enlargement of the tonsils; hydrocele; fungus of the testicle; stricture of the urethra; diseases of the rectum, etc. etc. etc. The revolution which has taken place, in regard to the performance of amputation, and the relative advantages of the flap and circular operations were canvassed. Particular amputations, as at the ankle-joint and the excision of joints, were considered. Other operations, new or modified, the withdrawal of some, and re-introduction of others, were discussed. The paper concluded with the expression of the belief, on the part of the author, that during the last thirty years so many useful improvements had been introduced into the practice of surgery, that the practitioners of the present time have no reason to fear its comparison with any other period of similar extent in surgical history.

Professor Miller said that he could not agree with all that had been stated in Mr Syme's paper, the estimation of what were and what were not improvements in surgical practice, during the period referred to, being very much a question of individual opinion. At that late hour he would not detain the Society, but proposed to bring before it, at its next meeting, a communication on the same subject.

After some remarks from other individuals, the Society adjourned.

MEETING VIII.—*Wednesday, 6th April* 1853.—Dr SIMPSON, President, in the Chair.

Dr J. Warburton Begbie exhibited a specimen of highly coagulable urine, occurring in the course of small-pox. The appearance of the eruption had been preceded by hæmaturia to a marked degree; and after the blood disappeared, the urine continued slightly coagulable till the eighth day of the eruption, when the albumen increased greatly in quantity, and then again diminished, being, at the end of third week, scarcely perceptible.

Professor Miller read a paper entitled "Observations on the Recent Progress of Surgery." The object of the paper was somewhat similar to that of Mr Syme read at last meeting; but the author, besides tracing the progress of operative surgery, dwelt at considerable length on the employment of internal remedies in surgical practice, and insisted on the necessity of drawing closer the alliance between surgery and medicine. The paper scarcely admits of analysis; but the subjects of stricture of the urethra, urinary calculi, aneurism and its treatment by compression, varicocele, etc., were among those which occupied the attention of the author, and which furnished the materials for a protracted discussion after the reading of his paper. The following members took part in the discussion:—Dr Dunsmure, Mr Spence, Dr Mackenzie, Dr Andrew Wood, Dr W. T. Gairdner, Dr Simpson, and Mr Miller.

The more important opinions expressed, and facts stated, were as follows:—

Stricture of Urethra.

Dr Dunsmure was less sanguine than Mr Miller as to the results of medical treatment in this disease. He could not look upon it as at all probable that surgical manipulations could ever be dispensed with in a considerable proportion of cases.

Dr Andrew Wood thought that the greatest improvement to be looked for in regard to urethral stricture, was the discovery of means to prevent its occurrence, by restraining and removing one of its most frequent exciting causes—gonorrhœa.

Lithotrity.

Mr Miller maintained that this operation was likely to prove a most efficient substitute for lithotomy, in very many cases where the latter is still chiefly performed; and that this was especially the case where the stone was small.

Dr Dunsmure took a different view of the applicability of lithotrity to such cases, as he thought the improvements that had been introduced in the performance of lithotomy, and particularly the use of chloroform (which enabled the internal incision to be much more precisely made), had removed many of its dangers, and correspondingly limited the field for lithotrity. This was particularly the case when the stone was small, the most favourable circumstance for either operation.

Extra-Peritoneal Section in Strangulated Hernia.

Mr Spence claimed the credit of originating this operation for Petit in France and the second Monro in this country. Mr Miller assented.

Treatment of Varicocele by Pressure.

Mr Spence said that Mr Colles of Dublin was the originator of the principle of the vein truss. This was likewise assented to by Mr Miller.

Treatment of Aneurism in the Extremities by Compression.

Mr Miller strongly maintained the advantages of this operation over the ligature in a large proportion of cases. He considered it well established, that in cases suitable for either operation, the ligature was both less safe and less certain in its results, while it was attended by more suffering. He considered that a grave responsibility was incurred by any surgeon who persisted in employing the ligature in cases suitable for compression.

Mr Spence considered that compression had been fairly tried by Boyer, Deschamps, and other surgeons, before the practice now so prevalent in the Irish school was introduced. It had failed in the hands of these surgeons, and he did not see that there was any improvement in the character of the proceeding, or the mechanism now employed, which warranted the hope of much greater success. As compared with ligature of the artery, scientifically and well performed, he considered the results of compression in aneurism by no means satisfactory. Compression of the artery could not well be accomplished without involving the vein in the pressure, and sloughing, with other bad consequences, had already resulted from this. The uncertainty of the operation, the protraction of the cure, where it occurred (in one case twenty-one months), and the dangerous consequences which had been shown to result from the operation in some instances, would induce him (Mr Spence) to prefer the ligature in most cases. He would not attempt compression in the first instance, as he believed that this operation, if unsuccessful, would in many cases render the application of the ligature afterwards difficult and unsafe.

Dr Mackenzie said that the subject of the paper before the Society included much too large a field for one night's discussion. He had not been present at the reading of Mr Syme's paper, but he had since read it in the Monthly Journal; and the paper brought before the Society this evening appeared to him (with the exception of some mention of Mr Fergusson's operation of sta-

phyloraphy, the modified mercurial treatment of syphilis, and one or two subjects of less importance) to be simply a repetition of the surgical improvements to which Mr Syme in his paper had drawn attention.

Some remarks of Mr Miller on one subject, however,—viz., the treatment of aneurism by compression, ought not, he thought, to be allowed to pass unnoticed. Mr Miller, after dwelling on the dangers of treating popliteal aneurism by ligature of the femoral artery, and the certainty and ease with which the disease could be cured by compression, had wound up his remarks on the subject, by saying that the surgeon who now ventured to place a ligature on the femoral artery, for the cure of popliteal aneurism, was "guilty of a serious moral responsibility." Dr Mackenzie was no enemy to the treatment by compression; on the contrary, he had advocated the plan of treatment in his lectures for some years past. He had obtained from Dublin the instruments used there, and had explained to his class the mode in which they were to be employed, and the cases in which it appeared to him the treatment would be especially applicable. He had attempted, in one case of popliteal aneurism, to follow out the treatment, but the patient would not submit to it, and the artery was ligatured. He thought much was due to Mr Bellingham, and the Dublin surgeons, for reviving the treatment by compression, for showing the mode in which moderate compression cured the disease, and for elucidating the *modus operandi* of both kinds of treatment. At the same time, he regretted to hear the benefits of compression, and the danger of the ligature, spoken of in such extravagant terms, as had been done by Mr Miller. Such untoward advocacy of any plan of treatment, he thought, must only tend to retard the progress of surgery in the treatment of aneurism. He wished to know on what experience Mr Miller grounded such strong statements. He (Dr Mackenzie) could speak as to the result of a considerable number of cases. In the last sixteen years he had seen a ligature applied to the femoral artery in twenty-five cases. In some of these he had performed the operation himself, and in the rest he had assisted in the performance of the operation, and was cognisant of the results. In one only had the operation been productive of a fatal result, and this from secondary hemorrhage. Compared with this, the results of the treatment by compression in Scotland were hitherto unsatisfactory. He was acquainted with the facts only of four cases, in which the practice had been attempted in Scotland. The first case he had already mentioned. The patient was irritable, and would not suffer even moderate compression for five minutes at a time,—the artery was tied successfully. The remaining three cases had occurred in Mr Miller's own practice; and, while he drew the attention of the Society to the facts of these cases, he begged Mr Miller would correct him if he were in error. In the first case, treated in hospital, the treatment by compression was persevered in for a fortnight, during which time the patient suffered much and slept little. At the end of this time, the tumour had doubled its size, a high degree of constitutional disturbance was present, and the treatment had to be abandoned. The artery was then ligatured, when the patient was in a most unfavourable condition for the performance of the operation; the disease was cured, and the patient made a good recovery. In the second case (popliteal aneurism), the compression caused gangrene of the limb; amputation of the thigh was performed, and death was the result. The third case was one of aneurism of the axillary artery beneath the clavicle; compression of the subclavian artery by an apparatus was employed; the aneurism continued to increase in size; the treatment was abandoned; the aneurism allowed to burst, and the patient to die, without a ligature being placed on the artery. With such results, he wished to know what reason Mr Miller had to make the statement, that the surgeon who now resorted to the use of the ligature was guilty of a "serious moral responsibility." Dr M. then read an extract of a letter from a young surgeon in Dublin (an advocate for the treatment by compression), giving an account of seven cases of aneurism, which he had seen treated in that city. Of these, six were aneurisms of the popliteal artery, and one a

traumatic aneurism of the brachial artery. Of the six popliteal aneurisms, four were cured by compression, whilst in the two remaining cases, amputation was performed, in one of which the treatment by compression had failed. The case of brachial aneurism was also cured by compression. In one of the cases, the patient had formerly been treated by ligature for popliteal aneurism of the other limb, and stated, as the result of his experience, that he would "much rather undergo the operation again than the compression." "Usually, but not always, the patients had required large anodynes," whilst undergoing the treatment by compression. In his advocacy of the new method, Mr Miller had urged that that treatment was the best which cured the disease "*tuto, cito, et jucunde.*" He (Dr M.) had said something of the safety. Experience hitherto had shown, that the length of time required (on an average) to cure the disease by compression was decidedly greater than by ligature. As to the comfort of the patient, he could not imagine how the two modes of treatment could bear comparison. In compression, the patient had, to say the least of it, an irksome ordeal to go through. In the treatment by ligature, the pain of the operation was annulled by chloroform; and he thought he exaggerated little, if at all, when he said that the patient had not a moment's pain from beginning to end. The question, however, depended on the safety; and he had no doubt, that if the treatment by compression was ultimately proved to be safer than that by ligature, it would justly come into general practice.

Mr Miller replied, and repeated the declaration of his belief, that the statistics of compression, as compared with those of the ligature, demonstrated undeniably the superiority of the former. The case to which Mr Spence had alluded as occupying twenty-one months in the cure, was not an average case, and its parallel might be found among those treated by ligature. The average length of treatment was nearly the same in the two operations; indeed, the cure by pressure was rather more rapid, and it certainly was far more safe, and at least equally permanent. He admitted that his own personal experience had not as yet been of a satisfactory character; but hoped in future, with greater opportunities than at present, to be able to show better results. Meantime, Mr M. stated his experience was not altogether barren of conclusions in favour of compression, for one of his cases illustrated the ease and satisfaction with which the ligature might be applied after compression; while the other two were cases in which, although compression undoubtedly failed, the ligature would have had no chance of success, and indeed would not have been attempted by him.

THE POWERS OF THE PROCURATOR-FISCAL WITH REGARD TO THE MEDICAL PROFESSION IN SCOTLAND.

The prosecution of crimes in Scotland is carried on,—not as in England, really or nominally by the individuals who have sustained injury,—but at the public expense, by a staff of officials—of whom the superiors have their head-quarters in the capital, and the subordinates are ramified into every part of the country. Each county has its "Procurator-Fiscal," whose business is to trace out reported transgressions, and place alleged culprits within reach of the law. In the discharge of this duty he frequently requires the assistance of medical information,—and generally obtains it from some member of the profession whom he is led to select by private friendship. So far there seems nothing objectionable, and much to admire, in the existing arrangement. But as the subject of investigation is often under treatment before the authorities take cognisance of it, great inconvenience is apt to result from the displacement, to which the original attendant is thus rendered liable. The feelings of patients and friends are hurt by the intrusion of a stranger armed with the sheriff's warrant; professional jealousy is excited or increased; inferior persons are led to ingratiate themselves with the man in power, and make work for themselves by aggravating the aspect of cases under consideration,—while, worst of

all, the ends of justice are thwarted by ignoring the primary evidence. It is, therefore, not surprising that the most bitter heart-burnings have sprung from this source,—or that numerous complaints on the subject have been addressed to the authorities. Neither is it surprising that a single-handed practitioner has found it in vain to contend with a Lord Advocate or the Secretary of State for the Home Department. But the College of Surgeons lately appointed a committee to communicate with the authorities, in order to get the rights of regularly qualified members of the profession protected from unnecessary interference; and as the College of Physicians have followed this good example, there now seems to be a reasonable prospect of relief. In the meantime the following letters will afford an illustration of the system at present pursued:—

(*Mr Syme to Maurice Lothian, Esq., Procurator-Fiscal for the County of Midlothian.*)

2, Rutland Street, April 2, 1853.

Sir,—Understanding that Mr George Glover has given a certificate as to the state of Patrick Clark, at present under my care in the Royal Infirmary, I beg to be informed if this was done by your authority, or with your sanction.—I am, Sir, your obedient servant,

James Syme.

Maurice Lothian, Esq.

(*Mr Lothian to Mr Syme.*)

County Buildings, Edinburgh, 4th April 1853.

Sir,—I have to-day received your letter of Saturday's date. Dr Glover gave to me the certificate to which you refer, in the discharge of his duty as Police Surgeon.

Before answering your question, whether he did so by my authority or with my sanction, will you be so obliging as explain to me *why* you put that question, and *why* you expect me to answer it?—I am, Sir, your obedient servant,

Maurice Lothian.

James Syme, Esq., 2, Rutland Street.

(*Mr Syme to Mr Lothian.*)

Rutland Street, 5th April 1853.

Sir,—As your letter contains all the information I desire, it is unnecessary to answer the questions which you propose.—I am, Sir, your obedient servant,

James Syme.

Maurice Lothian, Esq.

(*Mr Syme to the Lord Advocate of Scotland.*)

Edinburgh, April 5, 1853.

My Lord,—With reference to the subject which, as one of a Deputation from the Royal College of Surgeons I lately had the honour of submitting to your Lordship, I beg to state, that a few days ago Mr George Glover gave a certificate, on soul and conscience, relative to the condition of a patient at present under my charge in the Royal Infirmary. As this patient had suffered a compound fracture of the leg, and as Mr Glover had no opportunity of inspecting the limb, it is plain that this certificate did not rest upon his personal knowledge.

In these circumstances I leave your Lordship to determine whether or no the Procurator-Fiscal was guided solely by a desire to attain the ends of justice in preferring so questionable a document to the authentic information which he might have procured from myself or my clerk, Dr Dobie, who is resident in

the Hospital.—I have the honour to be, My Lord, your Lordship's most obedient servant,

JAMES SYME.

The Right Honourable Lord Advocate of Scotland.

(*Mr Syme to the Commissioners of Police.*)

Edinburgh, 9th April 1853.

GENTLEMEN,—There is at present a boy under my charge in the Royal Infirmary, on account of a very severe and complicated injury of the leg. A few days ago the Surgeon of Police, Mr Glover, went to the ward where this patient lies, and attempted to turn down the bed-clothes in order to examine the limb, but was prevented from doing so. He then gave a certificate, "on soul and conscience," describing the particulars of an injury which he had not seen, and expressing an opinion as to what he had no means of knowing any thing. How far the Procurator-Fiscal, Mr Lothian, was justified in preferring a document so questionable to the authentic information which might have been procured from myself or the House Surgeon, is a matter that belongs to the Lord Advocate,—and to his Lordship has accordingly been submitted. But being informed by Mr Lothian that, in giving the certificate, Mr Glover acted in the discharge of his duty as Police Surgeon, I beg to ascertain from the Commissioners—who of course determine the duties of their officers—if they really have authorised a proceeding which I may safely say has no precedent in any hospital within her Majesty's dominions.—I have the honour to be, Gentlemen, your obedient servant,

JAMES SYME.

The Commissioners of Police.

(*Copy of Mr Glover's Certificate.*)

Edinburgh, 21st March 1853.

I hereby certify, on soul and conscience, that I this day examined Patrick Clark, aged about eleven years, lying at the Royal Infirmary, and find he has a large lacerated wound of the soft parts on the outer side of the left leg, and a fracture of the bone a little above the ankle; that his life is in danger, and that he is in a fit state to emit a declaration.

(Signed) GEO. GLOVER, *Surgeon.*

THE TREATMENT OF STRICTURE BY EXTERNAL INCISION.

(*Mr Syme to Mr Guthrie.*)

Edinburgh, 12th May 1853.

DEAR SIR,—It appears that the operation which I performed on your old patient, Captain ——, has not proved so fruitless as it is represented to have been in your publication on the "Successful Treatment of Strictures." Before the stricture was divided by external incision, his friends regarded him as a confirmed invalid; and he led the life of a recluse on account of the inconveniences attending his urinary complaint. But since the operation was performed he has been restored to health and vigour, has travelled extensively in Norway, and has now become established as a landed proprietor in Canada. I regret that you did not apply to myself instead of the patient for the information you desired as to the effect of the operation,—since he wrote what you have printed under the influence of an unpleasant feeling between us which had resulted from his obstinately refusing to let me pass any instrument after the catheter was withdrawn—and thus exposing not only himself, but the character of the operation, to no small risk. Having come to regard the matter more justly, and probably compared his present comfortable condition with the sad fate of his two brothers—both of whom, you are aware, died of stricture at

an early period of life—or his own protracted sufferings and narrow escapes; especially in Paris, when the Homœopathist went on prescribing his globules without being at all aware of the extensive urinary extravasation, which would certainly have proved fatal but for the timely incisions of M. Cloquet—he now says, in a letter to a friend, dated Canada, 10th April 1853: "I am, and always will be, sorry that I answered Guthrie's letter, and am much annoyed at the use he has made of it." As you have said that this case is the one of all that you have ever known which you would have selected for a trial of the real value of my operation, I trust you will pardon me for sending you this information, and giving it such a degree of publicity as may, in some measure, counteract the erroneous impression that may have been produced by your account of the result.—I am, dear Sir, yours very truly,

JAMES SYME.

G. J. Guthrie, Esq.

(*Mr Guthrie to Mr Syme.*)

4, Berkeley Street, May 14, 1853.

MY DEAR SIR,—I have to acknowledge your letter of the 12th. I do regret very much I had not the pleasure of seeing you when you visited London lately, as I should have been glad to have had the opportunity of talking over many points of surgical difference which have arisen of late.

With respect to Captain ——, I merely published his own written statement to me, some months after the operation was performed by you. I added nor subtracted nothing. If Captain —— should now write you another and a different one, it will be satisfactory to you, and I shall be glad to see it; but it must be from himself, and not from hearsay of another person, as to his state, upon which I should place no reliance whatever.

I am of opinion that your operation will often effect a cure—that mine will do the same—that both are good when properly applied,—and that they will do more than any other mode of treatment.

I venture to think that enough has been done to bring the subject-matter before the profession, and sufficiently so to make every man think and act according to the best of his judgment. Such is also the feeling of the principal people in London, and all are desirous that controversial writing on this point should cease for the present, if it can be so managed.

I think you must have seen an indisposition on the part of the London surgeons to continue it when you were here, and it has been from the best motives.—I am, my dear Sir, very truly and respectfully yours,

G. J. GUTHRIE.

J. Syme, Esq.

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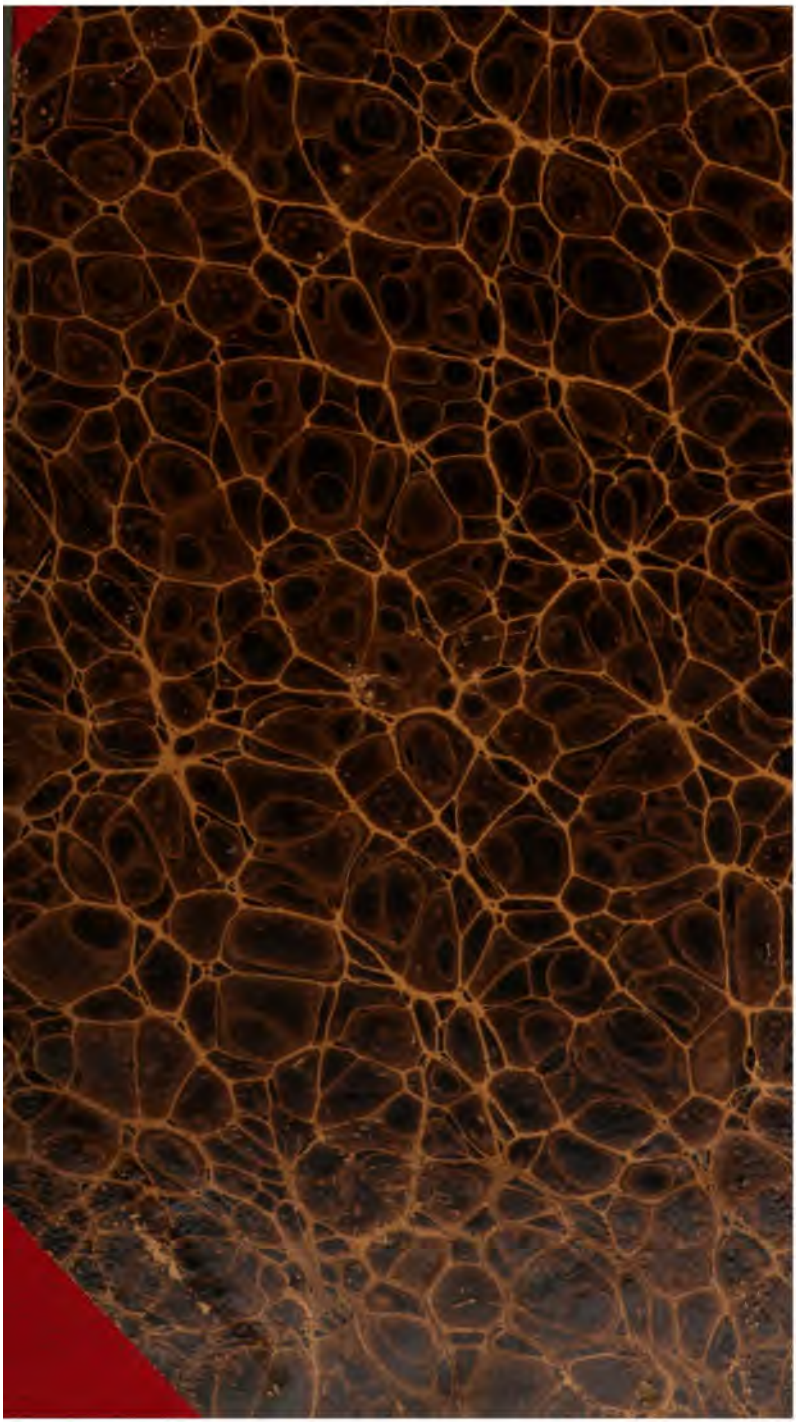
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